







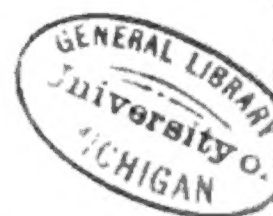
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THE  
LONDON, EDINBURGH, AND DUBLIN  
**PHILOSOPHICAL MAGAZINE**  
AND  
**JOURNAL OF SCIENCE.**

CONDUCTED BY

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---

“Nec araneorum sane textus ideo melior quia ex se fila gignunt, nec noster  
vilior quia ex alienis libamus ut apes.” JUST. LIPS. *Polit. lib. i. cap. 1. Not.*

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VOL. XXX.

NEW AND UNITED SERIES OF THE PHILOSOPHICAL MAGAZINE,  
ANNALS OF PHILOSOPHY, AND JOURNAL OF SCIENCE.

JANUARY—JUNE, 1847.

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**"Meditationis est perscrutari occulta; contemplationis est admirari  
perspicua . . . . . Admiratio generat quæstionem, quæstio investigationem,  
investigatio inventionem."—*Hugo de S. Victore.***

# CONTENTS OF VOL. XXX.

(THIRD SERIES.)

## NUMBER CXCVIII.—JANUARY, 1847.

	Page
Mr. T. Ransome on the Composition and Explosion of Gun-Cotton .....	1
Sir J. W. Lubbock on the Meteor of September 25, 1846 .....	4
Mr. G. Boole's Remarks on the Rev. B. Bronwin's Method for Differential Equations .....	6
Mr. C. J. Hargreave's Observations on the Rev. B. Bronwin's Paper on the Integration and Transformation of certain Differential Equations.....	8
Mr. E. C. Nicholson on the Compounds of Phosphoric Acid with Aniline .....	10
Mr. W. R. Birt on the remarkable Barometric Depression of the 25th December 1821 .....	18
The Rev. N. S. Heineken on the Relics of the Mechanical Productions of Abraham Sharp, the assistant of Flamsteed..	25
Mr. J. Cockle on some Formulæ which serve to indicate the limits of the application of Indeterminate Methods to the solution of certain Problems .....	28
Mr. J. Wilson on the Solvent Action of Drainage-Water on Soils .....	30
Prof. Challis's Report of Proceedings in the Cambridge Observatory relative to the New Planet. ....	33
Mr. J. P. Joule and Dr. Lyon Playfair on the Maximum Density of Water .....	41
M. le Dr. Ch. Robin on a peculiar Organ found in the Rays (Raia, Cuv.) .....	47
Notices respecting New Books .....	53
Proceedings of the Royal Society .....	57
————— Royal Astronomical Society.....	60
Buratite—a new Mineral .....	65
On Bisulphuret, Tersulphuret, and Sulphocyanuret of Methyle, by M. Auguste Cahours . ....	65
On Sulphocyanuret and Bisulphuret of Ethyle, by M. Auguste Cahours .....	67
Analysis of the White Diopside.....	68
Analyses of the Native Phosphates of Copper, by M. Hermann	68
On Chiolite—a new Mineral, by M. Hermann .....	70
Purification of Mercury, by M. Millon.....	70
Meteorological Observations for November 1846.....	71
Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at Chiswick, near London; by Mr. Veall at Boston; by the Rev. W. Dunbar at Applegarth Manse, Dumfries-shire; and by the Rev. C. Clouston at Sandwick Manse, Orkney.....	72



## NUMBER CXCIX.—FEBRUARY.

	Page
The Astronomer Royal on Sir David Brewster's New Analysis of Solar Light .....	73
Mr. J. P. Joule on the Effects of Magnetism upon the Dimensions of Iron and Steel Bars .....	76
Mr. J. W. Draper's Remarks on the Existence and Mechanism of the Negative or Protecting Rays of the Sun .....	87
Prof. Powell's Note to a former paper on the Theory of the Aberration of Light .....	93
Mr. G. Boole's Note on a Class of Differential Equations ....	96
Prof. Gianalessandro Majocchi's Observations and Experiments respecting the Origin of the Voltaic Current .....	97
Mr. A. W. Hobson on the Equation of Continuity in Fluid Motion .....	104
The Rev. B. Bronwin on the Integration of some Equations in Partial Differentials .....	107
Mr. John Southern's Experiments on the Density, Latent Heat and Elasticity of Steam .....	113
Mr. L. Thompson on the Exhalation of Bicarbonate of Ammonia by the Lungs .....	124
Proceedings of the Royal Society .....	125
———— Cambridge Philosophical Society .....	129
———— Royal Astronomical Society .....	130
On Newton's Telescope at the Royal Society .....	139
The Central Sun .....	140
On the Fermentation of Tartaric Acid and its products.—Butyro-Acetic Acid, by M. Nicklès .....	141
On the Ignition of Brass Wire in the Vapour of Alcohol, by M. Reinsch .....	143
On the Distillation of Mercury, by M. M. E. Millon .....	144
Easy method of preparing Iodide of Potassium .....	146
Formation of Glycerine, by M. Rochleder .....	146
On the Composition of Bohemian Glass .....	147
On Oxypicric Acid.—Styphnic Acid .....	148
Reduction of the Persalts of Iron by Zinc .....	149
Award of Medals.—Linnæan Society .....	150
Meteorological Observations for December 1846 .....	151
———— Table .....	152

## NUMBER CC.—MARCH.

Sir David Brewster's Reply to the Astronomer Royal on the New Analysis of Solar Light .....	153
Dr. A. Waller's Additional Observations on Hail, and on the Organic Bodies contained in Hailstones, &c. (With a Plate.)	159



	Page
The Rev. H. Moseley on a Machine for Calculating the Products, Quotients, Logarithms, and Powers of Numbers. (With a Plate.)	171
Mr. J. G. Children on the use of a mixture of Spirit of Wine and Camphine, as a Light for Optical Purposes	179
M. Encke on the newly-discovered Planet	181
Mr. H. S. Warner on $\sqrt{-1}$	185
Prof. J. Henry on the Induction of Atmospheric Electricity on the Wires of the Electric Telegraph	186
Mr. H. Spencer on the Form of the Earth no proof of Original Fluidity	194
Proceedings of the Royal Society	196
————— Royal Astronomical Society	207
On Newton's Telescope at the Royal Society	211
Formation of Sulphovinic Acid, by M. M. E. Millon	211
On a Method of producing Lights and Shades in equal perfection in Daguerreotype Pictures, by MM. Belfield Lefèvre and Leon Foucault	213
Observations on the Experiments of Messrs. Foucault and Fizeau, relative to the action of the Red Rays upon Daguerreotype Plates, by M. Edmond Becquerel	214
Researches on Mellon and its Compounds, by MM. Aug. Laurent and Ch. Gerhardt	218
On the Preparation, Properties and Composition of Acetal, by M. Stas	220
Presence of Sulphur in Metallic Substances struck by Lightning.	222
On Chromate of Chromium, by M. Rammelsberg	223
Meteorological Observations for January 1847	223
————— Table	224

### NUMBER CCL.—APRIL.

Mr. J. P. Joule on the Effects of Magnetism, &c. upon the Dimensions of Iron and Steel Bars	225
Prof. Challis's Second Report of Proceedings in the Cambridge Observatory relating to the New Planet (Neptune)	241
Mr. T. Hopkins on the Hourly Alterations of the Vapour Atmosphere at Bombay. (With a Plate.)	246
Dr. E. Ronalds's Remarks on the Extractive Material of Urine, and on the Excretion of Sulphur and Phosphorus by the Kidneys in an unoxidized state	253
Mr. A. Cayley's Note on a System of Imaginaries	257
Mr. E. F. Teschemacher on Gun-Cotton	258
Prof. E. Wartmann's Third Memoir on Induction. (With a Plate.)	261



	Page
Messrs. R. Porrett and E. F. Teschemacher on the Chemical Composition of Gun-Cotton .....	273
Dr. A. Waller's Microscopic Examination of the Papillæ and Nerves of the Tongue of the Frog, with Observations on the Mechanism of Taste. (With a Plate.) .....	277
On the Behaviour of Metallic Zinc towards Solutions of Mercury, by H. Rose. ....	290
On the Composition of the Organic Alkalies, by M. Aug. Laurent. ....	293
On the Change effected in Hydrated Peroxide of Iron when kept in Water, by M. G. C. Wittstein. ....	294
On Villemite, by MM. Delesse and Descloizeaux .....	295
Solubility of Carbonate of Lime in Water containing Carbonic Acid, by M. Lassaigne .....	297
Solubility of Phosphate of Lime in Water saturated with Carbonic Acid, by M. Lassaigne. ....	298
Crystallized Bile .....	298
Singular Property of Gun-Cotton mixture, by Prof. Draper ..	299
On the Dehydration of Sulphate of Lime under various circumstances, by E. Millon .....	299
Coffee as an Antidote to Acetate of Morphia .....	301
On the Formation of Cylindrical Masses of Snow in Orkney, by Charles Clouston, Esq. ....	301
Meteorological Observations for February 1847 .....	303
————— Table. ....	304

### NUMBER CCII.—MAY.

Sir David Brewster on the Knowledge of Distance given by Binocular Vision. (With a Plate.) .....	305
Dr. de Haldat on the Universality of Magnetism. ....	319
Mr. R. Maddrell on the Metaphosphates. ....	322
Mr. H. C. Sorby on the Amount of Sulphur and Phosphorus in various Agricultural Crops .....	330
Dr. H. Kolbe's Observations on the oxidizing power of Oxygen when disengaged by means of Voltaic Electricity .....	334
Sir R. I. Murchison on the Discovery of Silurian Rocks in Cornwall .....	336
Dr. J. W. Draper on the Production of Light by Heat .....	345
Prof. H. Rose on the Acid contained in the North American Columbite .....	360
Mr. W. J. Henwood's Abstract of Meteorological Observations made during the year 1846 at Gongo Soco, in the interior of Brazil .....	361
Proceedings of the Cambridge Philosophical Society .....	365
Electrical Experiments, by Prof. Henry .....	368
On the Examination of the Ashes of Organic Bodies, by H. Rose	369



	Page
Sugar in Healthy Blood.....	378
Chemical Action of Light .....	378
On some Compounds of Cyanogen, by M. Ad. Wurtz .....	378
Researches on Hydrates, Stannic and Metastannic Acids, by M. E. Fremy .....	380
On Sulphoxiphosvinic Acid and its Compounds, by M. Cloez..	382
On Anhydrous Hydrofluoric Acid .....	382
Meteorological Observations for March 1847 .....	383
————— Table.....	384

---

### NUMBER CCIII.—JUNE.

Mr. I. B. Cooke on the Use of a double Circuit in the measure- ment of Electrical Forces. (With a Plate.) .....	385
M. A. Descloizeaux's Physical and Geological Observations on the principal Geysirs of Iceland .....	391
Mr. R. Porrett on the existence of a New Vegeto-Alkali in Gun- Cotton .....	409
On some new Researches in Animal Chemistry. Extracted from a Letter from Professor Liebig to Dr. A. W. Hofmann ....	412
Dr. J. S. Muspratt on the Salts of Sulphurous Acid .....	414
Mr. T. Rowney's Analysis of the Bohemian Glass as found in the Combustion Tubes employed in Organic Analysis. ....	421
Prof. J. R. Young on a Generalization of a Theorem of Euler in reference to the Products of the Sums of Squares .....	424
Mr. W. Crum on a Method for the Analysis of Bodies contain- ing Nitric Acid, and its application to Explosive Cotton....	426
Sir D. Brewster on the Conversion of Relief by Inverted Vision	432
Sir G. C. Haughton's Experiments proving the common nature of Magnetism, Cohesion, Adhesion and Viscosity .....	437
Prof. Sir W. R. Hamilton on Quaternions; or on a New Sy- stem of Imaginaries in Algebra. ....	458
Sir D. Brewster's Observations on the Analysis of the Spectrum by Absorption .....	461
On the Preparation of the Ammoniaco-Magnesian Phosphate from Urine, by M. Boussingault .....	462
Chemical Researches on the Yolk of Eggs .....	463
On Plaster of Paris, by M. Plessy.....	464

---

### NUMBER CCIV.—SUPPLEMENT TO VOL. XXX.

Prof. Dove on the Phænomena exhibited by Polarized Light when its plane of polarization is rapidly rotated .....	465
Prof. Dove on the Depolarization of Light .....	469

	<u>Page</u>
Professors Wöhler and Liebig on Thialdine and Selenaldine, two new artificial organic bases . . . . .	470
Dr. R. A. Smith's Remarks on the Air and Water of Towns. .	478
Mr. W. R. Birt on certain Atmospheric or Barometric Waves which traversed Europe during November 1842 . . . . .	482
Sir G. C. Haughton's Experiments proving the common nature of Magnetism, Cohesion, Adhesion and Viscosity ( <i>concluded</i> )	502
On Carotin. . . . .	523
On the Preparation of Iodide of Lead . . . . .	524
On the Action of Bromine upon certain Salts, by M. Auguste Cahours . . . . .	525
A Chemical Investigation of some Silicates containing Chlorine, Sulphuric and Carbonic Acid, by M. Whitney of Boston, United States. . . . .	528
Meteorological Observations for April 1847 . . . . .	529
————— Table . . . . .	530
Index . . . . .	531

### PLATES.

- I. Illustrative of Dr. Waller's Paper on Hail, and on the Organic Bodies contained in Hailstones, &c.
- II. Illustrative of the Rev. H. Moseley's Paper on a Machine for calculating the Products, Quotients, Logarithms, and Powers of Numbers.
- III. Illustrative of Mr. Hopkins's Paper on the Hourly Alterations of the Vapour Atmosphere at Bombay.
- IV. Illustrative of Prof. Wartmann's Paper on Induction.
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THE  
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**PHILOSOPHICAL MAGAZINE**  
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**JOURNAL OF SCIENCE.**

[THIRD SERIES.]

JANUARY 1847.

I. *On the Composition and Explosion of Gun-Cotton.*  
By THOMAS RANSOME, Esq.\*

SINCE the announcement by Prof. Otto of the process for preparing gun-cotton, many experiments have been tried both with reference to improvement in its manufacture and also to its explosive force, but the only explanation of its composition has been derived from the quantity of nitric acid taken up by the cotton. The following experiments were made to ascertain the quantities of carbon and hydrogen it actually contains.

The gun-cotton that I have analysed was prepared with carded cotton, and nitric acid of specific gravity 1·514 mixed with half its bulk of sulphuric acid, in the proportion of an ounce and a half of nitric acid to each 100 grs. cotton. Thus prepared it was quite white, and the fibre appeared unaltered; it left not the smallest residue upon ignition, and was quite insoluble in æther.

The carbon and hydrogen were estimated, as is usual in substances containing nitrogen, the cotton being mixed with oxide of copper, and the last few inches of the combustion-tube filled with metallic copper. It decomposed quite tranquilly, not giving out more gas at once than could conveniently pass through the potash apparatus.

In the first analysis 2·35 grs. gave 2·255 grs. of carbonic acid and ·665 gr. of water. In the second 2·075 grs. gave 2·01 grs. of carbonic acid and ·59 gr. of water.

Calculated to 100 parts—

	I.	II.
Carbon .....	26·16	26·41
Hydrogen .....	3·14	3·19

\* Read to the Literary and Philosophical Society of Manchester, Dec. 1, 1846, and communicated by the Author.

*Phil. Mag.* S. 3. Vol. 30. No. 198. Jan. 1847.

The mean of three experiments to ascertain the increase in weight in converting cotton-wool into gun-cotton was 64·1 upon each 100 parts of cotton: if this increase was anhydrous nitric acid, the gun-cotton would contain 10·20 per cent. of nitrogen.

Composition in 100 parts, taking the mean of the analyses of carbon and hydrogen,—

Carbon.....	26·28
Hydrogen .....	3·16
Oxygen .....	60·36
Nitrogen .....	10·20
	<hr/>
	100·00

These numbers nearly represent the formula  $C_{12}H_8O_{20}N_2$  which would contain in 100 parts—

Carbon.....	26·82
Hydrogen .....	2·94
Oxygen .....	59·70
Nitrogen .....	10·54
	<hr/>
	100·00

The cotton used was also analysed, being burnt with chromate of lead instead of oxide of copper.

1·435 gr. gave 2·825 grs. of carbonic acid and ·91 gr. of water.

In 100 parts—

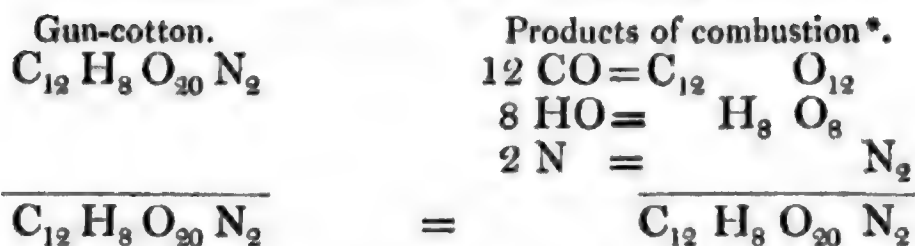
Carbon.....	44·20
Hydrogen .....	7·04
Oxygen .....	48·76
	<hr/>
	100·00

Theoretical composition of  $C_{12}H_{10}O_{10}$ —

Carbon.....	44·44
Hydrogen .....	6·17
Oxygen .....	49·39
	<hr/>
	100·00

From these formulæ, it appears that gun-cotton is formed from ordinary cotton by the abstraction of two atoms of hydrogen and the addition of two atoms of nitric acid. If this is the case, 100 parts of cotton ought to increase to 165·4; by experiment they increase to 164·1. The abstraction of two atoms of hydrogen will make the nitrogen ·18 per cent. more, which brings it nearer to the theoretical composition.

If this formula be correct, the cotton in exploding without the presence of oxygen would be converted entirely into carbonic oxide, water and nitrogen, thus:—



To ascertain whether carbonic oxide was produced, and if the cotton would explode without air, the following experiment was tried, the object being to explode the cotton either *in vacuo*, or in some indifferent gas which could afterwards easily be removed. I selected carbonic acid as being easily obtained and readily removed by a caustic alkali, and also because the air was more completely excluded than in the partial vacuum of an air-pump.

The gas was generated in a Woolfe's bottle, and dried by passing through a tube filled with chloride of calcium; it was then conducted into a rather wide tube in which the cotton was placed (about the tenth of a grain for each explosion); to this a small bent tube was attached, the end of which was under the water of the pneumatic trough. The carbonic acid was allowed to escape until a small quantity collected in a test-tube was entirely absorbed by caustic soda; the cotton was then exploded by placing a spirit-lamp under the part of the tube containing it, at the same time the gas given out at the small tube was collected in an inverted jar: this was repeated several times until a sufficient quantity of gas was obtained; the carbonic acid was then absorbed by caustic soda.

The residual gas was inflammable, and when mixed with oxygen and exploded by an electric shock was partially absorbed by caustic potash.

Red vapours were not formed when the gas was mixed with oxygen, showing that nitric oxide had not been formed.

After the explosion not the slightest residue was left in the tube.

This experiment proves that gun-cotton will explode without the presence of oxygen, and also under the same circumstances that carbonic oxide is formed, but no nitric oxide.

Manchester, Nov. 28, 1846.

Since the above paper was read, I have estimated the nitro-

\* Messrs. Porrett and Teschemacher have proved, in a paper read to the Chemical Society, that cyanogen is a product of the explosion of gun-cotton. As this is the case, the gases actually produced will not be exactly as described in the above diagram. It is possible that the gases would be produced in the proportion indicated above, if the gun-cotton exploded at a temperature lower than that requisite to cause the combination of the nitrogen with the carbon.—Ed.

4 Sir J. W. Lubbock *on the Meteor of September, 1846.*

gen from the ratio between the volumes of carbonic acid and nitrogen formed by combustion with oxide of copper. In one experiment the volume of carbonic acid was 2·43 cubic inches, to nitrogen ·46 cubic in. or as 6 to 1; therefore taking the carbon as the mean of the two former analyses, or 26·28, the nitrogen would amount to 10·33 per cent. I also endeavoured to ascertain the amount of nitrogen by measuring the volume of gas given out by a known weight of gun-cotton, and subtracting the carbonic acid as calculated from the previous estimation of carbon; but the quantity of nitrogen obtained was always too small, owing to the air in the tube taking part in the combustion, the largest amount obtained being 8·20 per cent.

Manchester, Dec. 14, 1846.

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II. *On the Meteor of September 25, 1846.*

By Sir J. W. LUBBOCK, *Bart., F.R.S.\**

*To the Editors of the Philosophical Magazine and Journal.*

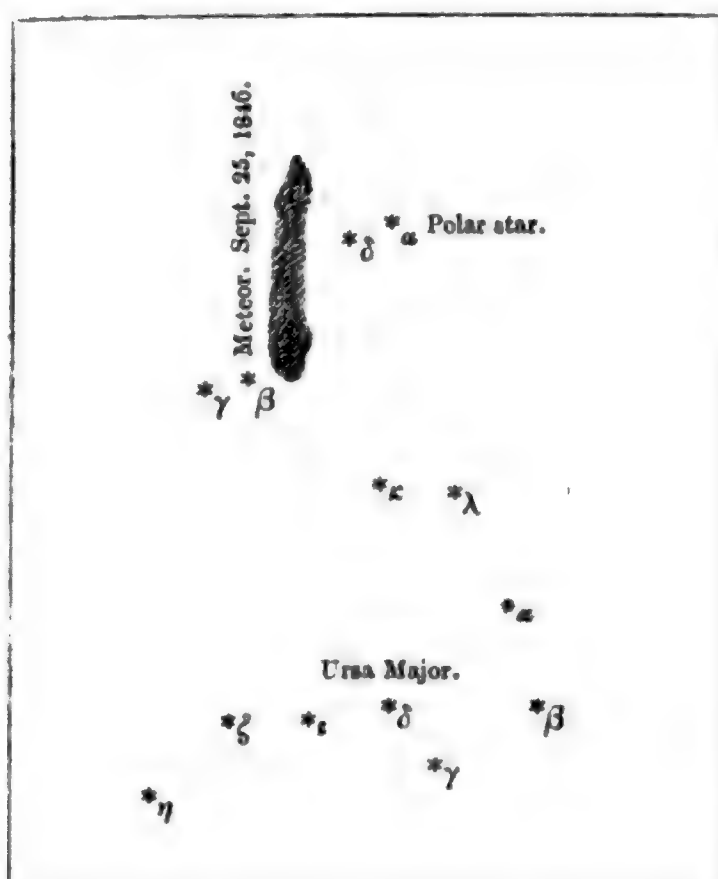
GENTLEMEN,

AS I happened accidentally to witness the extraordinary phenomenon which occurred on the 25th of September last †, and as it does not seem to have formed the subject of any other communication to your Journal, I address the following remarks, well-aware how imperfect unfortunately they must appear.

I happened to be walking just outside the house at a quarter before 10 P.M., when suddenly the whole scene was illuminated with a light, as it seemed to me, equal to that of the sun at noonday: certainly all objects became much more clearly visible than I ever saw them in the brightest moonlight. But contrast might lead to an exaggeration in this estimate. As the light proceeded from behind me, I immediately turned round, and I saw in Ursa Minor, and, as nearly as I can guess, occupying the place I have assigned to it in the inclosed sketch, a most brilliant meteor. I scarcely saw it before it faded, and in a few seconds it had entirely disappeared. As it disappeared the stars became again visible, and as I took particular note of its position I cannot be far out in my estimate. But the occurrence came so suddenly, never having before heard of any such apparition, and feeling confident that it would be observed by persons more competent than myself, that I neglected at the instant to observe by my watch the number of seconds it remained visible. Instrumental observation under any circumstances would have been impossible. I do not

\* Communicated by the Author. † See the Morning Post of the 26th.

apprehend that it *really* occupied so large a portion of the heavens as I have assigned to it; I think it probable that the



extreme rapidity of its motion left the impression upon the retina, but this can only be a matter of conjecture. It is much to be wished that any observation could be recorded of its position as seen from any other place, by which its parallax, if even roughly, could be obtained.

This body acquires still greater interest from its resemblance in external character to the *bolide* of the 21st March 1846, whose orbit has been calculated by M. Petit†, who assigns to it a period (round the earth) of ·1145685 day, or about two hours.

Another bolide was seen in France on the 9th of October of this year ‡, of which the external characters also resemble those of the meteor seen in London on the 25th of September 1846.

I am, Gentlemen,

Your most obedient Servant,

High Elms, near Farnborough, Kent,  
November 25, 1846.

J. W. LUBBOCK.

P.S. Long since what precedes was written, the number of the *Comptes Rendus* of the 23rd November last has been

† See *Comptes Rendus*, October 12, 1846.

‡ Ibid, October 26, 1846.



## 6 Mr. G. Boole on a Method for Differential Equations.

received, and in it are accounts of two other meteors observed at Dijon on the 17th of October and on the 9th of November. M. Méline describes the latter in words which would almost exactly represent the phænomenon which I have attempted, so imperfectly however, to place on record.

“Je sortais des serres du jardin, quand j’ai été frappé tout à coup d’une lumière aussi intense que celle du jour; j’aperçus distinctement toutes les parties du jardin, les arbustes comme les arbres, les plantes, etc. Je vis tout avec une teinte *jaune serin*. D’abord je crus à un incendie; mais, en jetant les yeux au ciel, j’ai vu un globe de feu se mouvant plus lentement qu’une fusée, de l’ouest à l’est, horizontalement, à 60 ou 70 degrés de hauteur. Le météore a laissé, sur toute la longueur de la route qu’il a suivie, une immense traînée d’un blanc couleur de cendre.”

### III. Remarks on the Rev. B. Bronwin’s Method for Differential Equations. By GEORGE BOOLE, Esq.

*To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,

I DESIRE very briefly to notice an error into which the Rev. B. Bronwin has fallen, in a paper on the Integration and Transformation of certain Differential Equations, published in the last Number of the Philosophical Magazine.

In doing this I shall adopt a notation which I have before employed in a similar description of analysis, and which has the advantages of brevity and simplicity. Changing for convenience  $p$  into  $m$  and  $y$  into  $u$ , we may observe that Mr. Bronwin’s transformations depend in all cases on the properties of two compound factors, which we shall designate by  $\pi_m$  and  $\rho$ , and which, upon whatever subject they may operate, combine in subjection to the relation

$$\pi_m \rho = \rho \pi_{m-1}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

the equation to be solved being

$$\pi_m u = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

Thus, in Mr. Bronwin’s first equation, D standing for  $\frac{d}{dx}$ ,

$$\pi_m = x(D^2 + k^2) + 2mD, \quad \rho = D^2 + k^2; \quad . \quad . \quad . \quad (3.)$$

in the second,

$$\pi_m = D^2 + kx D - mk, \quad \rho = D + kx; \quad . \quad . \quad . \quad (4.)$$

in the third,

$$\pi_m = x(D^3 + k^3) + 3mD, \quad \rho = D^3 + k^3; \quad . \quad . \quad . \quad (5.)$$

and so on for the rest.



Now (1.) gives  
 whatever  $v$  may be; let  $\rho v = u$ , then  $v = \rho^{-1}u$ , and we have  

$$\pi_m \rho v = \rho \pi_{m-1} v,$$

$$\pi_m u = \rho \pi_{m-1} \rho^{-1} u;$$
 so that the symbol  $\pi_m$  is equivalent in operation to the compound symbol  $\rho \pi_{m-1} \rho^{-1}$ ; writing then  $\pi_{m-1} = \rho \pi_{m-2} \rho^{-1}$ , we have  

$$\pi_m u = \rho^2 \pi_{m-2} \rho^{-2} u;$$

$$\pi_m u = \rho^m \pi_0 \rho^{-m} u.$$
 and finally,  
 Hence the equation  $\pi_m u = 0$  gives  

$$\rho^m \pi_0 \rho^{-m} u = 0;$$

$$\therefore u = \rho^m \pi_0^{-1} \rho^{-m} 0. \dots (6.)$$

This equation is unquestionably true, whatever may be the interpretation of the symbols  $\pi_m$  and  $\rho$ , provided that they satisfy the combination law (1.).  
 Now taking Mr. Bronwin's first equation, viz.

$$x(D^2 + k^2)u + 2mDu = 0,$$

$$\pi_m = x(D^2 + k^2) + 2mD, \rho = D^2 + k^2,$$

$$\pi_0 = x(D^2 + k^2),$$

and making  
 we have  
 whence (6.) gives  

$$u = (D^2 + k^2)^m \{x(D^2 + k^2)\}^{-1} (D^2 + k^2)^{-m} 0.$$

$$\{x(D^2 + k^2)\}^{-1} \text{ reduced to } (D^2 + k^2)^{-1} x^{-1},$$

$$u = (D^2 + k^2)^m \text{ in terms of } (D^2 + k^2)^{-1} x^{-1},$$

$$u = (D^2 + k^2)^{m-1} x^{-1} (D^2 + k^2)^{-m} 0.$$

But  
 therefore  
 Without entering into any special examination of the above result, and its difficulty, it is merely resting on the analogy of many similar cases, I which may be merely resting on the analogy of many similar cases, I should at once assert that, when  $m$  is greater than 0, it is not necessary that we should retain more than two. Thus one form of solution is unquestionably

$$u = (D^2 + k^2)^{m-1} x^{-1} (c \cos kx + c' \sin kx);$$

and there are, I believe, many equivalent forms. The nature of Mr. Bronwin's error consists in his virtually rejecting the factors  $x^{-1}$  and  $\cos kx + c' \sin kx$ , and his result is accordingly nugatory whenever  $m > 0$ . Similar remarks apply to the other equations in his paper, of which however I have only examined a small number in detail.

Perhaps, in connexion with this subject, it may not be quite irrelevant to mention, that about two years since I obtained the solution of the purely symbolical equation

$$\pi_m \pi_n u + q \rho u = 0,$$

in which, beside the law of combination,

$$\pi_m \rho = \rho \pi_{m+1}, \quad \pi_n \rho = \rho \pi_{n+1},$$

the further condition was imposed,

$$\pi_m \pi_n = \pi_n \pi_m - a(m-n)\rho.$$

This solution, which will appear in the Cambridge and Dublin Mathematical Journal for January 1847, involves the solution of a class of differential equations of which that of Laplace's functions is only a very particular case. Such methods, limited in their individual application, and apparently indefinite in their number, seem however to be chiefly valuable as exercises in symbolical algebra. Linear differential equations, and linear equations in finite differences, may, as I have elsewhere shown, be reduced to the general form

$$u + \phi_1(\pi)\rho u + \phi_2(\pi)\rho^2 u \dots = U,$$

in which  $\pi$  and  $\rho$  satisfy the relations  $f(\pi)\rho u = \rho f(\pi+1)u$  and  $f(\pi)\rho^m = f(m)\rho^m$ ; and additional experience confirms my belief, that the methods which are founded on the employment of this form are sufficient for every case. Mr. Bronwin's equations, when thus treated, are at once seen to be integrable.

It is but justice to add, that Mr. Bronwin's method displays considerable ingenuity.

I remain, Gentlemen,

Your obedient Servant,

Lincoln, Dec. 4, 1846.

GEORGE BOOLE.

IV. *Observations on the Rev. B. Bronwin's Paper on the Integration and Transformation of certain Differential Equations.* By CHARLES JAMES HARGREAVE, F.R.S.

*To the Editors of the Philosophical Magazine and Journal*

GENTLEMEN,

I TAKE the liberty of drawing your attention to an error which appears to exist in the paper contained in your December Number on the Integration and Transformation of certain Differential Equations.

I confine my observations in the first place to the first of the equations discussed by the author; but it will be seen that the same or similar remarks are applicable to the other equations.

The solution of the equation

$$x\left(\frac{d^2 y}{dx^2} + k^2 y\right) + 2p \frac{dy}{dx} = 0 \quad . \quad . \quad . \quad (1.)$$

is given as being  $y = (D^2 + k^2)^p u$ ,

where  $(D^2 + k^2)u = 0$ ,

a result which is evidently identical with  $y = 0$ , which is indeed a solution of (1.), but not the solution of which the author is in quest.

The error consists in inferring the equation

$$D^2 x u + (2p - 4)D u + k^2 x u = 0$$

from

$$(D^2 + k^2) (D^2 x u + (2p - 4)D u + k^2 x u) = 0.$$

The deduced equation ought to have been

$$D^2 x u + (2p - 4)D u + k^2 x u = (D^2 + k^2)^{-1} 0 = a \sin kx + b \cos kx,$$

which will lead to the result

$$y = (D^2 + k^2)^{p-1} \left\{ \frac{1}{x} (D^2 + k^2)^{-(p-1)} (a \sin kx + b \cos kx) \right\};$$

more conveniently written

$$y = (D^2 + k^2)^{p-1} \left\{ \frac{1}{x} (D^2 + k^2)^{-p.0} \right\}. \quad . \quad . \quad . \quad (2.)$$

In solving differential equations by successive operations of this nature, a difficulty frequently occurs with reference to the introduction of constants. Thus every operation denoted by  $(D^2 + k^2)^{-1}$  introduces two constants; and in many cases all the constants thus introduced except two, which are arbitrary, must be determined in terms of these two arbitrary constants by reference to the original differential equation.

This difficulty does not occur in the above equation (2.), which may be written in the simpler form,

$$y = (D^2 + k^2)^{p-1} \left\{ \frac{1}{x} (a \sin kx + b \cos kx) \right\}.$$

If  $p = 1$ ,  $y = \frac{1}{x} (a \sin kx + b \cos kx).$

If  $p = 2$ ,  $y = \frac{2}{x^3} (a \sin kx + b \cos kx) + \frac{2k}{x^2} (b \sin kx - a \cos kx),$

&c.

&c.

It will be found, in like manner, that the solution of the second of the equations given by Mr. Bronwin, viz.

$$\frac{d^2 y}{dx^2} + kx \frac{dy}{dx} - pk y = 0,$$

is

$$y = (D + kx)^p \{ D^{-1} (D + kx)^{-(p+1)} 0 \},$$

subject to the difficulty above adverted to as to the determination of the constants introduced by the operations.

Similarly, the solution of the third example,

$$x \left( \frac{d^3 y}{dx^3} + k^3 y \right) + 3p \frac{d^2 y}{dx^2} = 0,$$

is 
$$y = (D^3 + k^3)^{p-1} \left\{ \frac{1}{x} (D^3 + k^3)^{-p.0} \right\},$$

which will be found to be

$$y = (D^3 + k^3)^{p-1} \left\{ \frac{1}{x} (a \varepsilon^{-kx} + b \varepsilon^{\alpha kx} + c \varepsilon^{\beta kx}) \right\},$$

$\alpha$  and  $\beta$  being the imaginary cube roots of  $-1$ .

The solution of the fourth equation, viz.

$$x \frac{d^2 y}{dx^2} + (2p + 2mx) \left( \frac{dy}{dx} + my \right) = 0,$$

will be found to be

$$y = (D^2 + 2mD + 2m^2)^{p-1} \left\{ \frac{1}{x} (a \varepsilon^{\alpha x} + b \varepsilon^{\beta x}) \right\},$$

where  $\alpha$  and  $\beta$  are the roots of  $t^2 + 2mt + 2m^2 = 0$ .

A similar correction may be applied without difficulty to the other equations discussed by the author of the paper in question.

I am, Gentlemen,

Your obedient Servant,

69 Chancery Lane, Dec. 3, 1846. CHARLES JAMES HARGREAVE.

## V. On the Compounds of Phosphoric Acid with Aniline.

By EDWARD CHAMBERS NICHOLSON, Esq.\*

OF the inorganic acids there is none so remarkable for the number and variety of the compounds it forms with bases as phosphoric acid. The peculiar and complex reactions of the salts of this acid were first satisfactorily explained by Prof. Graham, in his well-known paper published in 1833†. The

\* Communicated by the Chemical Society; having been read May 18, 1846.

† Phil. Trans. of Royal Society of London, part 2. 1833. Researches on the Arseniates, Phosphates and Modifications of Phosphoric Acid, by Thos. Graham, Esq., M.A., F.R.S.E., Professor of Chemistry in the Andersonian University of Glasgow.

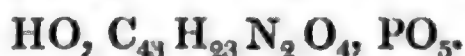
modifications of phosphoric acid by its amount of water of hydration, giving rise to pyrophosphoric and metaphosphoric acid, beautifully explained all that was enigmatical in the observed reactions.

Prof. Graham's excellent experiments have added a great deal to our knowledge of phosphoric acid; the study, however, of this acid and its salts is by no means exhausted. I consequently hope that any contribution to the subject will be accepted.

The following experiments, which were instituted in the laboratory of the Royal College of Chemistry, relate to the combination of phosphoric acid with a large and interesting group of bodies, viz. the organic alkaloids. It is curious that this class of salts has not been hitherto examined; indeed the composition of scarcely any phosphate of an organic base has been satisfactorily ascertained.

It was a very interesting question, whether these bases, which with monobasic acids comport themselves exactly the same as mineral oxides, were capable of combining with polybasic acids in the same remarkable proportions. In the absence of experiments, the only points which were known from whence we might, by way of analogy, have inferred any similarity in these compounds with the inorganic phosphates, are the salts of ammonia with phosphoric acid; ammonia being as it were the type of all the organic bases. A phosphate of ammonia indeed is known, containing two equivalents of the volatile alkali; but it still remained to be decided whether any true organic alkaloid unites in the proportions of two or more equivalents with one equivalent of phosphoric acid.

The only analysis of a phosphate with an organic alkali is published by Regnault\*. This chemist determined the carbon and hydrogen in phosphate of strychnine. Regnault considered the salt which he analysed as a compound of 1 equiv. of phosphoric acid, 1 equiv. of strychnine and water,



However, Regnault's formula of strychnine has been corrected by Professor Liebig†, who showed that this base contains one equivalent of carbon more, *i. e.*



it is therefore evident that the composition of Regnault's phosphate is to be expressed by the formula



\* Liebig's *Annalen*, vol. xxvi. p. 37.

† *Ibid.* vol. xxvi. p. 58.



as Professor Liebig\* already remarks, and, as may be seen from the following comparison of the per-centages corresponding to this formula with his analytical results, which I have calculated with the new atomic weights of carbon:—

44 eqs. Carbon . . . . .	3300·00	59·23	59·85
26 ... Hydrogen . . . . .	325·00	5·83	5·85
2 ... Nitrogen . . . . .	354·00	6·55	
7 ... Oxygen . . . . .	700·00	12·66	
1 ... Phosphoric acid . . . .	892·28	15·73	
1 ... Phosphate of strychnine	5571·28	100·00	

It is evident from these numbers that the phosphate of strychnine analysed by Regnault is a common phosphate, corresponding to the phosphate of soda with 1 equiv. of fixed base.

Besides this, Dr. A. T. Thomson† relates an analysis of phosphate of morphine by Dr. Gilbert, from which however no conclusion relative to the composition of this salt can be deduced, the analysis being incomplete.

It being a matter of indifference whether the base which I took for experiment was a natural alkaloid or one prepared artificially in the laboratory, I selected aniline, deeming it especially suited to my purpose, as this base generally forms with acids readily crystallizable compounds.

## I. COMPOUNDS OF TRIBASIC PHOSPHORIC ACID WITH ANILINE.

### 1. *Phosphate with 2 equivs. of Aniline and 1 equiv. of Water.*

On adding aniline in excess to a strong solution of tribasic phosphoric acid, the whole instantly solidified to a white crystalline mass, which was drained, pressed, dissolved in a large quantity of boiling dilute alcohol, and filtered through a warm funnel; on cooling, the whole became a mass of crystals; after pressing these between folds of bibulous paper, I finally dried them on a warm porous tile.

When dry, this salt appears as beautiful flesh-coloured nacreous plates, which might be readily taken for thionurate of ammonia, quite inodorous, slightly acid to test paper. They are very soluble in æther and water, sparingly soluble in cold alcohol, but very soluble in hot, so that upon cooling the whole

\* *Handwörterbuch der Chemie*, article 'Organic Bases,' vol. i. p. 70.

† *Pharm. Journal*, vol. ii. p. 500, 'On the Preparation, Characters, and Composition of the Nitrate, Phosphate, Tartrates and Hydriodate of Morphia,' by Anthony Todd Thomson, M.D., F.L.S.

liquid becomes solid. This salt cannot be dried at 100° C. (212° F.), because at this temperature it gives off aniline, assuming a red colour. It melts at a gentle heat, and upon raising the temperature gives off its aniline and leaves metaphosphoric acid.

*Analysis.*

When burnt with oxide of copper—

I. 0.419 grm. of the substance gave 0.774 grm. of carbonic acid and 0.227 grm. of water.

II. 0.646 grm. of the substance gave 1.183 grm. of carbonic acid and 0.356 grm. of water.

III. 1 grm. neutralized with ammonia and precipitated with nitrate of silver, gave 1.461 grm. of tribasic phosphate of silver.

These numbers correspond with the following per-cents. :—

	I.	II.	III.
Carbon . . . .	50.38	49.94	
Hydrogen . . .	6.01	6.19	
Phosphoric acid .	...	...	24.86

These determinations lead to the following formula,



as is seen by the following :—

24 eqs. Carbon . . . .	1800.00	50.57
17 ... Hydrogen . . . .	212.50	5.97
2 ... Nitrogen . . . .	354.10	9.97
3 ... Oxygen . . . .	300.00	8.42
1 ... Phosphoric acid .	892.28	25.07
1 ... Phosphate of aniline	3558.88	100.00

Therefore the salt corresponds to the common phosphate of soda,



and also to an ammonia salt of the same constitution.

It is anhydrous, like this ammonia salt; but, unlike these salts, possesses, as before remarked, an acid reaction, like all the other salts of aniline, this base being incapable of destroying the properties of an acid.

*2. Phosphate with 1 equiv. of Aniline and 2 equivs. of Water.*

This compound is obtained by adding tribasic phosphoric acid to the salt mentioned before until it ceases to produce a precipitate with chloride of barium, and concentrating in the water-bath. In the course of a few hours the salt crystallizes in beautiful silky needles; these are to be washed with æther and dried on a warm tile.

When dry the salt is quite white, but by exposure to the

air becomes rose-red; it is very soluble in æther, alcohol, and water; in the latter with decomposition, the common phosphate being produced.

### *Analysis.*

When burnt with oxide of copper—

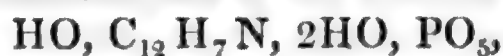
I. 0.456 grm. of the substance gave 0.633 grm. of carbonic acid and 0.223 grm. of water.

II. 0.403 grm. neutralized with ammonia and precipitated with nitrate of silver, gave 0.880 grm. of tribasic phosphate of silver.

These numbers correspond to the following per-cents. :—

	I.	II.
Carbon . . . . .	37.86	
Hydrogen . . . . .	5.44	
Phosphoric acid . . . . .	...	37.12

These determinations lead to the formula



as is seen by the following :—

12 eqs.	Carbon . . . . .	900.00	37.59	37.86
10 ...	Hydrogen . . . . .	125.00	5.22	5.44
1 ...	Nitrogen . . . . .	177.05	7.39	
3 ...	Oxygen . . . . .	300.00	12.54	
1 ...	Phosphoric acid . . . . .	892.28	37.26	37.12
1 ...	Phosphate of aniline	2394.33	100.00	

This salt corresponds to the biphosphate of soda,



Mitscherlich has analysed an ammonia compound of the same constitution.

I did not succeed in forming a phosphate in which the three equivalents of water in the hydrate are replaced by three equivalents of aniline. On adding aniline in excess to an alcoholic solution of the common phosphate, the base did not combine, and the common phosphate remained. I also tried to produce it by decomposing the phosphate with three equivalents of soda by oxalate of aniline; aniline however was instantly set free in the form of oily globules, and the common phosphate was produced with oxalate of soda.

I have not been more successful in endeavouring to prepare phosphates, into which aniline enters beside fixed bases. I have tried to obtain a salt corresponding to phosphorus salt,



by dissolving aniline in a solution of phosphate of soda with one equivalent of fixed base. The crystals, however, which



were formed did not contain soda, and were similar in appearance to the salt first-described. Similar results were obtained by neutralizing the acid phosphate of aniline with carbonate of soda. The salt produced did likewise not contain soda, the common phosphate appearing to be produced.

## II. PYROPHOSPHATE OF ANILINE.

There are two pyrophosphates of aniline, acid and neutral, which are always formed together when aniline is added in excess to pyrophosphoric acid. The precipitate produced by addition of aniline to pyrophosphoric acid is gelatinous, and becomes quite hard when exposed to the air.

Several combustions showed me that the compound obtained in this way consists of a mixture of the neutral and the acid pyrophosphate. I have not yet been able to obtain the neutral salt in a state of purity, but I found that by keeping the pyrophosphoric acid in great excess, the acid salt may be procured. The best way of forming it is to add aniline to rather concentrated pyrophosphoric acid (obtained by decomposing pyrophosphate of lead by hydrosulphuric acid), when the acid salt is precipitated along with the neutral compound; the mixture is then to be heated until the whole is dissolved, an excess of acid added, and the solution evaporated in the water-bath; upon cooling, the whole solidifies to a mass of beautiful white needles, which are to be pressed in bibulous paper, washed with æther, and finally dried *in vacuo*. This salt appears as white silky needles, very similar to the common basic sulphate of quinine; they are very acid, soluble in water, and almost insoluble in alcohol and æther; the crystals and their aqueous solution, like all other aniline salts, become red when exposed to the air.

### *Analysis.*

When burnt with oxide of copper—

I. 0.223 grm. of the substance gave 0.333 grm. of carbonic acid and 0.117 grm. of water.

II. 0.269 grm. of the substance gave 0.389 grm. of carbonic acid and 0.132 grm. of water.

III. 0.350 grm. of substance, neutralized by ammonia and precipitated by nitrate of silver, gave 0.578 grm. of pyrophosphate of silver.

These numbers correspond to the following per-cents. :—

	I.	II.	III.
Carbon . . .	39.00	39.43	
Hydrogen . .	5.68	5.45	
Phosphoric acid	...	...	38.85

which lead to the formula



as is seen by the following:—

12 eqs.	Carbon . . . . .	900·00	39·44	39·22
9 ...	Hydrogen . . . . .	112·50	4·93	5·56
1 ...	Nitrogen . . . . .	177·01	7·75	
2 ...	Oxygen . . . . .	200·00	8·77	
1 ...	Phosphoric acid . . . . .	892·28	39·11	38·85
1 ...	Pyrophosphate of aniline	2281·79	100·00	

This salt corresponds to the acid pyrophosphate of soda,



analysed by Professor Graham.

An analogous salt of ammonia is not known, pyrophosphate of ammonia existing only in solution, which, according to Graham's experiments, yields on evaporation crystals of the common phosphate of ammonia, with 2 equivalents of the volatile alkali and 1 equivalent of water.

### III. METAPHOSPHATE OF ANILINE.

I obtained this salt by adding a great excess of aniline to a very strong solution of metaphosphoric acid (glacial acid perfectly pure), or by adding metaphosphoric acid to a solution of aniline in alcohol or æther; in both cases the salt precipitates as a gelatinous white mass; this is to be placed on a filter and well-washed with æther, until the excess of aniline is removed and no smell of this body perceived, pressed and dried over sulphuric acid *in vacuo*.

When dry the salt appears as an amorphous white mass, which changes rose-red in the air, becoming glutinous. It is acid to litmus paper, soluble in water, and almost insoluble in alcohol and æther.

Its solution dissolves metaphosphate of silver, which property belongs also to metaphosphate of soda; upon boiling, it changes red, and partly reduces the silver salt. When an aqueous solution is boiled for a long time it is transformed into common phosphate of aniline.

#### *Analysis.*

When burnt with oxide of copper—

I. 0·664 grm. of the substance gave 0·976 grm. of carbonic acid and 0·264 grm. of water.

II. 0·994 grm. of the substance, precipitated by acetate of

lead, and the precipitate treated with sulphuric acid, gave 2·617 grms. of sulphate of lead.

III. I also determined the acid by precipitating with acetate of lead in great excess and boiling for a long time; the metaphosphate of lead is completely changed into the tribasic salt. This is a very advantageous method of determining metaphosphoric acid. 0·5785 grm. of the substance gave 1·3485 grm. of tribasic phosphate of lead.

These numbers correspond to the following per cents.:—

	I.	II.	III.
Carbon . . . . .	41·33		
Hydrogen . . . . .	4·55		
Phosphoric acid . . . . .	...	41·24	40·98

These analyses lead to the formula



as is seen by the following:—

12 eqs. Carbon . . . . .	900·00	41·48	41·33
8 ... Hydrogen . . . . .	100·00	4·62	4·55
1 ... Nitrogen . . . . .	177·01	8·11	
1 ... Oxygen . . . . .	100·00	4·62	
1 ... Phosphoric acid . . . . .	892·28	41·17	41·11
1 ... Metaphosphate of aniline	2169·29	100·00	

This salt is analogous to Prof. Graham's metaphosphate of soda,



which, according to the experiments of this chemist, likewise becomes viscid on evaporation, and never presents the slightest trace of crystallization.

The corresponding metaphosphate of ammonia is only known in solution. On evaporation this salt assimilates 1 equivalent of water, passing into the common phosphate with 1 equivalent of ammonia and 2 equivalents of water.

The experiments which I have described prove that there exist the following series of phosphates of aniline:—

Phosphates . . . . .	$\left\{ \begin{array}{l} 2(\text{HO, C}_{12}\text{H}_7\text{N}) \text{ HO, PO}_5. \\ \text{HO, C}_{12}\text{H}_7\text{N, 2HO, PO}_5. \end{array} \right.$
Pyrophosphate . . . . .	$\text{HO, C}_{12}\text{H}_7\text{N, HO, PO}_5.$
Metaphosphate . . . . .	$\text{HO, C}_{12}\text{H}_7\text{N, PO}_5.$

This series embraces compounds corresponding to all the combinations of soda with phosphoric acid, except the phosphate with 3 equivalents of soda, and the pyrophosphate with 2 equivalents of fixed base.

*Phil. Mag.* S. 3. Vol. 30. No. 198. Jan. 1847. C

The aniline salt, corresponding to the latter, however exists, but could as yet not be obtained in a state of purity.

All the compounds of aniline with phosphoric acid are anhydrous, like the other salts of this base which have been examined by Professor Hofmann. In this respect they differ from the corresponding soda salts, which nearly all contain a larger or smaller amount of water of crystallization. The two ammonia salts of phosphoric acid which we know, are however likewise anhydrous.

From the results of the preceding investigation we may conclude that the organic bases comport themselves also with polybasic acids like mineral oxides. I intend, however, to analyse the phosphates of some other organic alkaloids, particularly of those which occur in nature, and are frequently employed in medicine.

VI. *On the remarkable Barometric Depression of the 25th December 1821. By W. R. BIRT.*

*To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,

ON the 26th of November last the barometric depression, noticed at the close of my letter on the Great Symmetrical Barometric Wave, returned, making the sixteenth instance in eighteen years. The value of the depression at this station was 29.268 (unreduced): the observation was taken at 10 minutes past 8 in the morning. The value is about a tenth higher than the depression at Dublin on the 26th of November 1843. In that year the barometer commenced a decided rise on the 27th, which it also did at this station in the present year.

My friend E. W. Brayley, Jun., Esq., has suggested that this depression is somewhat allied to the great depression which occurred on the 25th of December 1821, and which has been made the subject of an investigation by Prof. Brandes of Breslau, and since by Prof. Dove of Berlin. The reader will find Prof. Brandes' paper in the *Annals of Philosophy*, N. S. vol. iv. p. 263, and Prof. Dove's in the *American Journal of Science and Art*, vol. xlv. p. 319. A paper on the same subject by Luke Howard is inserted in the *Philosophical Transactions*, vol. cxii. p. 113.

There can be no question that the depression of the latter end of November is periodical: the following interesting question consequently suggests itself. Is the depression of the 25th of December also periodical? To answer this question I selected all the readings given in Howard's *Climate of London* from

1806 to 1830 inclusive, on the 23rd, 24th, 25th, 26th and 27th of December. These readings, which consist of the maxima and minima, I have made the subject of a very careful discussion, the result of which forms the subject of this communication.

TABLE I.

Date.	Max.	Min.
1821.		
December 23.	29·17	28·75
24.	28·85	27·80
25.	28·45	27·80
26.	28·86	28·25
27.	28·97	28·50

This table gives the principal features of the depression of December 25, 1821, as it passed the neighbourhood of London. As we proceed with the investigation we shall find reason to isolate these observations.

TABLE II.—Barometric Maxima and Minima on Dec. 25.

Year.	Max.	Min.	Year.	Max.	Min.
1806	30·17	30·00	1819	29·50	29·40
1807	30·12	29·93	1820	29·81	29·70
1808	29·55	29·50	1821	28·45	27·80 <sup>b</sup>
1809	30·05	29·77	1822	30·47	30·40
1810	29·03	28·98 <sup>a</sup>	1823	29·88	29·70
1811	30·20	29·98	1824	29·80	29·44
1812	30·46	30·40	1825	30·05	29·77
1813	30·28	30·02	1826	30·55	30·42
1814	29·61	29·58	1827	30·56	30·48
1815	29·78	29·53	1828	29·73	29·50
1816	29·72	29·38	1829	30·53	30·26
1817	30·06	29·90	1830	29·61	29·58
1818	30·10	29·82			

From this table we learn that, with only two exceptions in twenty-five years, the barometer attained an elevation of at least 29·50 on the 25th of December in every year; the exceptions are the years 1810 and 1821 marked <sup>a</sup> and <sup>b</sup>.

TABLE III.

Date.	Max.	Min.
1810.		
December 23.	29·43	29·38
24.	29·46	29·26
25.	29·03	28·98
26.	29·77	29·51
27.	29·58	29·54



20 Mr. W. R. Birt on a remarkable Barometric Depression.

This table gives the principal features of the depression of 1810. It will immediately be seen from a comparison of the numbers of 1810 and 1821, that the depressions are essentially distinct.

TABLE IV.—Mean Barometric Maxima and Minima.

Day.	Max.	Min.	Range.
December 23.	29.91	29.72	.19
24.	29.99	29.80	.19
25.	30.00	29.84	.16
26.	29.96	29.78	.18
27.	29.94	29.75	.19

Rejecting on each day the readings of 1821 on the ground of the depression being so distinctly marked, and the readings being so considerably below any other of the series, and also rejecting on the 25th the readings for 1810 as evidently indicating a depression, and being nearly .5 below the lowest of the remaining readings, this table gives the means of the maxima and minima for the respective days; from which it appears that, on the average, the barometer passes a maximum on or about the 25th of December.

TABLE V.—Absolute Barometric Maxima and Minima.

Day.	Max.	Year.	Min.	Year.	Range.
Dec. 23.	30.40	{ 1818 1826 }	28.65	1824	1.75
24.	30.48	1827	29.25	{ 1815 1819 }	1.23
25.	30.56	1827	28.98	1810	1.58
26.	30.64	1826	29.00	1815	1.64
27.	30.80	1827	28.94	1814	1.86

From this table we learn that the years 1826 and 1827 were characterized by considerable elevations of the barometer from the 23rd to the 27th of December; and that in the years 1810, 1814, 1815, 1819 and 1824, the barometer sank to 29 inches or below, on the 23rd, 25th, 26th or 27th. In no instance except 1821 did the barometer fall below 29.25 on the 24th.

TABLE VI.—Showing the highest and lowest readings of the *Maxima* for the respective days and years in which they occurred.

Day.	Highest reading.	Year.	Lowest reading.	Year.	Range.
Dec. 23.	30.40	{ 1818 } { 1826 }	29.35	1819	1.05
24.	30.48	1827	29.40	1819	1.08
25.	30.56	1827	29.50	1819	1.06
26.	30.64	1826	29.42	1816	1.22
27.	30.80	1827	29.27	1811	1.53

In this table the *highest* readings of the maxima show the absolute altitude which the mercurial column attained on the respective days in the years named in the third column.

The *lowest* readings of the maxima show that the barometer attained a greater elevation than 29.25 on each day, from the 23rd to the 27th of December inclusive, in each year from 1806 to 1830 also inclusive, 1821 and 1810 only excepted; the exception of the year 1810 extends only to the 25th.

The reply which this examination furnishes to the question proposed is this—that the depressions of 1810 and 1821 were *accidental*, that is, they did not form portions of a periodical series of depressions, such as the depression of the latter end of November (see table on p. 359 in the last volume). Mr. Brayley however suggests that they may be instances of a *longer* series. May they not result from annual movements, which from some cause or other are *displaced*? The usual epoch of the crest of the great wave is about the middle of November; on some occasions it occurs as early as the latter end of October, and on others as late as the latter end of November, having a range of about a month.

This result conducts us to another of a very interesting character: we have just noticed that the great symmetrical wave is to a certain extent *erratic* as to the period of its transit. The depression of the latter end of November is to a certain extent *constant*, the period of its transit being confined to about four days: this has been determined from a consideration of the barometric curves of the last four days of November. Mr. Howard's observations do not furnish sufficient data for projecting the curves; still the readings lead to the conclusion, that some time during the five days, from the 23rd to the 27th of December inclusive, the barometer passes a maximum. We have an elevation of the mercurial column about this time.

From this it appears highly probable that we have two

22 Mr. W. R. Birt on a remarkable Barometric Depression.

points approximately fixed in our *annual barometric curve*, the *depression* on or about the 28th of November, and the *elevation* on or about the 25th of December. It also appears that we have two kinds of atmospheric movements that affect the barometer; those of an *erratic* character as to their period—the great symmetrical wave being an instance—and those of a more *constant* character, as the depression and elevation just noticed.

We now pass on to the evidence which the Greenwich observations afford as to the elevation of the 25th of December.

TABLE VII.—Barometric Readings, Royal Observatory, Greenwich.

Day.	Hour.	Barometer corrected.	Remarks.
1840.			
Dec. 23.	Noon.	30·060	
24.	Noon.	30·006	
25.	Noon.	30·245	{ Max. 30·348 midnight. Min. 30·135 2 A.M.
26.	Noon.	30·491	
26.	10 P.M.	30·558	Max. of the month.
28.	2 A.M.	30·402	
28.	Noon.	30·304	

From this table we find that the barometric readings on the 25th are strictly in accordance with the foregoing deductions, being considerably above 29·500; and we further learn that the maximum of this elevation occurred on the 26th at 10 at night; the readings of the 25th were therefore those of the *anterior* slope of the elevation. The maximum of the 26th was also the maximum of the month.

TABLE VIII.—Barometric Readings, Royal Observatory, Greenwich.

Day.	Hour.	Barometer corrected.	Remarks.
1841.			
Dec. 23.	Noon.	29·741	
24.	10 A.M.	29·940	Maximum.
24.	Noon.	29·924	
25.	Noon.	29·604	
25.	10 P.M.	29·539	Minimum.
26.	Noon.	29·726	
27.	2 P.M.	30·042	Maximum.
28.	Noon.	29·909	



The readings of the 25th are still in accordance with the results already arrived at. The trough between the two maxima occurred at 10 P.M. of the 25th, so that the readings of the 25th were those of the posterior slope of the first elevation.

TABLE IX.—Barometric Readings, Royal Observatory, Greenwich.

Day.	Hour.	Barometer corrected.	Remarks.
1842.			
Dec. 23.	Noon.	29.496	
24.	Noon.	29.586	
25.	10 A.M.	29.732	Maximum.
26.	12 P.M.	29.299	
27.	6 A.M.	29.149	Minimum
27.	Noon.	29.288	
28.	Noon.	29.957	

In this year the barometer passed a maximum on the 25th at 10 A.M., and this reading was the highest of the five days.

TABLE X.—Barometric Readings, Royal Observatory, Greenwich.

Day.	Hour.	Barometer corrected.	Remarks.
1843.			
Dec. 23.	Noon.	30.313	
24.	2 P.M.	30.374	
25.	10 A.M.	30.392	Maximum.
26.	Noon.	30.283	
27.	Noon.	30.369	
28.	Noon.	30.436	

The barometer again passed a maximum on the 25th at 10 A.M., and, as in 1842, this reading was the highest of the five days.

The observations in the four years fully confirm the results deduced from the observations as given in Howard's Climate of London.

By the kindness of Captain Childers, who has placed in my hands the hourly observations which he communicated to the British Association at its last meeting, I am enabled to ascertain the state of the barometer at St. Helier's, Jersey, for the above-named days in the years 1844 and 1845: it is strictly in accordance with all that has preceded.

TABLE XI.—Barometric Readings, St. Helier's, Jersey.

Day.	Hour.	Barometer uncorrected.	Remarks.
Dec. 23.	Noon.	30·185	Maximum.
23.	8 P.M.	30·260	
24.	Noon.	30·235	
25.	11 A.M.	30·120	
26.	Noon.	30·025	
27.	Noon.	29·980	
28.	Noon.	29·910	

The reading of the 25th occurred on the posterior slope of the elevation.

Captain Larcom's observations at Dublin, with Captain Childers' at Jersey for last year, will conclude the series. Captain Larcom's observations give a most decided and well-marked maximum on the 25th at 3 P.M.; the readings extend only to 9 A.M. of the 26th; but from the curve with which the Captain has most kindly furnished me, the reading on the 25th at 3 P.M. is the highest for the five days.

TABLE XII.—Barometric Readings, Ordnance Survey Office, Dublin.

Day.	Hour.	Barometer corrected.	Remarks.
Dec. 23.	Noon.	29·622	Maximum.
24.	Noon.	30·074	
25.	Noon.	30·134	
25.	3 P.M.	30·148	
25.	6 P.M.	30·124	
25.	9 P.M.	30·070	
26.	9 A.M.	29·631	

TABLE XIII.—Barometric Readings, St. Helier's, Jersey.

Day.	Hour.	Barometer uncorrected.	Readings.
Dec. 23.	Noon.	29·460	Maximum.
24.	Noon.	30·125	
25.	9 A.M.	30·260	
25.	3 P.M.	30·280	
25.	8 P.M.	30·300	
26.	Noon.	30·230	

*Notice of some Mechanical Productions of Abraham Sharp. 25*

Upon the whole, I apprehend we may fairly conclude, from the observations of thirty-one years, that on or about the 25th of December the barometer passes a maximum, and that this maximum is above 29.500. This conclusion is strengthened by the fact, that only two exceptions have been observed: a low barometer on the 25th will consequently demand close attention, and observations under such circumstances cannot fail of possessing great interest.

Should any of your readers be aware that between 1830 and 1840 depressions occurred on the 25th of December, a notice of them would be valuable.

I have the honour to be, Gentlemen,

Your very obedient Servant,

2 Sidney Place, Cambridge Road, Bethnal Green.  
London, Dec. 5, 1846.

W. R. BIRT.

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VII. *Relics of the Mechanical Productions of Abraham Sharp, the assistant of Flamsteed.* By the Rev. N. S. HEINEKEN.

GENTLEMEN,

Sidmouth, November 23, 1846.

**A**MONG my papers I have chanced to find one which I had intended *some years* since to forward for insertion in the Philosophical Magazine, if upon perusal you deemed it likely to interest any of your readers: again accidentally thrown in my way, I transmit it to you, hoping that, notwithstanding the interval which has passed, the statements therein made may still be found to be correct.

I am, Gentlemen,

Respectfully yours,

N. S. HEINEKEN.

It may perhaps be interesting to some of the readers of the Philosophical Magazine, to be informed that several of the mechanical productions of Abraham Sharp, the once celebrated assistant of Flamsteed, are still in existence and in good preservation. My attention was directed to this subject in consequence of observing his name mentioned in the review of the Rev. W. Whewell's *Flamsteed and Newton*. Feeling interested in everything which related to one so eminent in his day, I endeavoured, while on a visit at Bradford (Yorkshire), to obtain whatever information I could respecting the papers and "reliques" of the once "indefatigable calculator," and sought to procure some memento of him who is described by his biographer as having "had a clear head for contriving and a skilful hand for executing" any of the mechanical or philosophical apparatus which he required. The result of my inquiries I subjoin.

The lathe, mentioned in the *Encyclopædia Britannica* (article

‘Sharp’) for turning, rose-work, eccentric, swash-work, geometrical solids, &c., was sold some few years since at Bradford to Godfrey Wright, Esq., in the neighbourhood of Doncaster. It had for many years been used by its former possessor (W. Goodchild), and when I saw it was in excellent working condition; and many are the elaborate and beautiful specimens which I have seen of its efficiency, even at a period of upwards of a century since it was first constructed. Another smaller lathe, of a somewhat similar description, for turning ovals, and ovals combined with rose-work, which exhibits some ingenious contrivances of the same “master mind,” was purchased by a person whose name is “Murgy,” residing at Heaton near Bradford. The double sector mentioned in his biography and sculptured on his monument, is, I understand, in the possession of J. Bottomley, Esq. of Bradford, or his relative Miss Bamforth, of Manor Row, Bradford. The large (18-inch) elaborate ring dial, and also the set of calculating-rods in a walking-stick, were purchased by Mr. Muff, music-seller, Leeds. A beautifully divided plate for calculating, I was informed, was in the possession of — Skelton, Esq. of Bradford. A large equatorial (with the hour circle about thirty inches diameter) in complete preservation, was purchased at the sale at Bradford, and is now at the observatory of the York Philosophical Institution. A very ingenious and complete perambulator, measuring from *part of an inch* to 100 miles, together with some geometrical solids, turned in the first-mentioned lathe, now belong to my friend J. Waterhouse, Esq., of Well Head near Halifax. Of this perambulator I may observe, that it has only one *toothed wheel* in the whole of the machinery: all the intermediate measures are given from *the inch* to the *last index*, which moves *once in 100 miles*; and yet the disc of the dial is not more than about six inches in diameter and not a quarter of an inch in thickness. Of this perambulator I have made a model, which (with the consent of my friend) I will submit to you should you desire it. A mural quadrant of  $5\frac{1}{2}$  feet radius, diagonally divided, &c., I have in my own possession, and shall feel pleasure in showing it to any who may feel an interest about it. A spirit thermometer, the *scale* of which I am certain was the work of Abraham Sharp, from the punches used for the figures, was fixed in the wainscot of the hall at Horton (his residence). If the *tube* is the *original* one, this might be valuable for reference in a meteorological point of view; for I remember having seen many meteorological journals, which had been kept by Abraham Sharp, and which were among the papers left at Horton: these I believe are now in the

possession of — Giles, Esq., the representative of the family. Two planispheres were also among these papers, drawn by pen and ink; but so exquisitely, that until I examined them with a magnifying lens, I could not but believe them to be engravings. A vice which was made to turn on its axis, and which had the contrivance of a second pair of chaps near the screw, for pressing, I imagine, horn into moulds for *buttons*, was used by the before-named Mr. Goodchild as his working vice, and I dare say is still retained by some part of the family; and as I have mentioned the contrivance for horn-pressing, I may state, that even one set of these *identical buttons* is still in existence!! They are ornamented with every variety of rose-work patterns, and were probably intended by the philosopher for his holiday suit!! They are now, together with some lenses, regarded by their owner, Mr. Fearnley of Shipley, as precious relics, though I believe within a few years they have decorated modern broadcloth. The steel punches for figuring his instruments were constantly used by Mr. Goodchild for the same purpose; as were also several chisels, which, singularly enough, were made of *remarkably soft iron, case-hardened* at the cutting part. I remember also to have seen a “shutter micrometer” of brass at the Hall. This, I suppose, not having been sold, is retained by — Giles, Esq. It is much to be regretted that the ingenious and laborious productions of such men should be thus scattered abroad, and in too many instances allowed to decay or be wantonly destroyed. Two lamentable examples of this are, I learn from a paper in the Magazine of Science, exhibited even at the Royal Society, where the reflectors of Newton and Hadley are in a *state of complete dilapidation*. In the case of Abraham Sharp, it was by chance only that I was fortunate enough to save the large mural quadrant from the hands of an ignorant brazier, who was anxious to purchase it for the purpose of—mending kettles!! I was given to understand with respect to his MSS., that *many years* since, when they had been neglected by the owner of the house and left in a closet, the cook was in the habit of supplying herself from the ample store for the purpose of lighting fires and singeing fowls! what remained have since, I believe, been carefully preserved by — Giles, Esq., but they are now the mere *membra disjecta*—the Sibyl’s scattered leaves.

I am, Gentlemen,

Respectfully yours,

N. S. HEINEKEN.

P.S. One of the philosopher’s walking-canes, having its crutch-head formed into an opera-glass, I also saw at the Hall, and I hope is still preserved.



VIII. *On some Formulæ which serve to indicate the limits of the application of Indeterminate Methods to the solution of certain Problems.* By JAMES COCKLE, Esq., M.A., of Trinity College, Cambridge; Barrister-at-Law, of the Middle Temple. Second and concluding Part\*.

[The first part will be found at pp. 181–183 of the preceding volume†.]

LET  $x_1, x_2, \dots, x_n$  denote the roots of

$$x^n + x'x^{n-1} + x''x^{n-2} + \dots + x^{(n)} = 0,$$

the general equation of the  $n$ th degree, and, by a similar notation, let  $y_1, y_2, \dots, y_n$  be the roots, and  $y', y'', \dots, y^{(n)}$  the coefficients of the equation in  $y$ , when

$$y = \xi^{(0)} + \xi'x + \xi''x^2 + \dots + \xi^{xxii}x^{22},$$

it is required to point out how to determine  $\xi^{(0)}, \xi', \dots$ , so that we may have

$$y' = 0, \quad y'' = 0, \quad y''' = 0, \quad \text{and} \quad y^{(4)} = 0.$$

By means of  $y' = 0$ , eliminate  $\xi^{(0)}$  from the three succeeding equations, and, conformably with the notation adopted in one of my previous communications to this work‡, represent the respective results by

$${}_2Y'_n = 0, \quad {}_3Y'_n = 0, \quad \text{and} \quad {}_4Y'_n = 0.$$

Next, by processes which I have already referred to in the present periodical§, let  ${}_2Y'_n = 0$  be put under the form

$$h_1^2 + h_2^2 + \dots + h_{22}^2 = 0,$$

where  $h_1 = x'_1 \xi' + x''_1 \xi'' + \dots + x^{xxii}_1 \xi^{xxii}$ ,

and, in general,

$$h_r = x^{(r)}_r \xi^{(r)} + x^{(r+1)}_r \xi^{(r+1)} + \dots + x^{xxii}_r \xi^{xxii}.$$

We have twenty-two quantities ( $\xi$ ) at our disposal,—make

$$h_1^2 + h_2^2 = 0, \quad \text{. (A.),} \quad h_3^2 + h_4^2 = 0, \quad \text{. (B.),}$$

and  $h_5^2 + h_6^2 = 0, \dots, h_{21}^2 + h_{22}^2 = 0;$

with the aid of the last nine of these equations (previously put under the form

$$h_{r-1} \pm \sqrt{-1} h_r = 0),$$

let nine of the  $\xi$ 's be eliminated from the first two; after the elimination there will remain 22–9 or 13 of the quantities  $\xi', \xi'', \dots, \xi^{xxii}$  still undetermined.

Now it is known|| that, whatever be the number of the quantities  $\xi$ , the above expression for  $y$  is equivalent to

$$y = \Xi^{(0)} + \Xi'x + \Xi''x^2 + \dots + \Xi^{(n-1)}x^{n-1},$$

\* Communicated by Sir George Cayley, Bart.

† Phil. Mag. S. 3. vol. xxix.

‡ Ibid. vol. xxviii. p. 191.

§ Ibid. vol. xxvii. pp. 126, 292, 293.

|| Sixth Report of the British Association, p. 301 *et seq.*



where

$$\Xi^{(r)} = s_r^{(0)} \xi^{(0)} + s_r^{(1)} \xi' + s_r^{(2)} \xi'' + \dots + s_r^{(22)} \xi^{xxii};$$

so that the number of disposable and independent quantities ( $\Xi$ ) contained in the expression for  $y$  can in no case exceed  $n$ ; neither, after the elimination of the nine  $\xi$ 's, can it exceed 13, as is seen on referring to the last of the above equations. The elimination of those quantities does not, however, diminish the number of disposable quantities ( $\Xi$ ) except when  $n$  is greater than 13.

If then

$$h_1 = K_1' \Xi' + K_1'' \Xi'' + K_1''' \Xi''' + K_1^{iv} \Xi^{iv} + B_1$$

$$h_2 = K_2'' \Xi'' + K_2''' \Xi''' + K_2^{iv} \Xi^{iv} + B_2$$

$$h_3 = K_3''' \Xi''' + K_3^{iv} \Xi^{iv} + B_3$$

$$h_4 = K_4^{iv} \Xi^{iv} + B_4,$$

where  $B_1, \dots, B_4$  are functions of the  $n - 5$  quantities  $\Xi^v, \Xi^{vi}, \dots, \Xi^{(n-1)}$ , we see, by what precedes, that nine of the quantities  $\xi$  may be so determined as to enable us to decompose  ${}_2Y_n = 0$  into

$$h_1^2 + h_2^2 = 0, \dots (A.) \text{ and } h_3^2 + h_4^2 = 0, \dots (B.)$$

where  $h_1, \dots, h_4$  have the forms last above given, and the  $n - 5$  quantities  $\Xi^v, \Xi^{vi}, \dots, \Xi^{(n-1)}$  are undetermined, and perfectly at our disposal; at least when  $n$  is not greater than 13, and when  $n$  exceeds 13, we have eight of them undetermined and disposable. But it will be seen below that, for our present purpose, this last case does not require consideration.  $\Xi', \Xi'', \dots, \Xi^{iv}$ , have as yet no other conditions than (A.) and (B.) to satisfy.

Depress (A.) and (B.) to linear equations, and eliminate  $\Xi''', \Xi^{iv}$ , from  ${}_3Y'_n$  by their means. Then, on referring to my paper at pages 190-191 of the last volume but one of this work\*, it will be seen that, without determining  $\Xi', \Xi''$ , it will be possible to reduce the resulting equation to the form

$$(K_1' \Xi' + K_1'' \Xi'' + B_1)^3 + (K_2'' \Xi'' + B_2)^3 = 0;$$

or,  $h_1^3 + h_2^3 = 0; \dots \dots \dots (C.)$

and also that  $B_1$  and  $B_2$  will not give the illusory results which (under a different notation) I have before† pointed out, provided the number of disposable quantities  $\Xi^v, \dots, \Xi^{(n-1)}$  be more than three in number; this gives the condition

$$n - 5 > 3, \text{ or } n > 8 \dots \dots \dots (y.) \ddagger$$

With the aid of (C.) reduced to a linear form, eliminate  $\Xi'$  or  $\Xi''$  from  ${}_4Y'_n = 0$ , and solve the resulting equation.

\* Phil. Mag. S. 3. vol. xxviii.

† Ibid. pp. 190, 191, 395.

‡ This corresponds to the equation (y.) of p. 191 of Phil. Mag. S. 3. vol. xxviii.

In effect we solve (A.), (B.), (C.) and (D.), by means of  $\Xi'$ ,  $\Xi''$ ,  $\Xi'''$ ,  $\Xi^{iv}$ ; the remaining  $\Xi$ 's (excepting  $\Xi^{(0)}$ ) are determined in effecting the reduction of  ${}_3Y'_n$  to the form of (C.); and  $\Xi^{(0)}$  will be obtained from  $y'=0$ , after substituting in it the values of the other  $\Xi$ 's.

The above investigations give the formula

$$n(1, 1, 1, 1) \text{ (or, } n(1^4)) = 9; \quad . \quad . \quad . \quad (317'').$$

and shows that the *general equations of the NINTH and higher degrees may be transformed into others of the same degrees, from which the second, third, fourth and fifth terms disappear*: the corresponding formula for  $m$  (see the first part of this discussion\*) is

$$m(1, 1, 1, 1) = 11 \text{ (or 10)}. \quad . \quad . \quad . \quad (317.)$$

2 Church Yard Court, Temple,  
November 28, 1846.

*Postscript*, Dec. 14, 1846.—On looking over the proof-slip, I observe that, in this paper, I have not had occasion to use the foregoing notation for the *roots* of equations. But the above may be considered to suggest the following permanent notation; viz. that  $x_r$  should represent a root, and  $a^{(r)}$  the coefficient of the  $(r+1)$ th term of an equation in  $X$ ;  $X$  a quantity composed of symmetric functions of, and homogeneous and of the  $r$ th degree with respect to,  $x_1, x_2, \dots, x_n$ ; that  $\xi$  should denote the disposable quantities which enter (explicitly) into the equation for  $y$  and  $\Xi$ , the disposable quantities implicitly contained in that equation. When  $r$  is given as a number, we may, however, as above, express the coefficients by accents if  $r$  be small, or by Roman numerals if it be large. For facility of reference I have termed 'last' volume what was in fact the current one at the time of writing this paper.

### IX. On the Solvent Action of Drainage-Water on Soils.

By JOHN WILSON, Esq.†

**I**N the autumn of 1844, being a resident in East Lothian, where the system of *thorough draining* is very extensively carried out, it occurred to me that the drainage-water during its percolation of the soil must necessarily dissolve out and carry away a great portion of the soluble constituents of it, which, by the practice as at present followed, are carried off the land and entirely lost to the farmer. I therefore took advantage of the first fall of rain sufficient to set the drains

\* Phil. Mag. S. 3. vol. xxix. pp. 181–183.

† Communicated by the Chemical Society; having been read May 4, 1846.

running after the dry weather of the autumn, and collected some of the drainage-water, which I subjected to a partial analysis, the particulars of which were described in a paper read by Dr. W. Gregory at a meeting of the Royal Society, Edinburgh, in the early part of last year. The results I then obtained, though very incomplete, were quite sufficient to show me that they had a very important bearing on agriculture, and to induce me to go on with their further investigation.

About the usual quantity of rain had fallen during the time between November, when I collected the first sample, and April 29th, when I obtained the second, and during the whole of that period the land had laid ploughed as a winter fallow.

Immediately after the second sample was taken the field was prepared for seed and sown with guano and barley. In a few days after (May 16th) I was enabled to collect a third sample (of course from the same drains), and having submitted them to analysis, the following are the results:—

*Second Sample.*—18 lbs. of drainage-water on evaporation gave 15·2 grs. of solid residue, or about ·844 gr. to the pound.

Organic matter and water in combination	3·4
Silica . . . . .	0·9
Silicate of alumina . . . . .	0·4
Chloride of magnesium . . . . .	1·12
Chloride of sodium . . . . .	1·8
Chloride of calcium . . . . .	3·0
Sulphate of alumina . . . . .	0·85
Peroxide of iron . . . . .	2·1
Phosphate of lime . . . . .	0·3
	<hr/> 13·87

*Third Sample.*—18 lbs. of drainage-water on evaporation gave 27·5 grs. of solid residue, or about 1·525 gr. to the pound.

Organic matter, &c. . . . .	7·8
Silica . . . . .	0·7
Silicate of alumina . . . . .	0·2
Peroxide of iron . . . . .	2·25
Phosphate of magnesia . . . . .	1·8
Magnesia ? . . . . .	1·69
Chloride of sodium . . . . .	2·615
Chloride of calcium . . . . .	2·107
Carbonate of lime . . . . .	2·7
Phosphate of lime . . . . .	3·1
Phosphate of alumina . . . . .	0·45
	<hr/> 25·412

I should here observe that the first sample, collected in

November, after the drains had been dry for many weeks previous, contained 2·25 grs. of solid residue to the pound; whereas that collected in the following April (No. 2), after the land had been continually drenched by the winter's rains, only gave ·844 gr. to the pound.

On adding a quantity of easily soluble manure (guano) to the soil, the first waters (No. 3) that passed through not only brought with them an increased quantity (1·525 gr. to the pound), but they contained many of the very ingredients that constituted the value of the manure itself.

At the time the paper referred to was read at the Royal Society, Edinburgh, it was suggested that possibly the turbid portion first discharged from the drains after heavy rains might contain matter also very valuable to the soil; but on comparing the subjoined analysis with that of *the drained soil*, it appears to be composed of the same ingredients, with a decrease in the proportion of silica and an increase in the lime, both of which may be readily accounted for.

*Analysis of Soil deposited from Turbid Drain-Water.*

Silica . . . . .	60·0
Silicate of alumina . . . .	17·5
Protoxide of iron . . . .	6·5
Sulphate of lime . . . .	9·4
Sulphate of magnesia . . . .	0·75
Phosphate of lime . . . .	0·6
Alumina . . . . .	4·0
Water, &c. . . . .	1·25
	<hr/> 100·00

I do not for a moment wish to question the value of the principle of *thorough draining*; that is now I believe universally admitted; but if its results are deemed so beneficial to the farmer under the present practice, how much more so would they not become, if some remedy were devised either to prevent as much as possible this great abstraction, or else to render the enriched drainage-water again available to the soil!

This subject has not, I find, escaped the observant mind of Liebig, and in fact forms the basis of the 'Patent Manure,' according to his specification in October last. He has argued theoretically to the same end, and has proffered a remedy in the shape of a manure, by his patent process rendered much less soluble than before, which of course would not be acted upon so readily by the percolating rain-water, and would



consequently remain longer in the soil for the purposes of vegetation.

Mr. Smith of Deanston, a man to whom practical agriculture is deeply indebted, has suggested the application of all manures whatsoever in a largely diluted liquid state, and which I am informed has been most successfully applied on the western coast of Scotland.

Now both of these plans will most assuredly tend to lessen the loss at present sustained; the one by diminishing the solubility of the manures employed, and the other by rendering the drainage-water again available.

But should the farmer object to take advantage of either plan, it would appear expedient to avoid using as much as possible the very soluble manures, and instead of giving his land the usual good dose of manure that is expected to suffice for two or three seasons, to divide the quantity, and to apply it in as small a proportion and as frequently as the nature of his crops will permit him to do. In such case his crops will get more and his ditches less than by the present practice.

*X. Report of Proceedings in the Cambridge Observatory relative to the New Planet. By Professor CHALLIS\*.*

St. Catharine's Hall Lodge, Dec. 12, 1846.

**T**HE Syndicate appointed to visit the Observatory, conceiving the subject at the present time to possess peculiar interest, beg leave to submit to the senate the following statement of Prof. Challis, describing the course of observations, founded on the theoretical calculations of Mr. Adams of St. John's College, and made at the Observatory, with a view to the discovery of the new planet.

H. PHILPOTT, <i>Vice-Chancellor.</i>	G. PEACOCK.
JOHN GRAHAM.	JAMES CARTMELL.
B. CHAPMAN.	CHAS. W. GOODWIN.
W. WHEWELL.	W. C. MATHISON.
JOSHUA KING.	G. G. STOKES.

At a meeting of the Observatory Syndicate, held at the Observatory on December 4, for the despatch of ordinary business, a strong desire having been expressed by the Vice-Chancellor and the members of the Syndicate generally, to receive from me a special report of observatory proceedings relating to the newly-discovered planet, drawn up in such a manner, and in such detail, as would enable them to lay complete information on the subject before the members of the

\* Communicated by Professor Challis.

*Phil. Mag.* S.3. Vol. 30. No. 198. Jan. 1847.

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senate, I considered it to be my duty at once to comply with this request. A new body of the solar system has been discovered, by means depending on the furthest advances hitherto made in theoretical and practical astronomy, and confirming in a most remarkable manner the theory of universal gravitation. It is therefore on every account desirable that the members of the senate should be made fully acquainted with the part which has been taken by the Cambridge Observatory relatively to this important extension of astronomical science. The observations I shall have to speak of, and the reasons for undertaking them, are so closely connected with theoretical calculations performed by a member of this university to account for anomalies in the motion of the planet Uranus, that the history of the former necessarily involves that of the latter. I hope that for this reason, and because of the peculiar nature of the circumstances, I may be allowed to make a communication less formal and restricted in its character than a mere report of observatory proceedings.

The tables with which the observations of the planet Uranus have been uniformly compared were published by A. Bouvard in 1821. They are founded on a continued series of observations extending from 1781, the year of its discovery, to 1821. Previous to 1781, it had been accidentally observed seventeen times as a fixed star, the earliest observation of this kind being one by Flamsteed in 1690. Bouvard met with a difficulty in forming his tables. On an attempt to found them upon the ancient as well as the modern observations, it appeared that the theoretical did not agree with the observed course of the planet. He thought this might be attributed to the imperfection of the ancient observations, and consequently rejected all previous to 1781 in the formation of the tables finally published. These tables represent well enough the observations in the forty years from 1781 to 1821; but very soon after the latter year new errors began to show themselves, which have gone on increasing to the present time. It was now evident that the ancient observations had been rejected on insufficient grounds, and that from some unknown cause the theory was in fault. Were the tables calculated inaccurately? The difference between observation and theory (amounting in 1841 to  $96''$  of geocentric longitude) was too great, and Bouvard's calculations were made with too much care to allow of this explanation. The effect of small terms neglected in the calculation of the perturbations caused by Jupiter and Saturn, could not be supposed to bear any considerable proportion to the observed amount of error. This state of the theory suggested to several astronomers the idea



of disturbances caused by an undiscovered planet more distant than Uranus. But there is no evidence of this hypothesis having been put to the test of calculation previous to 1843. The usual problem of perturbations is to find the disturbing action of one body on another by knowing the positions of both. Here an inverse problem, hitherto untried, was to be solved; viz. from known disturbances of a planet in known positions, to find the place of the disturbing body at a given time. Mr. Adams, Fellow of St. John's College, showed me a memorandum made in 1841, recording his intention of attempting to solve this problem as soon as he had taken his degree of B.A. Accordingly, after graduating in January 1843, he obtained an approximate solution by supposing the disturbing body to move in a circle at twice the distance of Uranus from the sun. The result so far satisfied the observed anomalies in the motion of Uranus as to induce him to enter upon an exact solution. For this purpose he required reduced observations made in the years 1818-26, and requested my intervention to obtain them from Greenwich. The Astronomer Royal, on my application, immediately supplied (Feb. 15, 1844) all the heliocentric errors of Uranus in longitude and latitude, from 1754 to 1830, completely reduced. Mr. Adams was now furnished with ample data from observation, and his next care was to ascertain whether Bouvard's theoretical calculations were correct enough for his purpose. He tested the accuracy of the principal terms of the perturbations caused by Jupiter and Saturn, and concluded that the small terms which Bouvard had not taken into account would not sensibly affect the final results, the chief of them being either of long period, or of a period nearly equal to that of Uranus. Besides which he introduced into the theory several corrections which had been derived from observation and calculation by different astronomers since 1821. The calculations were completed in 1845. In September of that year, Mr. Adams placed in my hands a paper containing numerical values of the mean longitude at a given epoch, longitude of perihelion, eccentricity of orbit, mass, and geocentric longitude, Sept. 30, of the supposed disturbing planet, which he calls by anticipation "the new planet," evidently showing the conviction in his own mind of the reality of its existence. Towards the end of the next month, a communication of results slightly different was made to the Astronomer Royal, with the addition of what was far more important, viz. a list of the residual errors of the mean longitude of Uranus, for a period extending from 1690 to 1840, after taking account of the disturbing effect of the supposed planet. This comparison of observation

with the theory implied the determination of *all* the unknown quantities of the problem, both the corrections of the elements of Uranus, and the elements of the disturbing body. The smallness of the residual errors proved that the new theory was adequate to the explanation of the observed anomalies in the motion of Uranus; and that as the error of longitude was corrected for a period of at least 130 years, the error of radius vector was also corrected. As the calculations rested on an assumption made according to Bode's law, that the mean distance of the disturbing planet was double that of Uranus, without the above-mentioned numerical verification, no proof was given that the problem was solved, or that the elements of the supposed planet were not mere speculative results. The earliest evidence of the complete solution of an inverse problem of perturbations is to be dated from October 1845.

Although the comparison of the theory with observation proved synthetically that the assumed mean distance was not very far from the truth, it was yet desirable to try the effect of an alteration of the mean distance. Mr. Adams accordingly went through the same calculations as before, assuming a mean distance something less than the double of that of Uranus, and obtained results which indicated a better accordance of the theory with observation; and led him to the conclusion, which has since been confirmed by observation, that the mean distance should be still further diminished. This second solution, taken in conjunction with the first, may be considered to relieve the question of every kind of assumption. The new elements of the disturbing body, and the results of comparing the observed with the theoretical mean longitudes of Uranus, were communicated to the Astronomer Royal at the beginning of September 1846. These were accompanied by numerical values of errors of the radius vector, the Astronomer Royal having inquired after the reception of the first solution, whether the error of radius vector, known to exist from observation, was explained by this theory. It would be wrong to infer that Mr. Adams was not prepared to answer this question till he had gone through the second solution. Errors of radius vector were as readily deducible from the first solution as from the other.

The preceding details are intended to point out the circumstances which led astronomers to suspect the existence of an additional body of the solar system, and the theoretical reasons there were for undertaking to search for it. No one could have anticipated that the place of the unknown body was indicated with any degree of exactness by a theory of this kind. It might reasonably be supposed, without at all mistrusting

the evidence which the theory gave of the *existence* of the planet, that its position was determined but roughly, and that a search for it must necessarily be long and laborious. This was the view I took, and consequently I had no thought of commencing the search in 1845, the planet being considerably past opposition at the time Mr. Adams completed his calculations. The succeeding interval to midsummer of 1846 was a period of great astronomical activity, the planet Astræa, Biela's double comet, and several other comets successively demanding attention. During this time I had little communication with Mr. Adams respecting the new planet. Attention was again called to the subject by the publication of M. Le Verrier's first researches in the *Comptes Rendus* for June 1, 1846. At a meeting of the Greenwich Board of Visitors held on June 29, at which I was present, Mr. Airy announced that M. Le Verrier had obtained very nearly the same longitude of the supposed planet as that given by Mr. Adams. On July 9 I received a letter from Mr. Airy, in which he suggested employing the Northumberland telescope in a systematic search for the planet, offering at the same time to send an assistant from Greenwich in case I declined undertaking the observations. This letter was followed by another dated July 13, containing suggestions respecting the mode of conducting the observations, and an estimation of the amount of work they might be expected to require. In my answer, dated July 18, I signified the determination I had come to of undertaking the search. Various reasons led me to this conclusion. I had already, as Mr. Adams can testify, entertained the idea of making these observations; the most convenient time for commencing them was now approaching; and the confirmation of Mr. Adams's theoretical position, by the calculations of M. Le Verrier, appeared to add very greatly to the probability of success. I had no answer to make to Mr. Airy's offer of sending an assistant, as I understood the acceptance of it to imply the relinquishing on my part of the undertaking.

I have now to speak of the observations. The plan of operations was formed mainly on the suggestions contained in Mr. Airy's note of July 13. It was recommended to sweep over three times at least, a zodiacal belt  $30^{\circ}$  long and  $10^{\circ}$  broad, having the theoretical place of the planet at its centre; to complete one sweep before commencing the next; and to map the positions of the stars. The three sweeps, it was calculated, would take 300 hours of observing. This extent of work, which will serve to show the idea entertained of the difficulty of the undertaking before the planet was discovered, did not appear to me greater than the case required. It will

be seen that the plan did not contemplate the use of hour xxi. of the Berlin star-maps, the publication of which was equally unknown at that time to Mr. Airy and myself. It may be proper here to explain, that the construction of a good star-map requires a great amount of time and labour both in observing and calculating, and that precisely this sort of labour must be gone through to conduct a search of the kind I had undertaken. The stars must first be mapped before the search can properly be said to begin. With a map ready made, the detection of a moving body, as it happened in this instance, might be effected on a comparison of the heavens with the map by mere inspection. Not having the advantage of such a map, I proceeded as follows. I noted down very approximately the positions of all the stars to the 11th magnitude, that could be conveniently taken as they passed through the field of view of the telescope, the breadth of the field with a magnifying power of 166 being  $9'$ , and the telescope being in a fixed position. When the stars came thickly, some were necessarily allowed to pass without recording their places. Wishing to include *all* stars to the 11th magnitude, I proposed in going over the same region a second time, to avail myself of an arrangement peculiar to the Northumberland equatoreal, the merit of inventing which is due to Mr. Airy. The hour-circle, telescope and polar frame, are moveable by clock-work, which may be regulated to sidereal time nearly. While this motion is going on, the telescope and polar frame are moveable *relatively to the hour circle*, by a tangent-screw apparatus, and a handle extending to the observer's seat. This contrivance enables the observer to measure at his leisure differences of right ascension however small, and therefore meets the case of stars coming in groups. The observations made by this method might include all the stars it was thought desirable to take, and therefore might include *all* the stars taken in the first sweep. The discovery of the planet would result from finding that any star in the first sweep was not in its position in the second sweep. If two sweeps failed in detecting the planet among the stars of the first sweep, it might be among the stars of the second, which would be decided by taking a third sweep of the same kind as the second. It will appear that this plan carried out, would not only detect the planet if it were in the region explored, but would also, in case of failure, enable the observer to pronounce that it was not in that region. The second mode of observing required the aid of my two assistants, Mr. Morgan and Mr. Breen, in reading off and recording the observations.

I commenced observing July 29, employing on that day



the first method, with telescope fixed. The next day I observed according to the second method, with telescope moving. On Aug. 4, the telescope was fixed as to right ascension, but was moved in declination in a zone of about 70' breadth, the intention of the observations of that day being to record points of reference for the zones of 9' breadth. On Aug. 12, the fourth day of observing, I went over the same zone, telescope fixed, as on July 30 with telescope moving. Soon after, Aug. 12, I compared to a certain extent, the observations of that day with the observations of July 30, taken with telescope moving; and finding, as far as I carried the comparison, that the positions of July 30 included *all* those of Aug. 12, I felt convinced of the adequacy of the method of search I had adopted. The observations were continued with diligence to Sept. 29, chiefly with telescope fixed, and were made early in right ascension for the purpose of exploring as large a space as possible before I should be compelled to desist by the approach of daylight. On Oct. 1, I heard that the planet was discovered by Dr. Galle at Berlin, on Sept. 23. I had then recorded 3150 positions of stars, and was making preparations for mapping them. The following results were obtained by a discussion of the observations after the announcement of the discovery.

On continuing the comparison of the observations of July 30 and Aug. 12, I found that No. 49, a star of the 8th magnitude in the series of Aug. 12, *was wanting in the series of July 30.* According to the principle of the search, this was the planet. It had wandered into the zone in the interval between July 30 and Aug. 12. I had not continued the former comparison beyond No. 39, probably from the accidental circumstance that a line was there drawn in the memorandum book in consequence of the interruption of the observations by a cloud. After ascertaining the place of the planet on Aug. 12, I readily inferred that it was also among the reference stars taken on Aug. 4. Thus, after four days of observing two positions of the planet were obtained. This is entirely to be attributed to my having on those days directed the telescope towards the planet's theoretical place, according to instructions given in a paper Mr. Adams had the kindness to draw up for me. I would also beg to call attention to the fact, that, after Aug. 12, the planet was discoverable by a closet-comparison of the observations; a method of observing, depending on novel and ingenious mechanism, having been adopted, by which I could say of each star, to No. 48, 'This is not a planet,' and of No. 49, 'This *is* a planet.' I lost the opportunity of announcing the discovery by deferring the dis-



cussion of the observations, being much occupied with reductions of comet observations, and little suspecting that the indications of theory were accurate enough to give a chance of discovery in so short a time. On Sept. 29, I saw for the first time the communication presented by M. Le Verrier to the Paris Academy on Aug. 31. I was much struck with the manner in which the author limits the field of observation, and with his recommending the endeavour to detect the planet by its disc. Mr. Adams had already told me, that, according to his estimation, the planet would not be less bright than a star of the ninth magnitude. On the same evening I swept a considerable breadth in declination, between the limits of right ascension marked out by M. Le Verrier, and I paid particular attention to the physical appearance of the brighter stars. Out of 300 stars, whose positions I recorded that night, I fixed on one which appeared to have a disc, and which proved to be the planet. This was the third time it was observed before the announcement of the discovery reached me. This last observation may be regarded as a discovery of the planet, due to the good definition of the noble instrument which we owe to the munificence of our Chancellor.

From the reduced places of the planet on Aug. 4 and Aug. 12, and from observations since its discovery extending to Oct. 13, Mr. Adams calculated, at my request, values of its heliocentric longitude at a given epoch, its actual distance from the sun, longitude of the node, and inclination of the orbit, which were published as early as Oct. 17. I am now diligently observing the planet with the meridian instruments; and when daylight prevents its being seen on the meridian, I propose carrying on the observations as long as possible with the Northumberland equatoreal, for the purpose of obtaining data for a further approximation to the elements of the orbit.

My report of proceedings relating to the planet here terminates. I beg permission to add a few remarks, which the facts I have stated seem to call for. It will appear by the above account that my success might have been complete if I had trusted more implicitly to the indications of the theory. It must however be remembered that I was in quite a novel position: the history of astronomy does not afford a parallel instance of observations undertaken entirely in reliance upon deductions from theoretical calculations, and those too of a kind before untried. As the case stands, a very prominent part has been taken in the University of Cambridge, with reference to this extension of the boundaries of astronomical science. We may certainly assert to be facts, for which there is documentary evidence, that the problem of determining

from perturbations the unknown place of the disturbing body was first solved here; that the planet was here first sought for; that places of it were here first recorded; and that approximate elements of its orbit were here first deduced from observation. And that all this may be said is entirely due to the talents and labours of one individual among us, who has at once done honour to the University and maintained the scientific reputation of the country. It is to be regretted that Mr. Adams was more intent upon bringing his calculations to perfection, than on establishing his claims to priority by early publication. Some may be of opinion, that in placing before the first astronomer of the kingdom results which showed that he had completed the solution of the problem, and by which he was, in a manner, pledged to the production of his calculations, there was as much publication as was justifiable on the part of a mathematician whose name was not yet before the world, the theory being one by which it was possible the practical astronomer might be misled. Now that success has attended a different course, this will probably not be the general opinion. I should consider myself to be hardly doing justice to Mr. Adams, if I did not take this opportunity of stating, from the means I have had of judging, that it was impossible for any one to have comprehended more fully and clearly all the parts of this intricate problem; that he carefully considered all that was necessary for its exact solution; and that he had a firm conviction, from the results of his calculations, that a planet was to be found.

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*XI. On the Maximum Density of Water.*

*By* JAMES P. JOULE, *Esq.* and Dr. LYON PLAYFAIR\*.

**I**N all researches on specific gravity the selection of a proper standard of comparison is a matter of great importance. For obvious reasons water has been universally selected as this standard; but a diversity of opinion has been entertained as to the temperature at which the gravity of water should be called unity. Hence, whilst our continental neighbours have adopted the freezing temperature, our own countrymen have generally chosen the temperature of  $60^{\circ}$  Fahr. Water at  $60^{\circ}$  cannot be a desirable standard, on account of its high rate of expansion at that temperature; and for the same and other reasons the temperature of  $32^{\circ}$  is not at all more convenient. We conceive that it would be much more philosophical, and lead to many practical advantages, if water

\* Communicated by the Chemical Society; having been read April 20, 1846.

at its maximum density were taken as the unit\*. In that case, calculations would often be greatly facilitated; and if the temperature of the water should happen to be a degree under or over the maximum point in any experiment, the extreme slowness of the expansion would avert the possibility of a grave error.

Intending to give still greater accuracy to our future experiments, we deem the present a favourable opportunity for fixing upon a good unit of comparison. We propose therefore to occupy the present series with a brief discussion as to the point of temperature at which water arrives at its state of greatest density.

Although a variety of methods have been employed in the investigation of this subject, they may be classified under two general heads. The first of these embraces all the methods involving the necessity of an accurate acquaintance with the rate of the expansion of some solid body by heat. The inquiry, when made in this way, is one of great difficulty, and has occupied the attention of many of our most accurate experimenters. The Florentine Academicians, Croune, Deluc, Dalton, and others, compared the indications of a thermometer filled with water with one filled with mercury; Lefevre Gineau and Hallström weighed a solid body in water at different temperatures; and Blagden and Gilpin measured the variation of volume by the variation of the weight of water contained in the same vessel at different temperatures.

The other general method does not require a previous acquaintance with the expansion of a solid by heat: it consists virtually in weighing water in water—the heavier water descending, while the lighter ascends to replace it. This principle was introduced by Dr. Hope, who applied it in the following elegant manner:—He filled with water at different temperatures tall glass jars having thermometers at top, middle and bottom. In this way he observed that when water was cooled down to  $40^{\circ}$  at the surface, it sank to the bottom; and when cooled below  $40^{\circ}$  at the bottom, it rose again to the surface. Trallés, Count Rumford, Ekstrand and Despretz have repeated Hope's experiment with a similar apparatus.

Believing that the second general method is susceptible of a far greater degree of accuracy than the first, we at once determined to employ it in our own experiments. The particular

\* Since this paper was communicated to the Society we have seen the *Annuaire du Bureau des Longitudes* for 1845, in which, under the high authority of Arago, water at its point of maximum density is assumed as the unit for comparison.

apparatus of Dr. Hope did not however appear to us to present the method in a form calculated to give results of great accuracy; and hence we have found it necessary to devise a new instrument combining all the theoretical advantages with the requisite facilities for exact observation.

Our instrument is represented in the accompanying sketch. *a a* are two upright vessels of tinned iron, each  $4\frac{1}{2}$  feet high and 6 inches in diameter: they are connected at the bottom by means of a brass pipe *b*, furnished with an accurately wrought stop-cock. This pipe is altogether 6 inches long, and enters 1 inch within each vessel. When the stop-cock is opened a clear passage of 1 inch diameter throughout forms a communication between the vessels. A rectangular trough of tinned iron, *c*, 6 inches long, 1 inch broad, and 1 inch deep, forms a communication between the tops of the vessels. In the middle of this trough there is a slide, by means of which the motion of a current along the trough can be stopped when requisite.



The vessels were supported in two places by means of the wooden brackets *d d*, and in order to prevent the greater than desired effect of the atmosphere in raising or depressing their temperature, they were completely covered with haybands. During the experiments the instrument was placed upon a tripod stool resting upon a support quite independent of the floor of the laboratory, in order to keep it entirely free from vibration.

Now if the two vessels be filled with water and made to communicate with one another by opening the stop-cock and removing the slide, it is evident that a current will tend to flow through the trough connecting the tops of the vessels, if the density of the water in one of the vessels be in the least degree greater than that of the water in the other vessel. Although the changes in density are very minute near the maximum point, the extreme mobility of fluids led us to expect that we might in this way arrive at an exact and incontrovertible result.

The *thermometers* employed by us were of extreme accuracy, having been calibrated throughout their whole length, and their delicacy was such as to indicate a change of temperature considerably less than  $\frac{1}{100}$ th of a degree Fahrenheit.



The freezing-points of the thermometers were carefully determined within a few hours of the experiments. Each vessel was furnished with a *stirrer*, consisting of a disc of tinned iron 4 inches in diameter attached to the end of a slender rod of iron, by means of which the water was thoroughly stirred before each determination of temperature.

In order to measure the motion of the water in the trough connecting the tops of the vessels, a hollow glass ball of about three-eighths of an inch diameter was placed in it. The weight of this glass ball was carefully adjusted so as only just to float: a matter of great importance, as the slightest buoyancy is accompanied by a certain degree of capillary attraction, and makes the ball liable to adhere to the sides of the trough.

The *water* employed in the experiments was distilled by ourselves in clean vessels of tinned iron; and the additional precaution was taken to prevent, as far as possible, the solution of air.

Our method of experimenting was as follows:—Having filled the vessel with distilled water at a temperature of about  $37^{\circ}$ , we increased the temperature of one of the vessels to  $41^{\circ}5$  by the addition of a small quantity of hot distilled water. We then placed two delicate thermometers upon a proper stand, so that their bulbs dipped in the water to the depth of 6 inches. Having then closed the stop-cock and adjusted the slide, we stirred the water in each vessel thoroughly, and noted the temperatures indicated by the thermometers. The stop-cock was then opened and the slide carefully removed from the trough. After waiting three minutes the glass ball was put into the trough, and its motion watched for two or three minutes with the help of a graduated rule placed at the top of the trough. In conclusion, the stop-cock was again turned, the slide readjusted, the water stirred, and the temperatures again noted. The mean of the temperatures thus observed before and after each trial of the velocity of the current was taken as the temperature of the observation.

The following table contains the results of a series of observations taken in the above manner. The temperature of the laboratory being about  $38^{\circ}$ , the water in the warmer vessel cooled down more rapidly than the water in the other vessel increased in temperature; and therefore after two or three hours had elapsed, the water in the cooler vessel was found to have acquired greater buoyancy than that in the warmer vessel, although at the commencement of the experiments the current indicated a greater degree of buoyancy in the warmer water.



Series 1.

Temperature of the water in the warmer vessel.	Temperature of the water in the cooler vessel.	Mean of the temperatures of the two vessels.	Velocity of the current in inches per hour.
41°183	37°348	39°265	280 from the warmer vessel
41°129	37°368	39°248	240 ditto
40°959	37°363	39°161	20 ditto
40°905	37°368	39°136	8 ditto
40°711	37°317	39°014	40 from the cooler vessel

We could hardly have anticipated more satisfactory results than those of the above table. They show clearly that while water at a temperature of 40°·905 is lighter than water at a temperature of 37°·368, water at 40°·711 is heavier than water at 37°·317: in other words, that 39°·136 is above, whilst 39°·014 is below the maximum point. By drawing a curve from the results, we find that the exact point of maximum density indicated by the above series of observations is 39°·102.

During the next series of observations the temperature of the laboratory was about 41°, which occasioned a gradual increase of the temperatures of both vessels.

Series 2.

Temperature of the water in the warmer vessel.	Temperature of the water in the cooler vessel.	Mean of the temperatures of the two vessels.	Velocity of the current in inches per hour.
40°742	37°368	39°055	22 from the cooler vessel
40°758	37°420	39°089	8 from the warmer vessel
40°773	37°470	39°121	60 ditto

The point of maximum density indicated by this second series of observations is 39°·078.

Series 3.

Temperature of the water in the warmer vessel.	Temperature of the water in the cooler vessel.	Mean of the temperatures of the two vessels.	Velocity of the current in inches per hour.
40°332	37°633	38°982	70 from the cooler vessel
40°402	37°682	39°042	80 ditto
40°425	37°709	39°067	60 ditto
40°440	37°745	39°092	8 ditto
40°448	37°791	39°120	30 ditto
40°467	37°837	39°152	12 from the warmer vessel
40°483	37°873	39°178	30 ditto

The point of maximum density indicated by the above series will be situated at about  $39^{\circ}134$ .

We now proceeded to apply a severer test to our method. In the next series of experiments we arranged matters so that the temperature of the water in one vessel was only a degree and a half higher than that of the other vessel. The expansion of water increasing as the square of the temperature from that of the maximum density, it was obvious that the current in the trough would be much more feeble than in the former experiments. We therefore allowed the vessels to be in perfect repose for six minutes before we introduced the glass ball, and we afterwards watched its motion for four or five minutes.

#### Series 4.

Temperature of the water in the warmer vessel.	Temperature of the water in the cooler vessel.	Mean of the temperatures of the two vessels.	Velocity of the current in inches per hour.
39.921	38.382	39.151	30 from the warmer vessel
39.864	38.398	39.131]	0
39.821	38.362	39.091	0
39.782	38.332	39.057	2½ from the cooler vessel.

The position of the point of maximum density according to the above fourth series of experiments will be at  $39^{\circ}091$ .

I.	Point of maximum density of pure water	39.102
II.	...	...
III.	...	...
IV.	...	...
	Mean	39.101

Although in the different series of observations there are several irregular results, there is on the whole sufficient consistency among them to enable us to receive  $39^{\circ}1$ , the mean of the four sets of observations, as the actual point of maximum density. We think it highly probable that this temperature is within one-hundredth of a degree of the truth: it certainly cannot be more than one-twentieth of a degree in error. We were prevented by the mildness of the season from extending the experiments further, but we doubt not that by repeating them more frequently we should be able to bring the determination of the point to any required degree of accuracy. The result arrived at by Despretz from a very extensive series of experiments, with an apparatus similar to that employed by Hope, is  $39^{\circ}176$  \*, which agrees very well

\* *Annales de Chimie*, 1839, t. lxx. p. 45.

with our determination. But other results, such as those of Hallström  $39^{\circ}38$ , Blagden and Gilpin  $39^{\circ}$ , Hope  $39^{\circ}5$ , Deluc  $41^{\circ}$ , Lefebvre Gineau  $40^{\circ}$ , Dalton  $38^{\circ}$ , Rumford  $38^{\circ}8$ , Muncke  $38^{\circ}804$ , Stampfer  $38^{\circ}75$ , &c., show by their discordance with one another, and their disagreement with our result, the little dependence which can in general be placed on the results of former methods.

We believe that our new method may be applied with great advantage to a variety of interesting problems. One of the most important of these applications is the determination of the dilatation of glass bulbs by heat, which, though formerly presenting great practical difficulties, can now be accomplished in the most simple and decisive manner. The bulb has only to be filled with pure water and reduced successively to two temperatures, one as much above as the other is below the point of maximum density—the rise of the liquid in the stem of course indicates the contraction of the glass in passing from the higher to the lower temperature. The expansion of the glass bulbs being thus accurately ascertained, they may be advantageously applied in determining the dilatations of solutions and other liquids.

## XII. On a peculiar Organ found in the Rays (Raia, Cuv.).

By M. LE DR. CH. ROBIN\*.

**T**HERE exists upon each side of the tail of the Rays an organ which is not mentioned in any of the works which I have hitherto been able to consult. This apparatus however deserves, on more than one account, to attract the attention of physiologists, and probably of physicists also.

The two organs united form nearly the third of the entire bulk of the tail of the Rays. The bulk of each, at its largest part, is in one of these fishes of an average size, nearly that of the index-finger. Their origin is towards the union of the first and second quarter of the caudal appendage of the Rays, and they terminate in a point at the extremity of the latter. Their anterior extremity is soft and more or less slender, according to different individuals: it swells gradually as far as the middle of the tail of these fishes; the volume remains the same as far as the origin of the posterior quarter, whence it diminishes finally to the end. This organ is at first almost cylindrical, though a little flattened on the sides (to about the extent of the anterior quarter); in the whole of that part it is enveloped by several thin and concentric muscular layers. These muscles soon terminate in as many aponeurotic layers; the organ then becomes subcutaneous, and at the same time its form changes, becoming round externally and flattened

\* From the Annals and Magazine of Natural History for January 1847.

within. In a word, this organ, separated from the tissues to which it adheres, presents the form of an elongated fusiform body, swollen in the centre, more or less blunt at the extremity, and always flattened upon its internal surface.

In a Ray one metre\* long, the tail was 49, and the organ 36 centimetres in length; 11 millimetres in the transversal direction, and 13 from above downwards.

The tissue of this organ has the semitransparency of gelatine, but more consistence, and its colour is a clear translucent gray. It is furnished with a general fibrous envelope, which adheres to the adjoining tissues by aponcurotic membranous layers ranged at regular distances.

I have already said that its anterior portion was completely surrounded by some concentric muscular layers, and then that it became subcutaneous in its three posterior quarters, for the greatest part of its surface. I add, by way of more detail, that its internal surface alone is not subcutaneous, and is separated from the vertebral column by the two long muscular and tendinous bundles intended to move the caudal vertebræ. Its upper margin is traversed by a large subcutaneous vein, a branch of the lateral vessel; its external surface is traversed by the lateral vessel itself, which is accompanied by the lateral nerve. This nerve is situated between the organ and the skin, throughout the whole length of the subcutaneous portion.

After these details on the relations of this apparatus with the adjacent organs,—relations, moreover, common to all the species of Rays,—I proceed to make known the *texture* of the tissue peculiar to this organ and the distribution of its vessels and its nerves. For this purpose I shall adopt a comparative course, that is to say, at each step I shall refer to the relations of this organ with those which most resemble it in other fishes.

On examining attentively the apparatus in question, we observe that its substance does not constitute an uniform gelatinous mass, but that it is divided into a large number of polygonal flattened discs by the partitions of cellular tissue. These discs have consequently two surfaces larger than the rest, one turned forwards, the other backwards. With respect to the faces of the circumference, they are in number three, four or five, which gives the discs a triangular, tetragonal or pentagonal form; their smallest diameter measures the thickness of the disc, which is 1 millimetre in nearly all the species. The diameter of the large surfaces, which measures the height of the disc, is 2 mill., one more in the *Raia rubus* and *R. batis*, and 3 to 4 mill. in the *Raia clavata*.

From this difference it follows, in the greatest diameter of the

\* The metre is equivalent to 39·37 English inches.



discs according to the species, that in a transverse section of the organ, from eleven to fifteen discs are counted in a *R. clavata*, and twenty to twenty-five in the *R. rubus* and the *R. batis*. The volume of the discs increases with age and the size of the individuals, but their number does not appear to increase.

These gelatinous discs are piled up one upon another, in the direction of the length of the apparatus, by the adherence of their broad faces, with the interposition nevertheless of a thin cellular partition. These longitudinal rows of discs are arranged side by side, reunited by a cellular partition thicker than that which separates each disc from that which follows or precedes it. The kinds of longitudinal columns represented by the piled-up discs are not rectilinear and do not all follow the length of one of the faces of the apparatus; but they are more or less contorted, and are interrupted at intervals. The interruptions proceed from the discs becoming at intervals irregular, more narrow, and the series of discs terminates ordinarily in a very small, triangular one. It follows from these anatomical arrangements, that on the surface of the organ we may perceive one of the small faces of each of the superficial discs, and study very regularly the elongated, quadrilateral or lozenge-shaped polygonal, sometimes hexagonal form which it assumes in consequence of the reciprocal pressure which it undergoes from the adjoining discs. We may moreover very easily perceive that when the discs of a series begin to lose their form and are interrupted, there exist at the side other irregular discs which commence a new series. It is to be remarked also that the discs are ranged more regularly on the internal surface of the apparatus in the *Raia rubus* and *R. batis* than in the *Raia clavata*; in the first two species we also observe on the internal surface of the organ, that one of the partitions which separates the series of discs on the internal surface, follows its whole length and is of greater thickness than the rest: it is formed by glistening aponeurotic fibres, and it forms a sort of longitudinal pile into which the vessels and the nerves penetrate.

With respect to the gelatinous substance of the discs, magnified 400 diameters, it is seen to be hollowed out by cavities, and the walls of the latter are hollowed by cavities gradually lessening in size. The substance too which circumscribes these areolæ (to which we shall recur hereafter) is hyaline, homogeneous and transparent; it is studded with extremely fine molecular granules. From one spot to another are very regular granular spheres of 0<sup>mill</sup>.0050, surrounded by a very pale circular mass of granules similar to the preceding. It is impossible to recognise veritable cellules with walls and nuclei, and it is easy to see that the preceding areolæ are not cellules; we shall soon speak of their uses. On the margins of the discs, the homogeneous gela-



tinous substance presents regularly undulated striæ which it would be impossible to take for fibres.

At the point at which we are arrived, it is impossible not to recognise a great analogy between the semitransparent gelatinous substance which essentially constitutes the discs of the peculiar organ of the Rays, and that of the prisms of the apparatus of the Torpedo, the rhomboidal meshes of the *Silurus electricus*, and those interrupted ones between the transversal and vertical fibrous laminae of the *Gymnotus*.

Although there may be differences in form between the discs of the organ of the tail of the Rays and those which constitute the prisms of the electrical apparatus of the Torpedo, these differences are certainly less considerable than those of the portions of gelatinous substance circumscribed by the partitions and areolæ of the apparatus in the *Silurus* and *Gymnotus*, which however produce similar effects to those of the Torpedo.

The mode of arrangement of these discs is as regular in the Ray as in the Torpedo, and approximate much nearer to the latter than to that of the same parts in the apparatus of the *Silurus* and *Gymnotus*.

The nerves of this apparatus originate in the portion of the spinal marrow which is prolonged into the caudal vertebræ. I have an object in view in remarking that this portion of the spinal marrow must be composed of sensitive and motive nervous fibres, for it corresponds to the portion called *cauda equina* in the higher animals.

The nervous roots which originate from this organ do not take their rise together at the same level, but there springs alternately an anterior and a posterior root. It is always from the anterior one (before its anastomosis with the posterior) that the greatest number of nerves which exist in the apparatus proceed; lastly, some issue from the ganglion and the lowest branch of the two which proceed from it. These nerves are of the number of four to seven for each nervous pair. They are, as is seen, very numerous, and their diameter is from  $\frac{1}{3}$  to  $\frac{1}{2}$  millimetre. These nerves are finally distributed in the thickness of the partitions which separate the lateral muscles from the tail, when they penetrate into the organ, after being more or less subdivided. In the *Raia rubus* and *R. batis* the greatest number penetrate into the longitudinal pile of the internal surface; in the *Raia clavata* they penetrate into some one of the partitions of that surface. In these three species several branches wind round the superior and inferior margins of the apparatus to penetrate into one of the partitions of its subcutaneous portion. In the first two species these superficial branches freely anastomose before penetrating.

It results from these facts that a considerable number of nerves

extend into the partitions of each series of discs infinitely subdividing. From these subdivisions part the filaments which penetrate between the partition which separates each disc from that with which it is in contact. This filament expands opposite to the anterior face of each disc, but never does a single one penetrate into the substance of the disc. The nerves spread out on the internal surface of the partition between it and the disc. No single thread ever ramifies against the posterior face of the disc; we shall soon see that this surface receives only vessels.

The elementary fibres of the nervous filaments have a double character; that is, they are true elementary nervous tubes traversed by a semifluid substance which escapes in drops of variable forms from their extremities when torn across. [These observations however would require to be verified upon animals fresh captured.]

The elementary tubes which spread out against the prism are from 0<sup>m</sup>·01 to 0·013, that is to say, half the diameter of the elementary tubes measured on the nerves at the point of their penetration into the apparatus. The elementary nervous tubes do not terminate in a net-work, but actually in very large meshes, to effect which they fork out several times into two or three branches and anastomose by inosculation.

These facts rest on the clearest evidence, being easily proved even with a magnifying power of 100 diameters. The semifluid nervous substance contained in these elementary tubes may be made to flow out, and be seen to penetrate into each of their subdivisions and anastomoses. These anastomotic terminations of the elementary nervous tubes have already been proved to exist by Savi, in his "Anatomical Investigation of the Torpedo (1844)." He has also proved this fact in the partitions which separate the discs of gelatinous substance of the electric apparatus of this fish.

The last facts which I have just established exhibit a still greater analogy between the organ in question and the apparatus of the electric fishes. It is true that these nerves proceed from the termination of the spinal marrow, that is to say, from the *cauda equina*, but the same fact takes place in the *Gymnotus*, the most potent in its discharges of the electrical fishes, whose electrical organs however, according to Hunter, do not receive a mass of nerves proportionably so considerable as those of the Torpedo. In the Ray, as in the *Gymnotus*, the mass of the nerves sent to the electric apparatus by each nervous pair, is at least as considerable as those which they transmit to the skin and the muscles. The lateral nerve does not in the Ray, any more than in the *Gymnotus*, send any filament to the organ in question.

The nerves of the electrical apparatus of the *Silurus*, examined

by Geoffroy St. Hilaire, M. Valenciennes, Rudolphi and Peters, proceed from the lateral nerve, a branch of the eighth pair.

Thus there is nothing constant in the origin of the nerves of the electrical apparatus, as they proceed sometimes from the eighth and ninth pair (*Torpedo*), sometimes from the eighth pair alone (*Silurus*), sometimes from the pairs which arise from the spinal marrow (*Gymnotus* and *Raia*). Their situation has also no constancy, as they are sometimes situated towards the head (*Torpedo*), around the body (*Silurus*), and on each side of the tail (*Gymnotus* and *Raia*).

The vessels of this organ are numerous and curiously arranged. Between the articulation of each vertebra there passes a vessel, alternately an artery and a vein, proceeding from the principal artery and vein of the tail. These two vessels never pass together to reach their foramina, and they never traverse the inferior spinal apophyses, like the nerves, to issue from the spinal canal. Beyond the vertebræ, the vessels follow the course of the nerves, and penetrate with them into the apparatus. Several branches ramify on its surface, surround it with their anastomoses, and from the plexus which they form, some branches are detached, destined either for the skin or the adjoining muscles.

Those vessels which penetrate the thickness of the organ are there subdivided infinitely in the partitions of connective tissue which separate the discs from one another.

From the plexus formed by the arterial and venous ramifications capillaries are given off, which are directed towards the posterior face of the disc which is in front of them, and penetrate into its substance. A capillary vessel never penetrates into the anterior surface of a prism; but we have stated that the nerves ramify opposite to or against that surface. The capillaries which penetrate the discs are very elegantly arranged in flexuous loops, and are sometimes agglomerated in the form of tufts. These loops and tufts are lodged and buried in the cavities by which the disc is hollowed out; these excavations exist only on the posterior face of the discs, whilst the anterior face against which the nerves are arranged is smooth. The capillaries which are buried in the discs are from  $\frac{1}{10}$  to  $\frac{1}{23}$  of a millimetre in diameter.

To sum up the matter, there exists in the Rays a pretty voluminous organ, situated in the tail of that fish, as in that of the *Gymnotus*. [From a letter which I have received from Prof. J. Müller, Rüppell would appear to have described an organ analogous to the former in the tail of the fishes of the genus *Mormyrus*.] This organ of the Rays receives fine but very numerous nerves. It is formed of a gelatinous semitransparent and firm matter, as in all the electrical organs known. This substance is,

as in all these fishes, divided into polygonal discs, regularly piled together, against which nerves ramify that terminate by successive bifurcations and anastomoses supplied from their elementary fibres. How can we help seeing in this an electrical apparatus? It is true that its position is not the same as in the *Torpedo*, but in the *Gymnotus* and the *Silurus* the organ is also situated in the tail or around the body. These conclusions are further confirmed by the following facts: I have proved that this apparatus is wanting in the tail of the *Torpedo* and the genera *Mustelus*, *Scyllium*, *Squatina*, *Zygæna*, *Acanthias* and *Carcharias*.

The presence of this apparatus in the tail of the Rays explains the immoderate proportional length of this organ, its flattened form beneath, and the absence of the inferior lobe on the caudal fin, which scarcely exists in the Rays. The anal fin is also wanting in the Rays, it is also wanting in the *Torpedos*; but all these fishes have a complete caudal fin, whereas it is wanting in the Rays, as I have just observed.

I am indebted to the kindness of M. Bibron for being enabled to ascertain that the other fishes allied to the Rays (*Cephaloptera*, *Myliobates* and *Pastinacus*) whose tail is terminated by a thin and extended whip or prolongation, do not possess this apparatus. The whip is formed of a portion of the tail, which the electrical apparatus would occupy if it existed.

As we have just seen, this organ cannot be regarded as a gland, for it has not the structure of one; it does not possess an excretory duct, it does not communicate in any part with the inside, and no gland receives so many nerves of animal life\*.

### XIII. Notices respecting New Books.

*The Physical Atlas; a series of Maps illustrating the Geographical Distribution of Natural Phænomena.* By H. BERGHAUS, LL.D., F.R.G.S. &c., and A. K. JOHNSTON, F.R.G.S. &c.

ALTHOUGH the idea embodied in the publication before us is not here met with for the first time, yet it will probably have all the attraction of novelty for the general public, since we believe that the work on which it is founded is not very extensively known in this country. To those who know the *Physische Atlas* of Dr. Berghaus, it will be a great gratification to find that the design has been appreciated by the able editor of the *National Atlas*, and that he has succeeded in obtaining the assistance, not only of Dr. Berghaus, but of some of the most distinguished followers of the sciences,

\* Nevertheless the proof of its being an electrical organ must depend upon its power of giving electric shocks. Such a property, in our common Rays, if it existed, could hardly have escaped the notice of fishermen, in the constant habit of handling large Rays, Skates and Thornbacks immediately after their capture.—Ed.



the results of which it comprehends, in the endeavour to produce a British Physical Atlas the execution of which shall be worthy of the grand generalizations on which it is based.

The advantages which attend this mode of conveying information it is scarcely necessary to enlarge upon ; it has all the simplicity of a tabular arrangement, without that bare abstract quality which leaves to the mind no connecting links, no means of association, and demands a distinct effort of memory for every fact ; while with the majority of persons the pictorial form of a map makes a peculiar kind of impression, which can be reproduced at pleasure in the consciousness, and becomes a ground on which the several facts may be retraced. The good effect that must result from the more accurate knowledge thus attainable by the "uninitiated" of the vast extent of the complex details which are necessary to the evolution of the simplest generalizations, will be at once admitted on looking abroad upon the crude speculations which are so hastily grasped in the daily increasing desire for knowledge of the deeper mysteries of nature. A man who has carefully and patiently studied these maps and pondered over the multitude of "experimental truths" on which the simplest of them depends, will probably pause ere he accepts cosmical theories which find their materials in popular "abstracts." Not that we undervalue the speculative faculty ; but it is evident that the department of observation and experiment is that to which those inquirers should restrict themselves, whose opportunities or inclinations prevent their acquiring a comprehensive knowledge of the subjects by which they are attracted.

To such persons this Atlas will be exceedingly valuable, as indicating the precise state of our information in regard to the facts, and thus pointing out to them where their labours may be most usefully directed. To the general student it will convey clearly and agreeably a mass of that knowledge which is daily becoming more necessary to him. To the rising generation, in whose education these subjects cannot well be neglected, where any pretension is made to keep pace with the intellectual progress of the times, these maps will throw new and powerful interest into the study of geography, leading not merely to the knowledge of relative position and political or other artificial divisions, but to an appreciation of the natural relations, regarded either in a scientific or œconomic point of view, of the various regions to each other, and a comprehension of the grand unity of this our terrestrial world, where each part is, as it were, complementary to all the rest.

In the execution of this design the various subjects are arranged under the two general heads of the phænomena of Inorganic and Organic nature ; the former comprehending three subdivisions,—1. Meteorology and Magnetism ; 2. Hydrology, and 3. Geology ; the latter nominally two, viz. Phytology and Zoology ; but apparently under the latter head we must include Ethnology, since among the maps already published we find an Ethnographic map of the British Islands.

Five parts of the work are now before us, which, according to the



prospectus, form half of the contemplated series. Each part contains three maps; and as these are not published in any regular arrangement, they are sufficiently varied to give a good idea of the intentions of the editors. The following is a summary view of the published maps.

Part I.—1. A Physical Chart of the Atlantic Ocean; 2. a map of the Mountain Systems of Europe; 3. map and elevations illustrating the Distribution of Plants. In the first will be found, in addition to the usual peculiarities of charts, such as distances, currents, &c., the temperature at different points, the situation and form of Fucus banks and the appearances of volcanic action, with the existing statistics of icebergs and doubtful islands, and the tracks commonly followed by navigators. The various phænomena are briefly described in the accompanying letter-press, as much as possible of the information being tabulated. The most striking feature of the second map is the application of the system of contour lines, without which indeed all plane representations of elevated tracts are but very vague. In the third map are embodied Schouw's phyto-geographic labours, together with statistical information of Humboldt and other travellers.

Part II.—1. The distribution of part of the Mammalia; 2. a Hyetographic map of the world; 3. the River systems of Europe and Asia. The first includes a view of the range of four mammiferous families, viz. the Quadrumana, Marsupialia, Edentata and Pachydermata; the types of these families being represented pictorially for the benefit of those unacquainted with scientific classification. The Hyetographic or Rain map is an attempt to indicate, by various degrees of shading, the relative amount of rain falling in different districts; it is accompanied by a quantity of statistical tables of great value. This map has a peculiarly novel appearance; and the contrasts obtained by means of the shading are very striking; the great deserts and the rainless districts of South America, so graphically described by Mr. Darwin in his journal, standing out strangely in their whiteness from the dark regions near the equator. The River systems are arranged so as to display the great basins of the two continents and course of the rivers, thus forming a kind of supplement to the view of the mountain chains. The description includes hydrographic tables, &c.

Part III.—1. Glaciers and glacial phænomena; 2. distribution of Carnivora; 3. Physical Chart of the Pacific. The first map has been compiled from the researches of Forbes, Raymond, Charpentier, Weiss, &c. This is accompanied by a descriptive treatise by Prof. J. D. Forbes, giving a detailed account of the European glaciers, founded on his own extensive researches. Those of South America, described by Captain King and Mr. Darwin, are also noticed, and the glaciers of Sir James Ross's great south polar continent. In addition to the general distribution of the Carnivora, the second sheet contains a map of the regions of the northern whale and seal fisheries, and those inhabited by fur-bearing animals. The Chart of the Pacific resembles that of the Atlantic, containing of course many peculiar points of interest.

Part IV.—1. Phænomena of Volcanic action ; 2. Rain map of Europe ; 3. distribution of Reptiles. The first of these is enriched by a collection of small but very important special maps of the most celebrated volcanic districts. On the general chart are given the lines of direction of the most important earthquakes of which we have records, and a view of the various volcanic regions. Among the small maps are Lower Italy and the Greek islands, according to Von Buch ; the Sandwich Islands, &c., by Fitzroy ; Iceland after Krug von Nidda, &c. The accompanying table gives the various statistics of height, date of eruption, &c. of all the volcanoes of which these particulars have been obtained. In the Hyetographic map the points receiving equal annual amounts of rain are united by a system of curved lines called Isohyetoses, analogous to the isothermal lines of Humboldt ; also isotherombroses or curves indicating the distribution of summer rains ; the annual number of rainy or snowy days, depth of fall, &c., and the direction of the rain winds. In this map Europe is divided into three regions :—the province of winter rains, comprehending part of the southern portion ; of autumn rains, including the remainder of the southern and western portions ; the summer rains belong to the whole interior of the continent. The limits of these regions are of course only approximative. In the distribution of reptiles there are separate sections for the Testudines, Sauria and Batrachia ; the Ophidians have two, one for the innocuous species, and another for the venomous.

Part V.—1. Distribution of Birds ; 2. mountain chains of North America ; 3. Ethnographic map of Great Britain and Ireland. The distribution of Birds includes a general map of the world divided into sixteen provinces, and a map for Europe regarded as a single province. The second map contains, besides the mountain chains of the North American continent, a map of the Island of Trinidad and Humboldt's plan of the volcano of Jorulla. The Ethnographic map is the result of an endeavour to lay down, as far as the present condition of our knowledge will admit, the distribution of the various races by which our islands have been peopled. This is an attempt of acknowledged difficulty, since in a country which has attained a high degree of civilization, the intermixture of the elements which must have taken place necessarily leads to great uncertainty. The general conclusions to which the author of the map (the late Dr. Gustav Kohn) had arrived are as follows : there are five sub-varieties of the Celtic variety, and eight of the Teutonic ; and the following is a probable average calculation of the ethnographic blood of the population of Great Britain and Ireland.

#### I. Pure Blood.

1. Teutonic in England, Scotland, east and north-east of Ireland . . . . .	10,000,000
2. Celtic in Cornwall, Wales, Scottish Highlands and Ireland . . . . .	6,000,000
	<hr/>
	16,000,000

II. Mixed Blood.

1. Teutonic (that is with prevalent Teutonic character) in England, Scotland, east and north-east of Ireland.....	6,000,000
2. Celtic (with prevalent Celtic character) in Cornwall, Wales, Highlands of Scotland and Ireland	4,000,000
	<u>10,000,000</u>
Total Teutonic, pure and mixed.....	16,000,000
Total Celtic, pure and mixed.....	10,000,000
	<u>26,000,000</u>

The Physical Atlas contains all the elements of success, high interest in the subject matter, as we have shown, and this brought forward in a manner well-worthy of it. The size of the maps is imperial folio, each of the maps occupying a sheet; the engraving and colouring are beyond all praise for their beauty and clearness. The enterprize which has produced this work at such a moderate price calls for extensive patronage, and the care of its execution fully deserves it. We feel bound, both in justice to the editors and our readers, to give it our warmest recommendations.

*Introduction to Zoology for the Use of Schools.* By R. PATTERSON, Vice-Pres. of the Nat. Hist. Soc. of Belfast. 12mo. London 1846.

We cordially recommend to the notice of our readers this valuable little work by Mr. Patterson, who is well known to have long and energetically endeavoured to introduce the study of natural history into the ordinary course of general education. His great attainments as a naturalist well qualify him for the task which he has undertaken; and we feel sure that the publication of this volume will greatly tend to promote his highly desirable object.

In the present publication, which is profusely illustrated by wood engravings, he treats of the Invertebrate animals, but proposes to continue the subject in a future volume on the Vertebrata. We have here in small space a very complete introduction to the study of the interesting tribes upon which it treats detailed in simple and popular language, so as to be intelligible to persons who have not made natural history a study; but at the same time there is nothing in it of what is vulgarly called "popular science," every part being of a truly scientific character. It is well fitted to be placed in the hands of the young, and highly deserving of perusal by the accomplished naturalist.

We wish him every success in his laudable endeavour.

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XIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xxix. p. 409.]

Nov 19, "ON the Automatic Registration of Magnetometers, 1846. and other Meteorological Instruments, by Photogra-

phy." By Charles Brooke, M.B., F.R.C.S.E. Communicated by G. B. Airy, Esq., F.R.S., Astronomer Royal.

The author enters into fuller details than he had done in his former communication to the Society, which was read on the 18th of June, respecting the construction of the instrument, the preparation of the highly sensitive photographic paper employed in the process, and the minute adjustments necessary for ensuring accuracy in registering the results.

In a supplement to the above paper, the author describes the methods he has contrived for obtaining a similar automatic registration of the heights of the barometer and thermometer, by suitable additions to the same apparatus which registers the magnetic variations.

W. R. Grove, Esq., M.A., F.R.S., delivered the Bakerian Lecture—"On certain Phenomena of Voltaic Ignition, and on the Decomposition of Water into its constituent Gases by heat."

The author refers to an eudiometer, an account of which was published by him in the 'Philosophical Magazine' for 1840, formed of a glass tube, into the closed extremity of which a loop of platinum wire was sealed. The gases to be analysed were mixed in this tube with a given volume of oxygen and hydrogen, and detonated or slowly combined by the voltaic ignition of the platinum wire. He was thence led to try a further set of experiments on the analysis, by this instrument, of such gases and vapours as are decomposable by heat; the process being capable of much greater exactness than the received one of passing them through ignited tubes. The results of the analyses of several gases by this means are given in the paper. When carbonic acid and hydrogen are mixed in equal volumes and exposed to the ignited wire, the hydrogen abstracts oxygen from the carbonic acid, and leaves carbonic oxide. Conversely, when carbonic oxide is exposed over water to the ignited wire, it abstracts oxygen from the aqueous vapour, and forms carbonic acid.

It thus appeared, that provided there were bodies present capable of absorbing by affinity the elements of water, ignited platinum would either compose or decompose water. The author was thence led to hope that he might by ignited platinum decompose water into its constituents, without absorption by other bodies, and thus produce converse effects to those already known. In this he ultimately succeeded by various methods, in some of which the ignition was produced by electrical means; in others by ordinary calorific processes, such as the oxyhydrogen blowpipe, &c.

A platinum wire is ignited at the closed extremity of a species of tube retort, full of pure water, and having a narrowed neck close above the wire; as soon as the wire becomes incandescent, it forms around itself an atmosphere of vapour which it immediately decomposes; a natural valve being formed by the conflict of ascending gas and descending water, the bubbles of mixed gas are cut off by an intermittent action, and thus, as their recombination is prevented, a volume of gas collects in the bend of the tube and is ultimately



expelled at its orifice. If, again, a button of platinum be fully ignited by the oxyhydrogen blowpipe, and plunged into water previously heated to nearly its boiling-point, bubbles of mixed gas ascend and may be collected by an inverted tube. The electrical spark is shown to be capable of decomposing aqueous vapour, and various other modes of producing the same results are given.

Some theoretical views are then advanced as to the spheroidal state which appears to the author to be intermediate between that of ebullition and decomposition; as to the probable non-existence of water or steam in the interior of the earth, and as to the antagonism between physical repulsion and chemical affinity.

In a supplementary paper, the author considers how far catalysis affects the phenomenon, and regards the decomposition thus produced as presenting a parallel effect produced by the force of heat, to that known to be produced by electricity; he considers it explanatory of the decomposition of water by the electrical spark as in the experiments of Pearson and Wollaston. Some further experiments are given, in which iridium and osmium and silica are substituted for platinum; and also some experiments on the liquids bromine and chloride of iodine, both of which yield pure oxygen when exposed to the ignited wire in Mr. Grove's apparatus. These last experiments cannot however be long continued in consequence of these liquids ultimately attacking both the glass and the platinum. In conclusion, the author calls attention to the general evolution of permanent gas from all liquids, except the metals, when exposed to intense heat.

Dec. 17.—“*Researches on Physical Geology.*”—Part I. The Figure and Primitive Formation of the Earth. By Henry Hennessy, Esq. Communicated by Major North Ludlow Beamish, K.H., F.R.S.

The author's investigations of the figure of the earth proceed on the hypothesis of its having originally been a heterogeneous fluid mass, possessing only such general properties as those which have been established for fluids; and independently of the supposition, with which the theory has generally been complicated, that the volume of the entire mass, and the law of the density of the fluid, have suffered no change in consequence of the solidification of a part of that fluid. Assuming the figure of the mass to be an ellipsoid of revolution, the author obtains general analytical expressions for its ellipticity, and for the variation of gravity at its surface. He gives a general sketch of the consequences that may result from the improved hypothesis of the primitive figure of the earth, to physical geology, that is, to the changes occurring upon the external crust of the earth during the process of its solidification, resulting both from calorific and chemical changes taking place among its different parts, and giving rise to a process of circulation throughout the fluid portions of the mass.

The present memoir is only the first of a series which the author announces it is his intention to communicate to the Society on the same subject.

## ROYAL ASTRONOMICAL SOCIETY.

[Continued from vol. xxix. p. 232.]

November 13, 1846.—*Elements of Astræa.*

Mr. Graham, of Mr. Cooper's Observatory at Markree, has deduced the following elements, omitting any consideration of disturbing forces :—

1846. Jan. 1. Mean Anom. ....	318° 55' 21.5"	} Mean Equinox, 1846.0.
$\pi - \Omega$ .....	353 55 49.2	
$\Omega$ .....	141 24 43.8	
$i$ .....	5 19 22.1	
$\phi$ .....	10 45 57.7	
Log. $a$ .....	0.4107004	
Log. $n''$ .....	2.9339560	
$n''$ .....	858".9265	
Sid. Rev. 1509 days.		

The places from which these elements are deduced, are,—

	Greenwich M. T.	R.A.	Dec.
1845. Dec.	17.460393	63° 22' 21.62"	+12° 40' 18.29"
1846. Feb.	8.053359	62 37 24.46	15 40 31.99
March	27.310391	77 59 41.13	+19 57 8.40

The first place is a mean of ten continental observations; the second is a mean of four meridian observations by Mr. Cooper, at Markree; the third, a single observation at Pulkawa.

*Elliptical Elements of De Vico's Fourth Comet (Feb. 20, 1846),  
by Mr. Hind.*

The following orbit is based upon the Altona and Hamburg Observations of March 12, and Hamburg, March 31 and April 18. There appears no doubt that the elements are really elliptical :—

Perihelion Passage, 1846, March 5.57777, Greenwich Mean Time.

Longitude of Perihelion .....	90° 34' 45.79"	} Mean Equinox, 1846.0.
Longitude of Ascending Node.....	77 35 35.90	
Inclination .....	84 57 12.61	
Angle of Eccentricity .....	72 37 42.99	
Log. semi-axis major .....	1.8062261	
Sidereal Revolution 55.4 years.		
Motion direct.		

*Note on the First Comet of Brorsen, by Mr. Hind.*

"The comet discovered by Mr. Brorsen at Kiel, on the 26th of February last, has been found to have an elliptical orbit with a period of  $5\frac{1}{2}$  years only. Independent calculations by Dr. Brünnow, M. Goujon, and myself, have led to nearly the same results as regards the short time of revolution. This circumstance induced me to examine the path of the comet through our system more minutely. Assuming the last set of elements calculated by Dr. Brünnow, and published in No. 557 of the *Astronomische Nach-*

*richten*, I soon found that a very close approach to the planet Jupiter must have occurred about the 20th of May 1842: the comet at this time would be hardly further distant from Jupiter than the mean distance of his fourth satellite. The action of the planet would be more than ten times greater than that of the Sun; and it is very possible that a complete change might be produced in the elements of the comet's orbit. This encounter took place rather to the south of the plane of the ecliptic, in about  $283\frac{1}{2}^{\circ}$  heliocentric longitude."

Peters's Comet (26th June 1846).

On the 26th of June, Dr. C. H. F. Peters, of the Observatory of Capodimonte, found a very small comet in the constellation Scorpius, near 595 Mayer, and about one degree to the south of the nebula H. vi. 19, or 535 of Smyth's Cycle. The new object was quite as pale as the nebula, and somewhat rounder. The motion was perceptible at the end of an hour, when Dr. Peters commenced his observations with the equatorial of Reichenbach. These were continued on following nights till stopped by moonlight. The following are the results corrected for refraction:—

1846.	Naples Sid. Time.			R.A.			Dec.			No. of Obs.
	h	m	s	°	'	"	°	'	"	
June 26	17	24	4.0	226	51	31.4	—21	38	42.4	3
27	16	19	3.0	227	18	53.9	20	57	19.9	7
28	17	2	18.0	227	49	20.2	20	14	19.8	4
29	16	48	45.6	228	19	3.8	19	33	4.3	6
30	17	53	10.3	228	48	48.3	18	50	36.0	5
July 1	18	24	25.7	229	19	15.9	18	10	37.4	5
11	16	57	24.8	234	5	53.5	12	31	31.1	2
12	17	19	34.1	234	33	39.8	12	2	18.7	4
13	17	18	29.1	235	2	16.3	11	34	45.7	4
14	17	6	56.8	235	29	45.7	11	8	19.7	4
15	17	39	10.5	235	59	18.8	10	40	10.3	3
16	17	19	26.3	236	26	47.1	10	16	1.4	5
17	17	31	47.6	236	55	9.8	9	50	33.5	4
18	17	43	22.4	237	26	5.3	9	27	23.5	4
19	17	41	14.2	237	52	22.4	9	3	53.1	4
21	17	51	58.3	238	47	28.0	—8	19	12.3	3

It was seen on the 23rd July, but it was too faint to be observed. Upon these observations Dr. Peters has calculated the following elements, taking into account all small corrections (*Parall. Aberr. Nutat.*).

Perihelion Passage 1846, May 30,  $12^h 56^m 3^s.0$ , Berlin M.T.

Perih. ....	237	20	28.2	} Mean Equinox, July 1.
$\Omega$ .....	258	45	12.6	
$i$ .....	34	0	41.7	
Log. $q$ .....	0.204635			
Motion direct.				

These elements differ considerably from a first rough sketch. Dr. Peters believes them to be near the truth; still they give from day to day considerable anomalies, when compared with the observations.

De Vico's *Fifth Comet* (July 29, 1846).

This comet was also detected by Mr. Hind, at the South Villa Observatory, on the night of July 29, 1846, about 11 o'clock, two hours after its first discovery by De Vico.

"The following positions are the results of instrumental comparisons at South Villa Observatory with known stars, not far distant from the comet; but owing to the extreme faintness of this object, the observations of August 4–15 inclusive, are very uncertain:—

	Greenwich M.T.	R.A.	Dec.
	h m s		
July 29	12 6 6	48° 53' 47"	+60° 37' 2"
30	10 17 35	48 5 34	60 42 33
31	9 56 19	47 12 6	60 48 10
Aug. 4	11 11 21	43 11 19	61 8 14
13	9 21 21	32 4 0	61 15 8
14	9 50 23	30 35 57	61 11 5
15	12 56 27	28 55 22	61 5 10
21	9 46 27	19 39 27	+60 2 8

"On July 29 and 30 we obtained some micrometrical measures with two small stars. On the 29th, at 13<sup>h</sup> 25<sup>m</sup> 25<sup>s</sup> Greenwich mean time, the comet followed a star of 8·9 magnitude, 2<sup>m</sup> 12<sup>s</sup>·33, and was south of it 16' 17"·8. This star is found in Argelander's Zones; and the apparent place for July 29, as given in Professor Schumacher's Comet Circular, was

R.A. 3<sup>h</sup> 13<sup>m</sup> 11<sup>s</sup>·86

Dec. +60° 53' 47"·0,

whence the comet's place was

R.A. 3<sup>h</sup> 15<sup>m</sup> 24<sup>s</sup>·19

Dec. +60° 37' 29"·2.

"On July 30, at 11<sup>h</sup> 23<sup>m</sup> 13<sup>s</sup> mean time, the comet followed a 9th magnitude star 1<sup>m</sup> 25<sup>s</sup>·07, and was south of it 45"·04; but we have not yet identified this star in any of our catalogues. On this night, with great attention, and an exact knowledge of the position, the comet was just perceptible in the comet-searcher."

*Elements of De Vico's Fifth Comet, by Mr. A. Graham of Markree Observatory.*

The observations employed were, one at Mr. Bishop's observatory, July 29; that of Paris, on August 18; and another made by Mr. Cooper on August 31:—

Perihelion Passage, 1846, May 27·82285, Greenwich Mean Time.

$\pi$ .....	82 39 19·7	} Mean Equinox, 1846·0.
$\Omega$ .....	161 18 29·1	
$i$ .....	57 36 24·2	
Log. $q$ .....	0·1382020	

Motion Retrograde.

The errors of the elements at the time of the middle observation were in geocentric longitude  $-1''\cdot4$ , and in latitude  $+4''\cdot1$ . The corrections for aberration and parallax were deduced from M. Goujon's elements. The constants for August 30th, referred to apparent equinox, are,—



$$\begin{aligned}x &= [9.98351] \cdot r \cdot \sin (358^{\circ} 55' 9'' + v) \\y &= [9.50659] \cdot r \cdot \sin (144^{\circ} 52' 34'' + v) \\z &= [9.99340] \cdot r \cdot \sin (86^{\circ} 5' 25'' + v).\end{aligned}$$

Mr. Cooper's observation on August 31 gave the following position:—

Greenwich M.T.	R.A.	Dec.
August 31.46583	$2^{\circ} 54' 2.0''$	$+55^{\circ} 37' 7.3''$

*De Vico's Sixth Comet (September 23, 1846).*

This faint telescopic comet was discovered by Father De Vico, in the constellation Ursa Major. M. Rümker has reobserved the stars of comparison, and computed the following places from the original observations:—

	Rome M.T. h m s	R.A. $^{\circ} ' ''$	Dec. $^{\circ} ' ''$
1846. Sept. 23.	8 6 36.5	128 21 44.5	$+64^{\circ} 14' 23.4''$
	10 25 34.9	128 48 29.5	$+64^{\circ} 14' 18.5''$

M. Wichmann rediscovered this comet at Königsberg.

	Königsberg M.T. h m s	R.A. $^{\circ} ' ''$	Dec. $^{\circ} ' ''$
1846. Oct. 15.	8 39 19	187 33 5.1	$+44^{\circ} 30' 23.3''$
16.	7 45 11	188 36 23.9	$+43^{\circ} 29' 6.2''$

The following observations were made at Altona and Hamburg:—

	Altona M.T. h m s	R.A. $^{\circ} ' ''$	Dec. $^{\circ} ' ''$	No. of Obs.
1846. Oct. 21.	6 42 19.7	193 11 40	$+38^{\circ} 20' 46''$	
	Hamburg M.T.			
21.	7 5 18.2	193 12 46.1	38 19 47.3	2
22.	16 20 50.1	194 17 34.3	36 57 52.6	12

The elements, as computed by M. Powalky at Altona, and by M. Niebour at Hamburg, prove the identity of the comet of Wichmann with De Vico's Sixth Comet.

*Elements of the Sixth Comet of De Vico (September 23, 1846),  
by Mr. Hind.*

The following orbit depends on the observations at Rome, September 23; Königsberg, October 15; and Altona and Hamburg, October 21. All the small corrections taken into account.

Time of Perihelion Passage, 1846, Oct. 29.74708, Greenwich Mean Time.

Longitude of Perihelion .....	$98^{\circ} 35' 49.9''$	True Equinox, 1846, Nov. 0.
Longitude of Ascending Node .....	$4^{\circ} 41' 3.7''$	
Inclination .....	$49^{\circ} 41' 17.1''$	
Log. distance in Perihelion .....	9.9193956	

Motion direct.

The co-ordinate constants for the apparent equinox at the beginning of November, are,—

$$\begin{aligned}
 x &= r \cdot [9.9991563] \cdot \sin(v + 186^\circ 56' 50'') \\
 y &= r \cdot [9.4735634] \cdot \sin(v + 108^\circ 29' 45'') \\
 z &= r \cdot [9.9807954] \cdot \sin(v + 95^\circ 51' 36'')
 \end{aligned}$$

*Hind's Comet (October 18, 1846).*

"A few minutes before 4 o'clock on the morning of October 19, a very faint telescopic comet was discovered here, with the large refractor. It was just bright enough to bear a slight illumination sufficient to render visible the thick wires of the micrometer. By observations extending over rather more than one hour, or until day-break put a stop to them, the comet was found to have a direct motion in right ascension, of about four minutes in time daily, and a southerly motion in declination of about eleven minutes. Every night for three weeks afterwards was cloudy when the comet was above our horizon, and the observations of October 18 are, therefore, all we have been able to procure. Mr. Petersen, M. Rümker, and Mr. Brorsen, though immediately apprised of the discovery by Prof. Schumacher, have had no better success, owing to the overcast sky. The comet was compared instrumentally with  $\beta$  Leonis, for an approximate position, and subsequently, by means of a wire micrometer, with a star of the 9th magnitude.

"The instrumental comparisons gave,—

	Greenwich M. T.			R.A.			Dec.		
	h	m	s	h	m	s	°	'	"
Oct. 18.	16	15	11	11	59	49.1	+14	59	32
	17	6	17	11	59	57.5	+14	59	8

"By four micrometrical measures with the small star, the comet's right ascension was  $38^s.08$  less than that of the star, and its declination  $4' 26''.1$  greater than that of the star, at  $16^h 54^m 16^s$  Greenwich mean time. We are not yet in possession of the accurate position of this star.

"This morning (Nov. 11) the sky was clear in the east for the first time since the date of the comet's discovery. A careful search was made for it, without success."

*Observations of Double Stars, made at Poonah, by Capt. W.S. Jacob.*

The telescope is by Dollond, 5 feet focal length, and mounted on an universal equatorial stand, which mounting is not, however, so well adapted to high as to low latitudes. The telescope is a pretty good one, but shows rather large discs; hence Capt. Jacob has found no advantage in employing a higher power than 152. With this power he can generally measure angles of position when the stars are  $1''.5$  apart. The distances are not to be much relied upon, as "little can be done in that department without clockwork." More than half the stars are taken from Smyth's Cycle, the remainder are such as are too far south to be seen in England. Many of these, though discovered by Capt. Jacob, will probably be found in the forthcoming catalogue of Sir J. F. W. Herschel. In attempting to determine the

orbits of double stars, Capt. Jacob has only been satisfied with that of 12 Cassiopeiæ, which is circular, inclined to the eye at an angle of  $33^\circ$ ; period, 501.6 years; nearest approach, 1888.0; greatest and least distances,  $12''\cdot6$  and  $6''\cdot9$ ; position of node,  $54^\circ\cdot1$ . This seems a fair first approximation.

## XV. *Intelligence and Miscellaneous Articles.*

### BURATITE—A NEW MINERAL.

**T**HIS mineral is a hydrocarbonate of zinc, copper and lime, in definite proportions; it crystallizes in bluish radiating needles; its specific gravity is 3.20. Its composition, according to the analysis of M. Delesse, is—

Carbonic acid . . . . .	21.45
Oxide of zinc . . . . .	32.02
Lime . . . . .	8.62
Oxide of copper . . . . .	29.46
Water . . . . .	8.45

The relations between the elements lead to the formula  $2(\text{Zn, Cu, Ca})\text{C} + \text{aq}$ , and admitting the ideas of M. Scheerer on polymorous isomorphism, we obtain the very simple formula  $\text{CO}^2(\text{RO})^2$ .

Buratite has been found in the copper mines of Lotefskoi in the Altai mountains, at Chessy near Lyons, at Temperino in Tuscany, and in several other localities.—*Comptes Rendus*, Octobre 26.

### ON BISULPHURET, TERSULPHURET, AND SULPHOCYANURET OF METHYLE. BY M. AUGUSTE CAHOURS.

The first-named compound is obtained with equal facility by passing a current of chloromethylic gas into an alcoholic solution of bisulphuret of potassium, or by distilling a mixture of the concentrated solutions of bisulphuret of potassium and sulphomethylate of lime. In both cases a slightly yellow limpid liquid is obtained, which begins to boil at  $230^\circ$  to about  $234^\circ$  F., its boiling-point reaching from  $320^\circ$  to  $338^\circ$  F.

If the alkaline sulphuret employed be very pure, the greater part of the liquid distils between  $230^\circ$  and  $248^\circ$  F. By separating this product, drying it over chloride of calcium and submitting it to one or two rectifications, a definite liquid is obtained which boils between  $241^\circ$  and  $245^\circ$  F.: this is the bisulphuret of methyle. When pure it is a colourless liquid, perfectly limpid, refracting light powerfully, and possessing an intolerable and persistent odour of onions. Its density is 1.046 at  $64^\circ\cdot4$  F. It is scarcely soluble in water, but nevertheless imparts to it its peculiar smell: alcohol and æther combine with it in all proportions. On the approach of an ignited body it inflames and burns with a blue flame, yielding a strong smell of sulphurous acid.

Chlorine acts powerfully on this compound: there is at first  
*Phil. Mag. S. 3. Vol. 30. No. 198. Jan. 1847.* F

formed an amber-coloured substance, which crystallizes in rhombic tables possessing much lustre. These crystals are soon destroyed, and are replaced by a yellowish liquid, which eventually becomes of a ruby-red colour; by continuing the action there are obtained chloride of sulphur  $S Cl^2$ , and perchloride of sulphuret of methyle  $C^4 Cl^6 S$ .

Bromine also acts upon this body, forming products derived from substitution.

Very dilute nitric acid has no sensible action in the cold on bisulphuret of methyle; but it is different when it is moderately dilute. In this case the action is extremely vivid; sulphuric acid is produced, and also a peculiar acid, which forms with potash a salt that crystallizes in long asbestiform needles, and with barytes a salt which crystallizes in colourless tables and possessing much splendour. It also forms with strontia, lime, and the oxide of lead salts which are soluble and crystallizable. Sulphuric acid when concentrated combines with it in the cold, and decomposes it when heated.

When heated in a retort with a concentrated solution of potash, it distils without appearing to undergo any alteration; when submitted to analysis it appeared to be composed of—

C <sup>4</sup> .....	150.0, or in 100	25.53
H <sup>6</sup> .....	37.5 .....	6.38
S <sup>2</sup> .....	400.0 .....	68.09
	<hr/>	<hr/>
	587.5	100.00

The density of the vapour of this substance, taking the mean of two experiments, was found to be 3.298; and the author observes that, supposing the molecule of this compound to give two volumes of vapour, the density would be 3.259.

*Tersulphuret of Methyle.*—When bisulphuret of potassium is replaced by the persulphuret in forming the above compound, a considerable quantity of bisulphuret of methyle is also obtained; but towards the end, and at the temperature of about 396° F., an amber-coloured product distils, which contains more sulphur, and which acts with chlorine and nitric acid in the same manner.

The high boiling-point of this substance, and the slight alteration which it undergoes by heat, prevented the author from ascertaining the density of its vapour. Analysis showed that it was a tersulphuret of methyle.

C <sup>4</sup> .....	150.0, or in 100	19.05
H <sup>6</sup> .....	37.5 ..	4.76
S <sup>3</sup> .....	600.0 ..	76.19
	<hr/>	<hr/>
	787.5	100.00

*Sulphocyanuret of Methyle.*—On distilling a mixture of equal parts of a concentrated solution of sulphocyanuret of potassium and sulphomethylate of lime, there passes over with the vapour of water a heavy yellowish liquid, which after being dried over chloride of calcium, possesses a fixed boiling-point; nine-tenths of the product



come over at about  $270^{\circ}$  F. ; the temperature at which the last portion comes over may amount to  $280^{\circ}$  F.

The preparation of this product is difficult on account of the bumping of the liquid, which sometimes comes over. It must be heated slowly, and the retort should be ten times larger than required to contain the liquid.

Sulphocyanuret of methyle, after it has been purified by digestion over chloride of calcium and rectification, has the following properties : it is a colourless and very limpid liquid ; it has an alliaceous odour ; its vapour is unpleasant and it stupefies ; it boils steadily at about  $270^{\circ}$  F. Its density is 1.115 at about  $61^{\circ}$  F. Water dissolves a very small portion, but it nevertheless acquires its odour ; it combines with alcohol and æther in all proportions.

In diffused light chlorine acts very slowly upon it, producing fine crystals of chloride of cyanogen ; and at the same time there is formed a large quantity of a heavy yellow oil, which solidifies when in contact with ammonia.

Potash scarcely acts upon it in the cold. A solution when heated decomposes it, and there are formed ammonia and bisulphuret of methyle ; the residue contains cyanuret of potassium and carbonate of potash.

Solution of ammonia alters it readily, giving rise to a brown substance analogous to ulmine, and a small quantity of a crystallized white matter. Heated in an alcoholic solution, the sulphocyanuret of methyle is decomposed ; there are produced bisulphuret of methyle and sulphocyanuret of potassium, easily recognised by the persalts of iron. Moderately concentrated nitric acid dissolves it when heated, but it separates entirely on cooling.

By analysis it appears to consist of—

C <sup>s</sup> .....	300.0, or in 100	32.87
H <sup>6</sup> .....	37.5 ..	4.11
N <sup>3</sup> .....	177.0 ..	19.18
S <sup>2</sup> .....	400.0 ..	43.84
	<u>914.5</u>	<u>100.00</u>

The mean of two experiments gave 2.559 as the density of its vapour ; whereas, supposing its molecule to yield four volumes of vapour, calculation would give 2.537.—*Ann. de Ch. et de Ph.*, Novembre 1846.

#### ON SULPHOCYANURET AND BISULPHURET OF ETHYLE.

BY M. AUGUSTE CAHOURS.

*Sulphocyanuret of Ethyle.*—By distilling a mixture of sulphovinate of lime and persulphuret of potassium, both solutions being concentrated, a yellowish liquid is obtained, which after washing with water, digesting on chloride of calcium, and cautious rectification, becomes a colourless and very limpid liquid, the odour of which resembles that of sulphocyanuret of methyle. Its density is 1.020 at about  $61^{\circ}$  F. It is insoluble in water, but alcohol and æther dissolve it in

all proportions. It acts similarly to sulphocyanuret of methyle upon reagents.

Chlorine and bromine act energetically on the sulphur compounds of methyle and of ethyle. With bromine perfectly well-crystallized combinations are obtained.

By analysis this substance appears to be composed of—

C <sup>12</sup> . . . . .	450·0, or in 100 parts	41·38
H <sup>10</sup> . . . . .	62·5 . . . . .	5·74
N <sup>2</sup> . . . . .	177·0 . . . . .	16·09
S <sup>2</sup> . . . . .	400·0 . . . . .	36·79
	1089·5	100·00

By experiment the density of the vapour of this substance was found to be 3·018, while calculation would lead to 3·032, supposing the molecule represented four volumes of vapour.

*Bisulphuret and Tersulphuret of Ethyle.*—The former was obtained in considerable quantity by distilling a mixture of sulphovinate of lime and persulphuret of potassium; but towards the end of the rectification of the rough product there came over a yellowish and slightly volatile liquid, which gave by analysis results which tended to show that it was to be considered as a tersulphuret of ethyle.

M. Cahours analysed the bisulphuret of ethyle, and his results agreed precisely with those previously obtained by M. P. Morin, C<sup>8</sup> H<sup>10</sup> S<sup>2</sup>. The density of its vapour was found by experiment to be 4·270, and by calculation 4·240, on the supposition that the molecule of this compound furnishes two volumes of vapour.

Chlorine vividly acts upon the two compounds above described; moderately concentrated nitric acid does the same, a portion of the sulphur being converted into sulphuric acid; there is also produced an acid susceptible of forming soluble and crystallizable salts with potash, barytes, and oxide of lead.—*Ann. de Ch. et de Phys.*, Novembre 1846.

#### ANALYSIS OF THE WHITE DIOPSIDE.

M. Hermann found this substance from Achmatowsk to consist of—

Silica . . . . .	53·97
Lime . . . . .	25·60
Magnesia . . . . .	17·86
Protoxide of iron . . . . .	2·00
Protoxide of manganese . . . . .	0·57
	100·00

This analysis corresponds to the formula Si<sup>4</sup> M<sup>4</sup> O<sup>6</sup>: [Si<sup>4</sup> (Ca<sup>2</sup> Mg<sup>2</sup>) O<sup>6</sup>].—*Journ. de Pharm. et de Ch.*, Novembre 1846.

#### ANALYSES OF THE NATIVE PHOSPHATES OF COPPER.

BY M. HERMANN.

The phosphates of copper do not occur more extensively anywhere than in the mines of Nischnotaglsk in the Uralian Moun-

tains; M. Hermann has submitted to analysis some fine specimens of this mineral from them.

The method of analysis which he adopted was the following:—he reduced the mineral to fine powder, calcined it to determine the water, and fused the residue with four times its weight of soda at a red heat; it was then treated with boiling water, and the remaining oxide of copper was weighed after heating to redness. The phosphoric acid was afterwards ascertained by deficiency, after having ascertained the absence of the oxides of iron and manganese in the oxide of copper, as well as of alumina and manganic acid in the alkaline solution.

The *libethenite* of Tagilsk yielded 65.89 oxide of copper, 28.61 phosphoric acid, 5.50 water. These results differ a little from those obtained by M. Kühn,—66.94 oxide of copper, 29.44 phosphoric acid, 4.01 water; and also from those of Berthier,—63.9 oxide of copper, 28.7 phosphoric acid, and 7.4 water.

*Dihydrate* (prismatic phosphate of copper) yielded 68.2 oxide of copper, 25.3 phosphoric acid, 6.28–6.69 water. Arfwedson obtained nearly the same results with a phosphate from Rheinbreitenbach.

The *phosphorocalcite* is the most widely diffused of the phosphates of copper. M. Hermann analysed three specimens of it:

	Tagilsk.		Tagilsk.		Virneberg, near Rheinbreitenbach.
Oxide of copper . . . .	68.75	. . . .	67.73	. . . .	67.25
Phosphoric acid . . . .	23.75	. . . .	23.47	. . . .	24.55
Water . . . . .	7.50	. . . .	8.80	. . . .	8.20
	100.00		100.00		100.00

Except that the water is in rather larger quantity, this phosphate has the same composition as the dihydrate.

The analyses of Klaproth, Kühn and Lunn differ much from those of M. Hermann; it would appear that Lunn analysed a different mineral.

Under the name of *ehlite* another phosphate of copper has been described; it is found at Ehl, near Rheinbreitenbach. M. Hermann found a specimen from Tagilsk to consist of oxide of copper 66.86, phosphoric acid 23.14, water 10.00; he describes, under the name of *tagilithe*, an earthy phosphate of copper found at Tagilsk, on hydrate of iron, in the form of mammillated or spongy deposits. Its specific gravity is about 3.50. It dissolves in acids without giving out any gas.

The *trombolite* is another phosphate of copper found at Tagilsk; it has already been described by M. Plattner; it is remarkable for its conchoidal fracture; it was found to consist of 39.2 oxide of copper, 41.0 phosphoric acid, 16.8 water. M. Hermann has discovered the phenakite in granite to the west of Lake Ilmen, at some versts from Miask, accompanied with topaze and bluish stilbite.—*Ibid.* Nov. 1846.

## ON CHIOLITE—A NEW MINERAL. BY M. HERMANN.

There is found in the granite of the district of Miask in Russia a white mineral, which has much resemblance to cryolite, but the composition is different.

This new mineral occurs in compact and granular masses, and sometimes spathose or leafy. The lamellar portions are somewhat translucent, and give an appearance to the mineral of its being impregnated with moisture or with fatty matter. The cleavage faces form an angle of  $66^\circ$ .

Its hardness is that of fluor spar; its lustre is between vitreous and greasy. Its density is 2.72. It fuses below the melting-point of glass, without yielding a trace of water. Heated in an open tube, it evinces the action of hydrofluoric acid. It fuses readily with borax and a phosphate, giving colourless glasses. When mixed with sulphuric acid, the powdered mineral disengages much hydrofluoric acid. Carbonate of ammonia precipitates pure alumina from the solution; the liquor separated from the precipitate contains pure sulphate of soda, unmixed with potash or lithia.

M. Hermann found it to consist of—

Sodium . . . . .	23.78
Aluminium . . . . .	18.69
Fluorine . . . . .	57.33
	<hr/>
	100.00

Cryolite contains, for the same quantity of fluoride of sodium, only half the quantity of fluoride of aluminium contained in chiolite.—*Ibid.*

## PURIFICATION OF MERCURY. BY M. MILLON.

Mercury may be perfectly purified by agitating it for a considerable time with weak nitric acid. With two pounds of the metal, about an ounce and a half of the acid diluted with twice its volume of water may be employed. The mercury freed from the nitrate thus formed is to be boiled with pure nitric acid sufficient to dissolve about nine-tenths of the metal; the resulting nitrate of mercury is to be reduced to red oxide by heat, and this is to be calcined in a porcelain retort to reduce it.

By the action of the first portion of nitric acid the more oxidable metals are acted upon; the second portion of acid leaves the metals less oxidable than mercury in the undissolved portion.

As the mercury reduced by this process dissolves a notable quantity of oxide, this last is to be separated by agitation with sulphuric acid; it is afterwards to be washed with a very large quantity of water, and dried in the receiver of the air-pump over sulphuric acid. Mercury thus purified was employed by M. Regnault in his third determination of its density.

M. Millon states that when a saline solution, such as chloride of



calcium, hydrochlorate of ammonia, nitrate of potash, &c. is added to mercury in a bottle, the mercury is always divided into rounded globules, which remain separated from each other for a long time; but what is very striking is, that the size of the globules, which varies enormously, is always connected with the nature of the aqueous solution. Some solutions immediately cause extreme division in the mercury; others, on the contrary, produce only very large globules, to whatever extent the agitation may have been carried; and the same effect is always produced with the same solution. It is to this influence of saline solutions that is due the difficulty often witnessed in collecting mercury when it has been reduced in the moist way. —*Ann. de Ch. et de Phys.*, Novembre 1846.

#### METEOROLOGICAL OBSERVATIONS FOR NOV. 1846.

*Chiswick.*—November 1. Foggy. 2, 3. Very fine. 4. Overcast. 5. Foggy and drizzly. 6, 7. Foggy: hazy. 8. Foggy. 9. Hazy and cold. 10. Dusky clouds: fine. 11. Cold and dry. 12. Overcast. 13. Hazy. 14. Foggy. 15. Cloudy. 16. Overcast: cloudy and fine. 17. Densely overcast: slight rain at night. 18. Cloudy: very fine. 19. Cloudy and damp: very fine. 20. Cloudy: boisterous. 21. Very fine: rain. 22. Fine: clear. 23. Slight rain: overcast. 24. Hazy and damp: rain: overcast. 25. Rain. 26. Clear and fine: foggy. 27. Dense fog. 28. Clear and fine. 29. Frosty: clear and fine. 30. Sharp frost: overcast.

Mean temperature of the month .....	44°·53
Mean temperature of Nov. 1845 .....	44 ·26
Mean temperature of Nov. for the last twenty years ...	42 ·91
Average amount of rain in Nov. ....	2·56 inches.

*Boston.*—Nov. 1. Foggy. 2. Fine. 3, 4. Cloudy. 5. Fine. 6—9. Cloudy. 10, 11. Fine. 12—14. Cloudy. 15. Cloudy: rain p.m. 16. Cloudy. 17. Cloudy: rain p.m. 18. Cloudy. 19. Fine. 20. Stormy. 21. Cloudy: rain p.m. 22. Fine: rain p.m. 23. Cloudy: rain p.m. 24. Fine. 25. Cloudy. 26. Fine. 27. Cloudy: rain a.m. and p.m. 28. Cloudy: snow p.m. 29. Fine: snow on the ground. 30. Fine: melted snow in the gauge.

*Sandwich Manse, Orkney.*—Nov. 1. Showers: cloudy. 2, 3. Cloudy. 4. Bright: rain. 5. Bright: cloudy. 6. Hazy: rain. 7. Bright: showers. 8. Bright: clear: fine. 9. Clear: very clear and fine. 10. Hoar-frost: cloudy: fine. 11—15. Cloudy: fine. 16. Bright: clear: aurora. 17. Rain: clear: aurora, south. 18. Clear: showers. 19. Clear: aurora. 20. Rain: showers. 21. Showers. 22. Rain: clear. 23. Damp: cloudy. 24. Showers. 25. Showers: cloudy. 26. Showers. 27. Hail-showers. 28. Snow: hail-showers. 29. Snow-drift: clear. 30. Cloudy: shower: thaw.

*Applegarth Manse, Dumfries-shire.*—Nov. 1. Slight showers p.m. 2. Showers morning and evening. 3. Rain p.m. 4. Moist a.m.: rain p.m. 5. Showers. 6. Fair, but moist. 7. Fair, but dull and cloudy. 8. Fair and fine. 9. Fair, but cloudy. 10. Hoar-frost: clear and fine. 11. Fair and fine: hoar-frost a.m. 12—15. Fair, but dull. 16. Clear: piercingly cold. 17. Wet. 18. Showers. 19. Heavy rain. 20. Heavy rain: river in flood. 21. Heavy rain: thunder p.m. 22. Rain. 23. Heavy rain. 24. Rain a.m. 25. Rain and fog. 26. Dull and drizzly. 27. Fair. 28. Frost: shower of snow. 29. Hard frost: clear. 30. Frost: thick and foggy.

Mean temperature of the month .....	44°·4
Mean temperature of Nov. 1845 .....	42 ·7
Mean temperature of Nov. for 23 years .....	40 ·2
Mean rain in Nov. for 18 years .....	3·63 inches.

*Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at Chiswick, near London; by Mr. Veall, at Boston; by the Rev. W. Dunbar, at Applegarth Manse, Dumfries-shire; and by the Rev. C. Clouston, at Sandwick Manse, Orkney.*

Days of Month.	Barometer.						Thermometer.						Wind.				Rain.																
	Chiswick.			Dumfries-shire.			Orkney, Sandwick.			Chiswick.			Boston.			Dumfries-shire.			Orkney, Sandwick.			Chiswick.			Boston.			Dumfries-shire.			Orkney, Sandwick.		
	Max.		Min.	9 a.m.		9 p.m.	9 a.m.		8 p.m.	Max.		Min.	8 a.m.		Max.		Min.	9 a.m.		9 p.m.	Max.		Min.	8 a.m.		Max.		Min.	9 a.m.		9 p.m.		
	1846.	Nov.																															
1.	30.043	29.959	29.67	29.73	29.68	29.66	29.56	34	38	53½	50	52	53	calm	s.	calm	sse.	calm	sse.	calm	s.	calm	sse.	calm	s.	calm	sse.	calm	sse.	calm	sse.		
2.	29.934	29.911	29.61	29.70	29.64	29.76	29.72	59	47	54	49	50	52	calm	s.	calm	se.	calm	se.	calm	s.	calm	se.	calm	s.	calm	se.	calm	se.	calm	se.		
3.	30.050	29.991	29.58	29.78	29.73	29.80	29.87	61	52	55	43½	52½	49	calm	s.	calm	ese.	calm	ese.	calm	s.	calm	ese.	calm	s.	calm	ese.	calm	ese.	calm	ese.		
4.	30.096	30.053	29.70	29.78	29.83	29.84	29.87	60	51	57	50	52	52½	calm	s.	calm	se.	calm	se.	calm	s.	calm	se.	calm	s.	calm	se.	calm	se.	calm	se.		
5.	30.157	30.131	29.65	30.00	29.96	30.06	30.05	59	41	52.5	47½	50	52	calm	se.	calm	ese.	calm	ese.	calm	se.	calm	ese.	calm	se.	calm	ese.	calm	ese.	calm	ese.		
6.	30.149	30.146	29.76	29.98	30.00	30.04	29.93	46	49	55	51	52	50½	calm	se.	calm	ese.	calm	ese.	calm	se.	calm	ese.	calm	se.	calm	ese.	calm	ese.	calm	ese.		
7.	30.269	30.222	29.84	30.08	30.18	30.05	30.18	44	46	51	44	51	51	calm	se.	calm	ese.	calm	ese.	calm	se.	calm	ese.	calm	se.	calm	ese.	calm	ese.	calm	ese.		
8.	30.387	30.338	29.93	30.23	30.30	30.32	30.36	47	44	56½	48	50	45	calm	e.	calm	n.	calm	n.	calm	e.	calm	n.	calm	e.	calm	n.	calm	n.	calm	n.		
9.	30.449	30.429	30.05	30.35	30.37	30.39	30.42	43	45	49	40½	47½	43	calm	e.	calm	ene.	calm	ene.	calm	e.	calm	ene.	calm	e.	calm	ene.	calm	ene.	calm	ene.		
10.	30.444	30.396	30.21	30.40	30.39	30.46	30.47	49	45	48	30	43½	46½	calm	e.	calm	ene.	calm	ene.	calm	e.	calm	ene.	calm	e.	calm	ene.	calm	ene.	calm	ene.		
11.	30.278	30.258	30.02	30.39	30.35	30.47	30.48	51	38	50½	32	46½	45	calm	e.	calm	ene.	calm	ene.	calm	e.	calm	ene.	calm	e.	calm	ene.	calm	ene.	calm	ene.		
12.	30.343	30.314	30.00	30.40	30.38	30.50	30.51	51	43	49.5	42	45	45	calm	ne.	calm	ene.	calm	ene.	calm	ne.	calm	ene.	calm	ne.	calm	ene.	calm	ene.	calm	ene.		
13.	30.343	30.303	30.04	30.35	30.30	30.46	30.39	47	44	50	48½	47	47	calm	e.	calm	ene.	calm	ene.	calm	e.	calm	ene.	calm	e.	calm	ene.	calm	ene.	calm	ene.		
14.	30.271	30.216	29.95	30.23	30.20	30.30	30.28	49	37	46	42	47½	46	calm	ne.	calm	ene.	calm	ene.	calm	ne.	calm	ene.	calm	ne.	calm	ene.	calm	ene.	calm	ene.		
15.	30.162	30.122	29.77	30.10	30.05	30.23	30.17	48	40	47	45	41	46½	calm	e.	calm	ene.	calm	ene.	calm	e.	calm	ene.	calm	e.	calm	ene.	calm	ene.	calm	ene.		
16.	30.128	30.065	29.83	29.95	29.75	30.04	29.80	48	39	48	48½	40	47	calm	e.	calm	ene.	calm	ene.	calm	e.	calm	ene.	calm	e.	calm	ene.	calm	ene.	calm	ene.		
17.	29.902	29.848	29.51	29.54	29.59	29.56	29.52	51	39	45	41	49½	48	calm	se.	calm	e.	calm	e.	calm	se.	calm	e.	calm	se.	calm	e.	calm	e.	calm	e.		
18.	29.828	29.487	29.45	29.52	29.40	29.51	29.36	58	44	49	54½	48	47	calm	s.	calm	e.	calm	e.	calm	s.	calm	e.	calm	s.	calm	e.	calm	e.	calm	e.		
19.	29.810	29.768	29.32	29.42	29.40	29.29	29.27	56	48	48.5	45	47	45½	calm	s.	calm	se-sse	calm	se-sse	calm	s.	calm	se-sse	calm	s.	calm	se-sse	calm	se-sse	calm	se-sse		
20.	29.434	29.295	29.00	28.98	28.70	29.12	28.87	54	43	51	53	47	49	calm	sw.	calm	se-sse	calm	se-sse	calm	sw.	calm	se-sse	calm	sw.	calm	se-sse	calm	se-sse	calm	se-sse		
21.	29.631	29.530	29.24	29.24	29.17	28.91	29.13	53	39	46	50½	43½	48½	calm	sw.	calm	sw-se	calm	sw-se	calm	sw.	calm	sw-se	calm	sw.	calm	sw-se	calm	sw-se	calm	sw-se		
22.	29.579	29.366	28.96	29.10	29.33	29.03	29.19	55	39	44	47½	38	43	calm	w.	calm	sw.	calm	sw.	calm	w.	calm	sw.	calm	w.	calm	sw.	calm	sw.	calm	sw.		
23.	29.799	29.711	29.45	29.52	29.45	29.50	29.51	51	49	42	48	35	46	calm	sw.	calm	ese.	calm	ese.	calm	sw.	calm	ese.	calm	sw.	calm	ese.	calm	ese.	calm	ese.		
24.	29.696	29.580	29.19	29.34	29.45	29.33	29.37	56	46	52	53½	40	47½	calm	sw.	calm	sw.	calm	sw.	calm	sw.	calm	sw.	calm	sw.	calm	sw.	calm	sw.	calm	sw.		
25.	29.663	29.260	29.29	29.38	29.06	29.36	29.26	52	43	48	41	45	44	calm	s.	calm	e.	calm	e.	calm	s.	calm	e.	calm	s.	calm	e.	calm	e.	calm	e.		
26.	29.252	29.232	28.82	29.03	29.12	29.17	29.41	56	50	46	47	42	45	calm	sw.	calm	nw.	calm	nw.	calm	sw.	calm	nw.	calm	sw.	calm	nw.	calm	nw.	calm	nw.		
27.	29.348	29.306	29.00	29.28	29.42	29.65	29.58	44	42	45½	42½	40	33	calm	sw.	calm	nw.	calm	nw.	calm	sw.	calm	nw.	calm	sw.	calm	nw.	calm	nw.	calm	nw.		
28.	29.544	29.402	29.16	29.38	29.58	29.67	29.83	45	22	36.5	38½	27	35	calm	n.	calm	n.	calm	n.	calm	n.	calm	n.	calm	n.	calm	n.	calm	n.	calm	n.		
29.	29.799	29.710	29.46	29.68	29.75	29.84	29.96	37	16	32	36½	28	33½	calm	n.	calm	nw.	calm	nw.	calm	n.	calm	nw.	calm	n.	calm	nw.	calm	nw.	calm	nw.		
30.	29.949	29.848	29.62	29.86	29.82	29.92	29.57	34	27	30	35	27½	35½	calm	n.	calm	nw.	calm	nw.	calm	n.	calm	nw.	calm	n.	calm	nw.	calm	nw.	calm	nw.		
Mean.	29.957	29.873	29.56	29.759	29.745	29.808	29.796	50.46	38.60	45.6	49.4	41.4	46.38	45.66							1.43	1.70	3.55	3.27									

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[THIRD SERIES.]

FEBRUARY 1847.

XVI. *The ASTRONOMER ROYAL on Sir David Brewster's  
New Analysis of Solar Light.*

*To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,

Royal Observatory, Greenwich, Jan. 15, 1847.

**I**N the Notes to the second edition of the History of the Inductive Sciences, Dr. Whewell has (with my consent) referred to experiments, made by me and described by me at a meeting of the Cambridge Philosophical Society, tending to invalidate the conclusions drawn by Sir David Brewster in a paper headed "On a New Analysis of Solar Light, indicating three Primary Colours, forming Coincident Spectra of equal length," which is printed in the twelfth volume of the Edinburgh Transactions. I think it therefore incumbent on me to give to the public the best account that I am now able to give of these experiments. The reason of the partial imperfection of the account is the following. I never drew up in writing any orderly statement either of the experiments or of the reasoning connected with them. I had the pencil notes, written each at the instant of making an experiment; and from these notes, or rather from the strong recollection of every experiment detailed in the notes, I made my oral statement (as far as the mere experimental facts were concerned) to the Cambridge Philosophical Society. These notes I have mislaid, and in consequence I am unable to give details on the names and combinations of the different absorbing substances which I employed. But upon the method, upon the results, and upon the reasonings, my recollection is as perfect as it was on the days on which the experiments were made. Having premised this, I shall give my statement in the same order in which I gave it (in the year 1833, I believe) to the Cambridge Philosophical Society.

*Phil. Mag. S. 3. Vol. 30. No. 199. Feb. 1847.*

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First, then, I remark that, supposing Sir David Brewster's alleged experimental facts to be placed beyond doubt, the inference as to the three colours is exceedingly doubtful. It appears certain that combinations of other colours might be made to produce the apparent colours of the spectrum; and there seems therefore to be no sufficient reason for selecting those adopted by Sir David Brewster. It is quite proper that the reader's attention should be called to this; for in consequence of Sir David Brewster having given the details of his theory in considerable extent before he gives the experiments, the reader may be led to suppose that the experiments do actually exhibit three such spectra as are implied in the title of the paper. Those, however, who will enter into the minutiae of the paper, will see that the experiments exhibit no such thing. The simple facts which the experiments are supposed to exhibit are, that by the use of different absorbing media, certain portions of the spectra undergo certain changes of colour. And the only question which I am disposed to entertain is this: Is it established that the colour of any portion of the spectrum is changed by the use of any absorbing medium?

My second remark applies to what I conceive to be one possible cause of error in Sir David Brewster's experiment. It does not appear from his paper that the spectrum in its modified state was compared immediately with an unmodified spectrum. Now I state unhesitatingly as my own belief, that the eye has no memory for colours; and I have no confidence in any evidence upon the change of colour, unless the colour which is not altered and the colour in which change is suspected are placed side by side.

Thirdly, I remark that it does not appear that care was taken to exclude light coming to the eye from other sources. Indeed, in an experiment of Sir William Herschel's, to which Sir David Brewster refers, it is certain that there was a mixture of other light. When the spectrum was thrown upon clean brass, "the colour of the brass made the red rays appear like orange, and the orange colour was likewise different from what it ought to be." But on referring to Sir William Herschel's paper, it will be seen that the spectrum was formed by a prism placed in the upper part of an open window; and it is not difficult to predict what must be the effect of the light coming from all other parts of the window.

I now proceed to describe the form in which my own experiments were made.

The windows of my lecture-room were darkened by closing all the shutters, the joints of which were almost absolutely impervious to light. Through a hole in one shutter, a beam



of sun-light was thrown in a convenient direction by means of an external reflector. In front of this hole, but at a distance sufficient to leave room for the insertion of any of my absorbing substances, was placed a metallic plate with a narrow slit about two inches long and adjustable in width. The sun-light passing through this slit was received upon a very good prism placed with its axis parallel to the slit, at the distance of about a foot; a large lens was placed in front of the prism, and by means of this the rays diverging from the slit and falling on the prism were made to converge, so as to form upon a paper screen a very good spectrum, sufficiently pure to exhibit several of the stronger Fraunhofer's lines. Care was taken to prevent any other light from falling directly on the screen.

In order to exhibit the effect of any absorbing medium, it was only necessary to place it behind the slit, in such a manner as to receive and transmit the light which was to pass through a portion of the slit. The corresponding portion of the spectrum underwent the modification produced by the absorption: but the modified portion of the spectrum was in no degree dislocated from the unmodified part (Fraunhofer's lines running without dislocation through both), and the modified portion was separated from the unmodified portion only by the small interruption which is usually occasioned by the edges of any refracting substance. It is impossible to conceive circumstances more favourable than these for the comparison of the modified and unmodified spectra.

Of the absorptive media which I used, I am unable (as I have already stated) to give a complete catalogue. I had however several specimens of smalt-blue glass, all the varieties of coloured glass which I had been able at different times to procure from a glazier, and all the strikingly coloured fluids which I could obtain from a chemist, as well as port wine, porter, &c. The fluids were used in flat perfume-bottles: it would have been better to use them in slightly prismatic vessels, which would admit of the use of different thicknesses of the fluids.

Of the results of these experiments I can give little more than the single negative one,—that no change was produced in the qualities of the colours. After the most patient examination, I could not even suspect a change except in two or three instances, when I imagined that the blue colour was extended both ways, so as to trench upon the green on one side and upon the violet on the other side: but even these suspicions were removed when I closed with greater care the openings by which a little stray light entered between the shutter-hole and the slit in the plate of metal.

The evidence which I am able to offer upon this subject is necessarily, from the nature of the subject, only negative; and the conclusion to which it leads will be destroyed by the establishment of any one fact in opposition to it. But such a fact must be established beyond doubt: and I have no hesitation in saying that no form of experiment anterior to my own has been such as to place its conclusions beyond doubt. I insist upon the two points—of juxtaposition of the modified and unmodified spectra, and of exclusion of other light—as indispensably necessary for accuracy.

The experimenter must also be on his guard against the apparent effects of mere intensification or dilution of light in modifying its quality. Every one knows, for instance, that when the setting sun is beginning to assume a slightly yellow colour, its light received on a white plaster wall appears of a full blush red, although there is not the smallest reason to think that the quality of the light is really altered.

I am, Gentlemen,

Your obedient Servant,

G. B. AIRY.

. XVII. *On the Effects of Magnetism upon the Dimensions of Iron and Steel Bars.* By J. P. JOULE, Esq.\*

ABOUT the close of the year 1841, Mr. F. D. Arstall, an ingenious machinist of this town, suggested to me a new form of electro-magnetic engine. He was of opinion that a bar of iron experienced an increase of bulk by receiving the magnetic condition; and that, by reversing its polarity rapidly by means of alternating currents of electricity, an available and useful motive power might be realized. At Mr. Arstall's request I undertook some experiments in order to decide how far his opinions were well-founded.

The results of my inquiries were brought before the public on the occasion of a *conversazione* held at the Royal Victoria Gallery of Manchester on the 16th of February 1842, and are printed in the eighth volume of Sturgeon's *Annals of Electricity*, p. 219. As many of my readers may not have access to that work, I will, with the permission of the Editors of the *Philosophical Magazine*, quote a small portion of the paper, which is necessary to complete the history of this interesting branch of investigation.

“A length of thirty feet of copper wire, one-twentieth of an inch thick, and covered with cotton thread, was formed into a

\* Communicated by the Author.

coil twenty-two inches long and one-third of an inch in interior diameter. This coil was secured in a perpendicular position; and the rod of iron, of which I wished to ascertain the increment, was suspended in its axis so as to receive the magnetic influence whenever a current of electricity was passed through the coil. Lastly, the upper extremity of the rod was fixed, and the lower extremity was attached to a system of levers which multiplied its motion three thousand times.

“A bar of rectangular iron wire, two feet long, one quarter of an inch broad, and one-eighth of an inch thick, caused the index of the multiplying apparatus to spring from its position and vibrate about a point one-tenth of an inch in advance, when the coil was made to complete the circuit of a battery capable of magnetizing the iron to saturation, or nearly so. After a short interval of time, the index ceased to vibrate, and began to advance very gradually in consequence of the expansion of the bar from the heat which was radiated from the coil. On breaking the circuit, the index immediately began to vibrate about a point, exactly one-tenth of an inch lower than that to which it had attained.

“By multiplying the advance of the index by the power of the levers, we have  $\frac{1}{30000}$ th of an inch, the increment of the bar, which may be otherwise stated as  $\frac{1}{72000}$ th of its whole length.

“Similar results were obtained by the use of an iron wire, two feet long and one-twelfth of an inch in diameter. Five pairs of the nitric acid battery produced an increment of the thirty-thousandth part of an inch; and when only one pair of the battery was employed, I had an increment very slightly less, viz., the thirty-three thousandth part of an inch.

“This increment does not appear to depend upon the thickness of the bar; for an electro-magnet made of iron, three feet long and one inch square, was found to expand under the magnetic influence to nearly the same extent, compared with its length, as the wires did in the previous experiments.

“I made some experiments in order to ascertain the *law* of the increment. Their results proved it to be *very nearly proportional to the intensity of the magnetism and the length of the bar*.

“Trial was made whether any effect could be produced by using a *copper* wire, two feet long and about one-tenth of an inch in diameter; but I need scarcely observe that the attempt was unattended with the slightest success.

“A very good way of observing the above phænomena is to examine one end of an electro-magnet with a powerful microscope while the other end is fixed. The increment is then

observed to take place with extreme suddenness, as if it had been occasioned by a blow at the other extremity. The expansion, though very minute, is indeed so very rapid that it may be felt by the touch; and if the electro-magnet be placed perpendicularly on a hard elastic body, such as glass, the ear can readily detect the fact that it makes a slight jump every time that contact is made with the battery.

“When one end of the electro-magnet is applied to the ear, a distinct musical sound is heard every time that contact is made with, or broken from, the battery; another proof of the suddenness with which the particles of iron are disturbed.”

In another part of the lecture I stated my reasons for supposing that whilst the bar of iron was increased in length by the magnetic influence, it experienced a *contraction* at right angles to the magnetic axis, so as to prevent any change taking place in the bulk of the bar. I intended as soon as possible to bring this conjecture to the test of experiment, and I prepared some apparatus for the purpose; but owing to other occupations I was obliged to relinquish the experiments until the beginning of last summer. In the meantime the inquiry has been taken up by De la Rive, Matteucci, Wertheim, Wartmann, Marrian, Beatson and others, whose ingenious experiments have invested the subject with additional interest. The researches of Beatson have taken a similar direction to mine; and he appears also to have employed a somewhat similar apparatus to that which I shall presently describe. I have confirmed several of the results at which this gentleman has arrived, and have added new facts, which I hope will throw further light upon this rather obscure department of physics.

In order to ascertain how far my opinion as to the invariability of the *bulk* of a bar of iron under magnetic influence was well-founded, I devised the following apparatus. Ten copper wires, each 110 yards long and one-twentieth of an inch in diameter, were bound together by tape so as to form a good, and at the same time very flexible conductor. The bundle of wires thus formed was coiled upon a glass tube 40 inches long and  $1\frac{1}{2}$  inch in diameter. One end of the tube was hermetically sealed, and the other end was furnished with a glass stopper, which was itself perforated so as to admit of the insertion of a graduated capillary tube. In making the experiments, a bar of annealed iron, one yard long and half an inch square, was placed in the tube, which was then filled up with water. The stopper was then adjusted, and the capillary tube inserted so as to force the water to a convenient height within it.

The bulk of the iron bar was about 4,500,000 times the



capacity of each division of the graduated tube; consequently a very minute expansion of the former would have produced a perceptible motion of the water in the capillary tube: but, on connecting the coil with a Daniell's battery of five or six cells (a voltaic apparatus quite adequate to saturate the iron), no perceptible effect whatever was produced either in making or breaking contact with the battery, whether the water was stationary in the stem, or gradually rising or falling from a change of temperature. Now had the usual increase of length been unaccompanied by a corresponding diminution of the diameter of the bar, the water would have been forced through twenty divisions of the capillary tube every time that contact was made with the battery.

Having thus ascertained that the bulk of the bar was invariable, I proceeded to repeat my first experiments with a more delicate apparatus, in order, by a more careful investigation of the laws of the increment of length, to ascend to the probable cause of the phænomenon.

A coiled glass tube, similar to that already described, was fixed vertically in a wooden frame. Its length was such that when a bar one yard long was introduced so as to rest on the sealed end, each extremity of the bar was a full inch within the corresponding extremity of the coil. The apparatus for observing the increment of length consisted of two levers of the first order, and a powerful microscope situated at the extremity of the second lever. These levers were furnished with brass knife edges resting upon glass. The connexion between the free extremity of the bar of iron and the first lever, and that between the two levers, were established by means of exceedingly fine platinum wires.

The first lever multiplied the motion of the extremity of the bar 7·8 times, the second multiplied the motion of the first 8 times, and the microscope was furnished with a micrometer divided into parts each corresponding to  $\frac{1}{2220}$ th of an inch. Consequently each division of the micrometer passed over by the index indicated an increment of the length of the bar amounting to  $\frac{1}{138328}$ th of an inch.

The quantities of electricity passing through the coil were measured by an accurate galvanometer of tangents, consisting of a circle of thick copper wire one foot in diameter, and a needle half an inch long furnished with a suitable index.

The quantities of magnetic polarity communicated to the iron bar were measured by a finely suspended magnet 18 inches long, placed at the distance of one foot from the centre of the coil. This magnetic bar was furnished with scales precisely in the manner of an ordinary balance, and the weight

required to bring it to a horizontal position indicated the intensity of the magnetism of the iron bar under examination.

After a few preliminary trials, a great advantage was found to result from filling the tube with water. The effect of the water was, as De la Rive had already remarked, to prevent the sound. It also checked the oscillations of the index, and had the important effect of preventing any considerable irregularities in the temperature of the bar.

The first experiment which I shall record was made with a bar consisting of two pieces of very well-annealed rectangular iron wire, each one yard long, a quarter of an inch broad, and about one-eighth of an inch thick. The pieces were fastened together so as to form a bar of nearly a quarter of an inch square. The coil was placed in connexion with a single constant cell, the resistance being further increased by the addition of a few feet of fine wire. The instant that the circuit was closed, the index passed over one division of the micrometer. The needle of the galvanometer was then observed to stand at  $7^{\circ} 20'$ , while the magnetic balance required 0.52 of a grain to bring it to an equilibrium. It had been found by proper experiments that a current of  $7^{\circ} 20'$  passing through the coil was itself capable of exerting a force of 0.03 of a grain upon the balance; consequently the magnetic intensity of the bar was represented by 0.49 of a grain. On breaking the circuit, the index was observed to retire 0.3 of a division, leaving a permanent elongation of 0.7, and a permanent polarity of 0.42 of a grain. More powerful currents were now passed through the coil, and the observations repeated as before, with the results tabulated below.

#### Experiment 1.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of bar.	Total elongation.	Magnetic intensity of bar.	Square of magnetic intensity divided by total elongation.
— $7^{\circ} 20'$	128	1.0 E.	1.0	—0.49	240
0	0	0.3 S.	0.7	—0.42	252
— $9^{\circ} 30'$	167	2.9 E.	3.6	—0.93	240
0	0	1.2 S.	2.4	—0.74	228
— $14^{\circ} 48'$	264	5.9 E.	8.3	—1.42	243
0	0	3.8 S.	4.5	—1.00	222
— $23^{\circ} 10'$	428	10.3 E.	14.8	—1.87	236
0	0	7.6 S.	7.2	—1.26	220
— $47^{\circ} 25'$	1088	16.1 E.	23.3	—2.22	211
0	0	13.9 S.	9.4	—1.35	194
— $58^{\circ} 50'$	1653	14.8 E.	24.2	—2.21	202
0	0	13.3 S.	10.9	—1.35	168

Everything being left in the same position, the electricity was now transmitted in the + or contrary direction, so as first to remove the polarity acquired by the — current, and then to induce the opposite polarity. The total elongation of 10·9, acquired in the last experiment, is carried forward in the 4th column of the following second table of results.

Experiment 1 (continued).

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of bar.	Total elongation.	Magnetic intensity of bar.	Square of magnetic intensity divided by total elongation.
+ 6 15	109	3·4 S.	7·5	—0·12	2
0	0	0	7·5	—0·17	4
+ 9 55	175	0·1 E.	7·6	+0·57	43
0	0	1·0 S.	6·6	+0·25	9
+15 40	280	3·7 E.	10·3	+1·30	164
0	0	4·0 S.	6·3	+0·78	97
+38 45	802	16·8 E.	23·1	+2·30	229
0	0	14·5 S.	8·6	+1·12	148
+51 30	1257	16·7 E.	25·3	+2·35	218
0	0	16·3 S.	9·0	+1·05	122

The next series of results was obtained with a fresh bar of exactly the same size and temper as the preceding. To avoid an unnecessary occupation of space, I shall omit the fresh heading of the table for a change in the direction of the current, and simply designate the commencement of a new condition by ruling a line.

Experiment 2.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of bar.	Total elongation.	Magnetic intensity of bar.	Square of magnetic intensity divided by total elongation.
+ 5 0	87	0·1 E.	0·1	+0·08	64
0	0	0	0·1	+0·03	9
+ 8 27	148	1·9 E.	2·0	+0·50	125
0	0	1·0 S.	1·0	+0·30	90
+13 27	239	5·8 E.	6·8	+1·16	198
0	0	3·1 S.	3·7	+0·69	129
+33 50	670	18·8 E.	22·5	+2·20	215
0	0	14·3 S.	8·2	+1·01	124
+53 50	1368	19·0 E.	27·2	+2·32	198
0	0	17·1 S.	10·1	+1·03	105
— 7 5	124	2·0 S.	8·1	—0·15	3
0	0	0·1 S.	8·0	—0·07	0
—55 10	1437	20·0 E.	28·0	—2·20	173
0	0	14·6 S.	13·4	—1·39	144

The next experiment was with a bar of well-annealed iron, one yard long and about half an inch square. Its weight was  $45\frac{1}{2}$  oz. I have introduced an additional column into the table, in order to reduce the action on the magnetic balance to the section of the former bars, the weight of each of which was 8 oz.

## Experiment 3.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of bar.	Total elongation.	Magnetic intensity of bar.	Magnetic intensity per unity of section.	Square of magnetic intensity per unity, divided by elongation.
+ 5 10	90	0.4 E.	0.4	+ 1.18	0.21	110
0	0	0.1 S.	0.3	+ 0.45	0.08	21
+ 8 2	141	0.7 E.	1.0	+ 1.82	0.32	102
0	0	0.2 S.	0.8	+ 0.67	0.12	18
+14 43	262	2.0 E.	2.8	+ 4.10	0.72	185
0	0	1.0 S.	1.8	+ 0.90	0.16	14
+40 3	840	12.0 E.	13.8	+11.08	1.95	275
0	0	8.4 S.	5.4	+ 1.20	0.21	8
+54 0	1376	13.8 E.	19.2	+13.53	2.38	295
0	0	12.0 S.	7.2	+ 1.20	0.21	6
+62 5	1887	14.4 E.	21.6	+14.13	2.48	285
0	0	13.5 S.	8.1	+ 1.20	0.21	5
- 6 30	114	1.2 S.	6.9	- 0.70	0.12	2
0	0	0	6.9	- 0.30	0.05	0
-14 25	257	0.7 E.	7.6	- 3.80	0.67	59
0	0	1.3 S.	6.3	- 1.15	0.20	7
-41 15	877	11.0 E.	17.3	-11.33	1.99	229
0	0	8.8 S.	8.5	- 1.50	0.26	8
-62 45	1941	16.0 E.	24.5	-13.71	2.41	237
0	0	13.0 S.	11.5	- 1.55	0.27	6
+ 5 35	98	0.8 S.	10.7	+ 0.16	0.03	0
0	0	0	10.7	- 0.40	0.07	0
+ 9 0	158	0.2 S.	10.5	+ 1.17	0.21	4
0	0	0.2 S.	10.3	+ 0.15	0.03	0
+14 20	255	0.3 E.	10.6	+ 3.30	0.58	32
0	0	1.2 S.	9.4	+ 0.50	0.09	1
+24 45	461	3.3 E.	12.7	+ 7.16	1.26	125
0	0	3.4 S.	9.3	+ 0.82	0.14	2
+39 50	834	9.6 E.	18.9	+11.43	2.01	214
0	0	8.0 S.	10.9	+ 0.95	0.17	2
+54 15	1389	12.6 E.	23.5	+13.47	2.37	239
0	0	11.6 S.	11.9	+ 1.00	0.18	3
+60 45	1785	13.2 E.	25.1	+13.84	2.43	235
0	0	12.4 S.	12.7	+ 1.00	0.18	3



## Experiment 3 (continued).

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of bar.	Total elongation.	Magnetic intensity of bar.	Magnetic intensity per unity of section.	Square of magnetic intensity per unity, divided by elongation.
- 7 13	127	1.0 S.	11.7	- 1.13	0.20	3
0	0	0.1 S.	11.6	- 0.50	0.09	1
-10 25	184	0	11.6	- 2.16	0.38	12
0	0	0.2 S.	11.4	- 1.00	0.18	3
-15 57	286	0.5 E.	11.9	- 4.14	0.73	45
0	0	1.1 S.	10.8	- 1.25	0.22	4
-26 0	488	3.5 E.	14.3	- 7.45	1.31	120
0	0	3.2 S.	11.1	- 1.50	0.26	6
-40 55	867	9.6 E.	20.7	-11.48	2.02	197
0	0	8.0 S.	12.7	- 1.70	0.30	7
-62 48	1946	14.6 E.	27.3	-13.76	2.42	214
0	0	13.0 S.	14.3	- 1.73	0.30	6

From the last column of each of the preceding tables we may, I think, safely infer that *the elongation is in the duplicate ratio of the magnetic intensity of the bar*, both when the magnetism is maintained by the influence of the coil, and in the case of the permanent magnetism after the current has been cut off. The discrepancies observable will, I think, be satisfactorily accounted for when we consider the nature of the magnetic actions taking place. When a bar experiences the inductive influence of a coil traversed by an electrical current, the particles near its axis do not receive as much polarity as those near its surface, because the former have to withstand the opposing inductive influence of a greater number of magnetic particles than the latter. This phenomenon will be diminished in the extent of its manifestation with an increase of the electrical force, and will finally disappear when the current is sufficiently powerful to saturate the iron. Again, when the iron, after having been magnetized by the coil, is abandoned to its own retentive powers by cutting off the electrical current, the magnetism of the interior particles will suffer a greater amount of deterioration than that of the exterior particles. The polarity of the former may indeed be sometimes actually reversed, as Dr. Scoresby found it to be in some extensive combinations of steel bars. Now whenever such influences as the above occur, so as to make the different parts of the bar magnetic to a various extent, the elongation will necessarily bear a greater proportion to the square of the magnetic intensity measured by the balance than would otherwise be the case.

For similar causes the interior of the bar will in general receive the neutralization and reversion of its polarity before the exterior, and hence we see in the tables that there is a considerable elongation of the bar after the reversion of the current; even when the effect upon the balance has become imperceptible, owing to the opposite effects of the interior and exterior magnetic particles.

The bars employed in the preceding experiments were annealed as perfectly as possible. The next series was made with a bar of exactly the same dimensions and quality as the bars employed in Experiments 1 and 2, excepting that it was not annealed.

#### Experiment 4.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of bar.	Total elongation.	Magnetic intensity of bar.	Square of magnetic intensity divided by total elongation.
+ 9° 20'	164	0·2 E.	0·2	+0·15	112
0	0	0	0·2	+0·08	32
+15 20	274	0·9 E.	1·1	+0·50	227
0	0	0·7 S.	0·4	+0·33	272
+38 32	796	7·1 E.	7·5	+1·36	247
0	0	5·2 S.	2·3	+0·80	278
+50 30	1213	10·2 E.	12·5	+1·76	247
0	0	9·6 S.	2·9	+0·97	324
+57 40	1580	13·0 E.	15·9	+1·94	236
0	0	11·8 S.	4·1	+1·00	244
+62 20	1907	14·0 E.	18·1	+2·10	243
0	0	14·0 S.	4·1	+1·01	249
- 6 50	120	1·2 S.	2·9	+0·58	116
0	0	0	2·9	+0·65	145
-10 35	168	0·4 S.	2·5	+0·21	17
0	0	0	2·5	+0·35	49
-14 57	267	0	2·5	-0·30	36
0	0	0·2 S.	2·3	-0·13	7
-40 10	844	5·7 E.	8·0	-1·36	231
0	0	4·6 S.	3·4	-0·88	228
-53 30	1351	10·0 E.	13·4	-1·70	215
0	0	9·5 S.	3·9	-0·95	231
+ 9 27	166	1·3 S.	2·6	-0·36	50
0	0	0·1 E.	2·7	-0·40	59
+22 30	414	0·1 S.	2·6	+0·38	55
0	0	0	2·6	+0·22	18
+38 27	794	4·9 E.	7·5	+1·50	300
0	0	4·6 S.	2·9	+0·97	324

In the foregoing series the discrepancies before adverted to do not make their appearance to any considerable extent, except in the case of the reversion of the magnetic polarity. Taken altogether the series is strikingly confirmatory of the law of elongation already announced.

The next series of observations was obtained with a piece of soft steel wire one yard long and a quarter of an inch in diameter. Its weight being exactly 8 oz., no reduction of magnetic intensity had to be made for unity of section.

## Experiment 5.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of bar.	Total elongation.	Magnetic intensity of bar.	Square of magnetic intensity divided by total elongation.
+38° 10'	786	1·4 E.	1·4	+0·94	631
0	0	0·6 S.	0·8	+0·65	528
+50 45	1224	2·8 E.	3·6	+1·43	568
0	0	1·8 S.	1·8	+0·98	533
+60 25	1761	3·8 E.	5·6	+1·71	521
0	0	3·1 S.	2·5	+1·12	502
+67 50	2454	5·0 E.	7·5	+1·88	471
0	0	4·5 S.	3·0	+1·23	504
+69 20	2651	5·5 E.	8·5	+1·97	456
0	0	4·5 S.	4·0	+1·28	409
-41 40	890	1·3 S.	2·7	-0·76	214
0	0	1·0 S.	1·7	-0·35	72

Experiments 6 and 7 were made with fresh steel bars of precisely the same sort as that employed in the last series.

## Experiment 6.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of bar.	Total elongation.	Magnetic intensity of bar.	Square of magnetic intensity divided by total elongation.
+38° 25'	793	0·8 E.	0·8	+0·78	760
0	0	0·5 S.	0·3	+0·46	705
+60 50	1792	5·2 E.	5·5	+1·60	466
0	0	3·4 S.	2·1	+0·99	467
+70 30	2824	7·0 E.	9·1	+1·88	388
0	0	5·8 S.	3·3	+1·16	408
-16 28	295	1·8 S.	1·5	+0·82	448
0	0	0·2 S.	1·3	+0·94	680
-38 50	805	0	1·3	-0·64	315
0	0	0·3 S.	1·0	-0·33	108

Experiment 7.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of bar.	Total elongation.	Magnetic intensity of bar.	Square of magnetic intensity divided by total elongation.
+38 26 0	790 0	1.4 E. 0.7 S.	1.4 0.7	+0.74 +0.46	391 302
+61 5 0	1810 0	5.3 E. 3.2 S.	6.0 2.8	+1.64 +1.07	448 409
+69 55 0	2735 0	4.7 E. 4.5 S.	7.5 3.0	+1.90 +1.20	481 480
-26 40 0	502 0	3.0 S. 0.2 S.	0 -0.2	+0.20 +0.32	

The uniformity of the numbers in the last columns of the preceding tables shows that where the metal possesses a considerable degree of retentive power the anomalies occasioned by the reaction of the magnetic particles upon one another, which have been already adverted to, do not exist to any considerable extent, and presents an additional confirmation of the law I have stated, viz. *the elongation is proportional, in a given bar, to the square of the magnetic intensity.*

I now made trial of a bar of steel one yard long, half an inch broad, and a quarter of an inch thick, weighing 23 oz. It was hardened to a certain extent throughout its whole length, but not to such a degree as entirely to resist the action of the file.

Experiment 8.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of bar.	Total elongation.	Magnetic intensity of bar.	Magnetic intensity per unity of section.	Square of magnetic intensity per unity, divided by elongation.
+39 6 0	810 0	0 0.2 E.	0 0.2	+1.11 +1.36	0.38 0.47	1104
+52 35 0	1307 0	0.8 E. 0.3 E.	1.0 1.3	+4.09 +2.85	1.42 0.99	2016 754
+60 15 0	1750 0	0.5 E. 0.1 E.	1.8 1.9	+5.10 +3.52	1.77 1.22	1740 783
+69 45 0	2710 0	0.6 E. 0.2 E.	2.5 2.7	+5.91 +4.20	2.06 1.46	1697 790
-41 15 0	877 0	1.6 S. 0.1 E.	1.1 1.2	-0.43 +0.35	0.15 0.12	20 12
-56 5 0	1487 0	1.4 E. 0.1 E.	2.6 2.7	-3.90 -2.63	1.36 0.91	711 307



In the above table it will be observed that the hard steel bar was slightly increased in length every time that contact with the battery was broken, although a considerable diminution of the magnetism of the bar took place at the same time. I am disposed to attribute this effect to the state of tension in the hardened steel, for I find that soft iron wire presents a similar anomaly when stretched tightly.

On inspecting the tables, it will be remarked that the elongation is, for the same intensity of magnetism, greater in proportion to the softness of the metal. It is greatest of all in the well-annealed iron bars, and least in the hardened steel. This circumstance appears to me to favour the hypothesis that the phænomena are produced by the attractions taking place between the magnetic particles of the bar, an hypothesis in perfect accordance with the law of elongation which I have pointed out.

[To be continued.]

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XVIII. *Remarks on the Existence and Mechanism of the Negative or Protecting Rays of the Sun.* By JOHN WILLIAM DRAPER, M.D., Professor of Chemistry in the University of New York\*.

**I**N a letter published in the Philosophical Magazine, Nov. 1842, I had occasion to make some incidental remarks respecting a class of rays existing in the sun-light, which have the quality of exerting a negative or antagonizing action upon those engaged in producing Daguerreotype results.

In October last, MM. Foucault and Fizeau having made a communication to the French Academy of Sciences to a similar effect, and M. Edmond Becquerel, in criticising their results, having referred to me as the original author of the fact, I may on this occasion be excused for offering a few observations on this, which perhaps is destined to become one of the most important phænomena in relation to the chemical action of the sun-light.

That the opposite ends of the solar spectrum possess opposite qualities is an idea which, far from originating with me, has been floating among chemists for many years. The first distinct statement with which I am acquainted in relation to it occurs in a work published by Mr. B. Wilson, the second edition of which dates as early as 1776. It is entitled "*A Series of Experiments on Phosphori.*" He shows that it is the more refrangible rays which excite the phosphorescence of sulphuret of lime, *but the less refrangible ones extinguish it when shining.*

\* Communicated by the Author.

In 1801 Ritter found that chloride of silver which had been blackened in the violet rays, had its colour partially restored when placed in the red. He states also that phosphorus, which is oxidized with the production of fumes in the invisible red, is instantly extinguished in the violet.

The well-known experiments of Wollaston with guaiacum served to show the opposite relations of the red and violet rays. It is remarkable, that he subsequently abandoned this interpretation of the phænomenon, on discovering that green guaiacum changed its colour by the application of a hot silver spoon.

In 1839 Sir J. Herschel encountered the same action in the case of some of the preparations of silver. His first idea was that of a positive and negative polarity of the spectrum; but this was subsequently modified for the reasons set forth in his memoir\*.

From the time when I examined the Daguerreotype spectrum in Virginia in 1842, I have never doubted the actual existence of these negative or protecting rays; and on this occasion, when that existence is re-asserted by Lerebours, Fizeau and Foucault, I will make known certain new facts, premising that I do not think the views taken by M. Becquerel are correct. They are founded on what seems to me to be a misapprehension of the phænomenon of the Daguerreotype.

A Daguerreotype plate can exhibit three different varieties of surface:—1st, a black aspect on those regions where it has been unaffected by light; 2nd, various shades of white; 3rd, a coloured blackness, the tint of which may be of a deep watch-spring, or sometimes of an olive shade. Persons familiar with the process will understand completely what I mean. The first of these conditions is represented in the deep shadows of such a photograph, the places where the light never acted; the second is exhibited in the various intensities of whiteness, which constitute the figures of the picture, the whiteness varying in intensity according to the intensity of the light; the third is the solarized or overdone condition, which arises from too long an exposure to the rays. Like the first this may be spoken of as a blackness, but in reality it is a dark green, or blue, or tawny tint. It is this solarized condition of surface which M. Becquerel confounds with the first, the blackness arising from the unchanged state; and it is precisely on this point that the whole argument turns. For the sake of having distinctive words to mark out these three conditions, I will call the first the unaffected state, the second the white state, and the third the solarized state.

\* Phil. Trans. 1840, § 60, &c.

The observations I made in Virginia were as follows. That if a solar spectrum was received on a Daguerreotype plate on which a weak daylight was simultaneously acting, the red, orange, yellow, green, and part of the blue rays, arrested the action of the daylight on that portion of the plate on which they fell, and maintained it in the unaffected state; whilst the residue of the blue, the indigo and violet, carried their part of the plate to a completely solarized condition. This therefore seemed to justify the assertion, that the less refrangible rays *protect* Daguerre's preparation from the action of a diffused daylight.

It was also found that if the plate was exposed to the daylight for a few seconds, so that had it been then mercurialized it would have whitened uniformly all over, on being made to receive the spectrum the less refrangible rays actually carried it back to the unaffected condition, reversing what had been already done. Whilst the more refrangible rays were forcing it on to the solarized state, these were returning it into the condition of shadow: they therefore not only *protect*, but seem even to exert a *negative* or antagonistic action.

The following observation appears to me to be in contrast with the beautiful explanation which Sir J. Herschel gives of the phænomena of one of these specimens, and moreover to have a very significant meaning. I found that it was perfectly immaterial whether the exposure to the spectrum was for thirty seconds or one hour—the result was the same. The final action had been produced, the less refrangible rays had carried their region to the unaffected state, while the more refrangible had solarized theirs. Now if the phænomenon was due, as M. Becquerel supposes, to an unequal action of the same kind in the different rays, it is obvious that the final result ought to depend on the time of exposure; the red ray, aided by the daylight, should carry its portion through the various shades of white, and solarize it at last. But this in the longest exposure never takes place; that part of the plate remains as though a ray of light had never fallen upon it.

Such are the facts I observed, and they seem to have been reproduced by MM. Foucault and Fizeau; but there are also others of a much more singular nature. In these Virginia specimens *the same protecting action reappears beyond the violet.*

The only impressions in which I have ever seen this protecting action beyond the violet, are those made in Virginia in 1842; they were made in the month of July. Struck with this peculiarity, on my return to New York the following August I made many attempts to obtain similar specimens, but in no in-

*Phil. Mag. S. 3. Vol. 30. No. 199. Feb. 1847.* H

stance could the extra-violet protecting action be traced, though the analogous action of the red, orange, yellow, green and blue, was perfectly given. Supposing, therefore, that the difference must be due either to impurities in the iodine or to differences in the method of conducting the experiment, I tried it again and again in every possible way. *To my surprise I soon found that the negative effect was gradually disappearing*; and on Sept. 29 it could no longer be traced, except at the highest part corresponding to the yellow and green rays. In December it had become still more imperfect, but on the 19th of the following March the red and orange rays had recovered their original protective power. It seemed, therefore, that in the early part of the year a protective action had made its appearance in the red ray, and about July extended over all the less refrangible regions, and as the year went on it had retreated upwards.

Are there then periodic changes in the nature of the sun's light? The absorptive action of the earth's atmosphere is out of the question: if that were the cause, the character of these spectrum impressions should vary with the hour of the day. Or is it not more probable that these singular phænomena rather depend on incidental changes in the experiment, such as external temperature, variations of moisture, the colour of the sky, &c.?

Under proper circumstances there is no difficulty in exhibiting the power which the less refrangible rays exert in arresting the action of the daylight: under such circumstances a Daguerreotype impression of the sun's spectrum yields all three of the varieties of surface before alluded to. The plate in the less refrangible and extreme violet region is unaffected; a narrow space of white separates these unaffected portions from the indigo and violet spaces, which are in a highly solarized state.

But a totally different result is obtained when the daylight is *not* allowed to fall on the plate, either before or during its exposure to the spectrum. Under these circumstances the rays which would otherwise protect now act on the plate and slowly whiten it. A Daguerreotype spectrum formed in *darkness* and without previous exposure to the light, exhibits a white stain over all the less refrangible regions, and bears a marked contrast to one formed under the simultaneous action of a weak daylight. For brevity I will call the former the spectrum of darkness, and the latter the spectrum of daylight. The following are some additional observations.

In the spectrum of darkness there is in the white stain a



point of maximum action. This corresponds with the maximum of protection in the spectrum of daylight.

The white stain of the spectrum of darkness is apparently narrower than the protected space in the spectrum of daylight.

Rays of luminous or of non-luminous heat projected on the darkness or daylight spectra during their formation appear to exert no kind of special influence on the result.

The white fringe which borders the solarized portion is not due to anything analogous to conduction. These chemical changes, unlike thermal changes, cannot be conducted.

By interposing between the prism and the Daguerreotype plate a convex lens of short focus, so as to intercept in succession each of the coloured rays, I threw all over the plate, while the spectrum was in the act of being impressed upon it, red, orange, yellow, &c. lights in succession; the object being to ascertain how far the impressed spectrum would change when these monochromatic rays were used along with daylight, Sir J. Herschel having previously showed in similar experiments that new phænomena arise during the conjoint action of rays\*. The following are some of the observations I made; their date is Sept. 24, 1842.

The red ray when projected increases the length of the solarized portion, and also of its white extremities.

The yellow ray shortens the solarized portion.

The green ray exerts a greater action of the same kind.

The indigo ray gives a most remarkable result. It totally inverts the action of the less refrangible rays; and they solarize the plate, acting precisely in the same way that the more refrangible rays commonly do, causing it to exhibit a watch-spring lustre.

I further found that when different rays are brought to act upon each other, the result does not alone depend upon their intrinsic differences, but also on their relative intensities. Thus the green and lower half of the blue rays, when of a certain intensity, protect the plate from the action of the daylight; but if of a less intensity, they aid the daylight.

The red and orange rays, when of a certain intensity, increase the action of daylight on the plate; but if of a less intensity, they restrain it.

These facts seem to be connected with the circumstance that there is often to be traced on Daguerreotype plates a remarkable difference between the central and lateral parts of a spectrum. Thus if a line be drawn through the centre of such a spectrum and a parallel to it on one of the edges, the action at any point on the central line is the reverse of that at

\* Phil. Trans. 1840, § 64.

the corresponding point on the edge. A similar remark, as respects impressions on paper, has been previously made by Sir J. Herschel.

Such are the chief facts I have observed in relation to the Daguerreotype spectrum. It would seem at first sight that their diversity is so great that we can have but little hope of reducing them to a common system of results originating in the same cause. I have however been long led to believe that the explanation is to be met with in the great and fertile principle of interference. From this point of view I regard the action of rays of every kind as being essentially positive, and that action mainly consists in impressing a vibratory movement on the atoms of the decomposing substance. It is to my mind a fact of no common significance, that in those Virginia specimens the places of maximum protection in the less and more refrangible regions fall where the lengths of the luminous waves have the extraordinary relation of 2 : 1. Then, when we also see that, before a perfect neutralization of action between two rays ensues, those rays must be adjusted in intensity to each other, does it not show that interference of some kind is going on? Again, the yellow ray is in numberless instances the ray which most completely antagonizes those at the red and violet extremes of the spectrum: to use the language of Sir J. Herschel, "this ray may be considered as marking a sort of chemical centre, a point of equilibrium, or rather a change of action in the spectrum." I cannot avoid seeing that these phænomena are connected with the remarkable fact, that the waves of red, yellow and violet light, are of lengths which correspond to 2,  $1\frac{1}{2}$ , 1.

If then a powerful yellow ray can hold in check a feeble violet one, and prevent it from decomposing iodide of silver, merely because their relation of length is in the proportion of  $1\frac{1}{2}$  to 1, it should follow on the same principles, that a red ray acting conjointly with a violet should give rise to an increased effect, because the lengths have now become 2 : 1. And that this in reality is the case I found by direct experiment; for on projecting the red upon the violet, so that the colours should half overlap each other, I found that at the point of concurrence the plate instantly solarized, and assumed a splendid green metallic colour.

I have now explained the acceptance in which I receive the term *negative ray* as a synonym (in this instance of iodide of silver) for the yellow ray, and alluded to the mechanism which seems to be the cause of protecting action generally. Perhaps on a review of his own experiments M. Becquerel may find reason to believe that there are in reality antagonizing

actions in different parts of the spectrum; actions not limited to the Daguerreotype, but occurring in all kinds of cases. They have been met with by every one who has examined the spectrum with sensitive papers, and, in a totally different series of phænomena, M. Becquerel has himself furnished a most conclusive illustration. He shows that when sulphuret of lime and other phosphorescent bodies in a shining state are exposed to the spectrum, the more refrangible rays increase the glow, but the less extinguish it at once. What is to be made of a fact like that?

It is proper to observe, that some of the phænomena recorded in this communication which seem to be in opposition to the principle set forth are not so in reality. All reasonings founded on the decomposition of light by the prism, and the action of the prismatic spectrum on changeable surfaces, are liable to error. In a memoir which I shall soon publish "On the Production of Light by Heat," this point will be particularly dwelt on; for the present it is sufficient to say, that the only method free from these difficulties is to employ the interference spectrum formed by a ruled surface or a grate; a method which was proposed eight years ago by Sir J. Herschel with a view of getting rid of the disturbing agencies arising from the ideal coloration of glass, and which I first carried into effect in 1844 with so much success, that the resulting Daguerreotype impressions contained Fraunhofer's lines, even with microscopic minuteness. With this spectrum we avoid a far more serious difficulty than that of the ideal coloration of glass, a difficulty arising from the magnitude of the refracting faces of the prism. It is this which makes a prismatic spectrum blacken paper, made sensitive with the bromide of silver, from the red to the violet end; whereas the interference spectrum shows that the true action is confined to the more refrangible side, and stops short of the centre of the yellow space.

University, New York,  
Dec. 24, 1846.

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XIX. *Note to a former paper on the Theory of the Aberration of Light. By the Rev. Professor POWELL.*

SINCE the publication of my paper in a late Number of this Journal\*, I have been favoured with several communications from the distinguished individuals on whose opinions I have there commented; and their friendly criticisms seem to call for a brief supplementary remark.

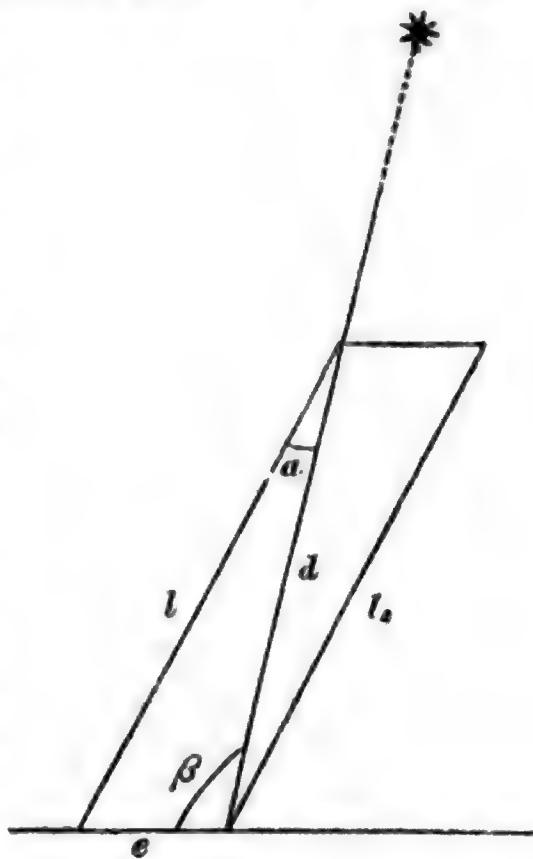
• Phil. Mag., Dec. 1846.

In the view which I gave in that paper of the explanation of the aberration, my object was to put the principle proposed by Prof. Challis in such a form as I conceived might divest it of certain difficulties which had appeared to some to attach to it.

I had in fact attempted to *combine* Prof. Challis's principle with what seemed necessary for explaining the direction of vision. But that principle is in itself quite independent of this last consideration; and the whole may thus be more distinctly expressed by keeping them separate as follows:

Supposing the same construction as in my former paper, fig. 2, then—

(1.) The observer being at the bottom of  $l$ , let the ray from the star have arrived at the top of  $l$ , which point may represent the place of the telescope-wire, or any other terrestrial object to which the star is referred, and from which rays emanate in all directions. Of these, that one which proceeds in the direction  $d$  will pursue that direction along with the ray from the star, and will in *every respect* coincide with it; and they will arrive together at the bottom of  $d$  at the same time as that at which the observer's eye arrives there. Both objects will consequently be referred to the *same* direction, though the wire has really moved into a new position, viz. to the top of  $l_2$ .



This is all that is essential to the idea of aberration according to Prof. Challis's view. It is the angular difference  $\alpha$  between these two directions which is defined to be aberration.

(2.) *What* the direction is, to which the *eye* actually refers the star and the wire, is a distinct question. By some it may be regarded as altogether needless; and so I admit it is as far as the above abstract idea of aberration is concerned: yet it seems to me at least desirable to take it into account; and it may, I conceive, be best put as follows:—

While the rays come down  $d$  the telescope has moved along  $e$  parallel to itself, into the position  $l_2$ , *independently* of the ray which had started from the wire down  $d$ : yet at every



instant of its course that ray has one point coinciding with a point in the axis of the tube in motion. Thus by composition of the two motions, this ray, though by a different *modus operandi*, comes down the axis of the telescope: whether this will, in theory, be the direction in which the eye sees both the star and wire, will depend on the consideration of a similar question to that stated in p. 433.

But there can be no doubt that in point of fact the axis of the telescope *is* the direction to which the eye (by whatever means) is led to refer the two objects seen together by rays pursuing a different route, relatively to space.

Prof. Challis has favoured me with the following additional illustration.

“Suppose two ships to be sailing due north at the same rate, and one to be due east of the other: a boat is despatched from the eastern ship to the western, and goes in a *rectilinear* course from the one to the other. If the crew of the western ship, seeing the other ship due east, and knowing that the boat came from her, concluded that the course of the boat had been exactly from east towards west, they would plainly be mistaken. It is precisely this error that the astronomer commits.”

Among some valuable remarks on the entire subject which I have received from Mr. Stokes, I learn that the very cursory statement I made as to the difference between the theoretical investigations of that gentleman and Prof. Challis, does not convey a perfectly accurate idea of the point at issue; I am therefore anxious to represent it more correctly as follows:—

Mr. Stokes at first contended that it was necessary to suppose the expression ( $u dx + \&c.$ ) to be an exact differential, while Prof. Challis rejected that restriction. Subsequently however Prof. Challis agreed in Mr. Stokes's view: but he showed that, on that supposition, the expression for the change in the direction of the normal is such as to *allow us* to suppose the motion of the æther at the surface of the earth relatively to the earth to be of *any* amount. But Mr. Stokes differs in opinion as to the *fact*; conceiving it most probable that the æther within the earth and close to its surface is at rest relatively to it.

In my paper however I professedly avoided entering on this part of the question; and the remark just made in no way affects the other parts of my observations.

XX. *Note on a Class of Differential Equations.*

By GEORGE BOOLE, Esq.

*To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,

THE following may be deemed a proper supplement to the remarks, offered in common by Mr. Hargreave and myself, in the January Number of the *Philosophical Magazine* on the subject of the Rev. B. Bronwin's differential equations.

In the greater number of those equations, the independent variable  $x$  only entered in the first degree. All linear equations which possess this character, whether in differentials or in finite differences, admit of a complete symbolical solution, from which the particular solutions assigned in the papers referred to, may be regarded as deductions.

Every equation, indeed, to which this characteristic belongs, may be expressed in the form

$$x \phi(D)u + \psi(D)u = X; \quad . . . . . (1.)$$

$D$  standing for  $\frac{d}{dx}$ ,  $X$  being a function of  $x$ , and  $\phi$  and  $\psi$  denoting arbitrary functions or combinations of the symbol to which they are affixed.

The complete solution of (1.) is

$$u = \{\phi(D)\}^{-1} \epsilon^{\chi(D)} x^{-1} \epsilon^{-\chi(D)} X, \quad . . . (2.)$$

the form of  $\chi(D)$  being given by the equation

$$\chi(t) = \int \frac{\psi(t)}{\phi(t)} dt.$$

The analogy which exists between the above solution and that of the linear differential equation of the first order, it is scarcely necessary to notice. It belongs to a class of subjects which have been considered in a paper on the *Theory of Developments*, published in the *Cambridge Mathematical Journal*, vol. iv. p. 214.

As a particular illustration of (2.), let us suppose that the given equation is of the form

$$x \phi(D)u + m \phi'(D)u = X.$$

We have

$$\chi(t) = \int \frac{m \phi'(t)}{\phi(t)} dt = m \log \phi(t),$$

$$\epsilon^{\chi(D)} = \{\phi(D)\}^m. \quad \epsilon^{-\chi(t)} = \{\phi(D)\}^{-m};$$

$$\therefore u = \{\phi(D)\}^{m-1} x^{-1} \{\phi(D)\}^{-m} X,$$

from which the solution of most of the equations considered by Mr. Bronwin will obviously follow. There are however various other cases in which the general solution is interpretable.

I remain, Gentlemen,  
Your obedient Servant,

Lincoln, Jan. 1, 1847.

GEORGE BOOLE.

XXI. *Observations and Experiments respecting the Origin of the Voltaic Current.* By PROFESSOR GIANALESSANDRO MAJOCCHI\*.

AFTER the discussion which took place in the last sittings, and principally in that of yesterday, respecting the origin of voltaic electricity, among some of our distinguished physicists, and particularly between the illustrious president of the section Prof. Orioli and Prof. Botto, I have been induced to extract the following paper from an essay of mine on the same subject.

At the Turin Congress in 1840, the question of the two theories, the *chemical* and that of *contact*, was agitated, with a view to explain the origin of voltaic electricity. There is no doubt that where a chemical action takes place, there is also a development of electricity; but the fluid which becomes free requires certain conditions to form a current. In like manner it is certain that, on placing two heterogeneous bodies in contact, there is a development of electricity; and in this precisely consist the fundamental facts which led Volta to the discovery of his pile; and on the same principle is founded the ingenious apparatus of insulating plates, armed with leaves of heterogeneous metals, which our colleague Prof. Marianini exhibited at the Congress of Turin, and which, variously arranged, he yesterday showed to this assembly†. The chemico-electricians maintain that the fundamental facts of Volta depend on the chemical action upon the metal, produced by the moisture of the hand, of the air, &c.; but the careful experiments instituted by Marianini, Pfaff, Belli, Peltier, and some other physicists, appear, in the development of electri-

\* Translated from the *Annali di Fisica, Chimica e Matematiche*, and communicated by the Author.

The chief portion of this paper was read by the author at the Congress of Milan, Sept. 27, 1844. See *Annali*, t. xvi. p. 120; and *Atti della sesta Riunione degli Scienziati Italiani*, p. 118-119.

† It is mentioned in his work *Memorie di Fisica sperimentale*, Modena, 1838, and in another work printed at Modena under the same title in 1841, in which is contained the *Memorie sui coibenti armati che si caricano da sé col mutuo contatto d' un' armatura coll' altra*.

city by Volta's two dissimilar metals, to exclude any cause foreign to that which arises from the contact of the two bodies. Moreover, the period of thirty-three years, for which Prof. Zamboni's dry pile has now existed, confirms such a mode of electrical disturbance\*.

How then does electricity by contact take place? Contact is not a force; it is the simple indication of the mode in which metals arrange themselves in the case of the voltaic experiment; and no fluid whatever, the electric for instance, can be put in motion without an agent. At the Turin Congress in 1840 I expressed my opinion, that in the so-called contact, although the action cannot be considered chemical, there is a force of attraction which is the principle of the action itself, I mean that force recognised and admitted by all, investigated by many at the close of the last century and the beginning of the present, that force, namely, which is called adhesion or attraction of simple surfaces. I shall not repeat here what I said on that occasion†; but will rather observe, that such facts have confirmed me in my opinion concerning the phenomenon of electricity in the fundamental experiments of Volta.

My purpose here is to examine the conditions necessary in the pile to the generation of the current, which is quite distinct from the simple electrical disturbance in the fundamental experiments of Volta. In a circuit, in order to give birth to the electric current, it is necessary that there should be two forces; one to emit the electric fluid in a given direction, the other to carry it on from the place where it is lodged: these two forces in Volta's pile are,—the one, adhesion, by which the direction of the current is determined; the other, the chemical action which takes place in electrolyzation, or in other terms, in the decomposition of the intermediate liquid by which the electricity is evolved. It matters little whether this decomposition is perceptible or imperceptible; since the polarization of two platinum electrodes intended to complete the circuit with the intermediate electrolyte, proves that, if there is no perceptible separation in the components of the liquid, there is an imperceptible one; for the polarity of the electrodes themselves cannot happen without the decomposition of the liquid in which they are immersed. The above-named conditions are verified in any voltaic arrangement, both in the original piles of Volta, and in those of Wollaston, Novellucci, Miccheliotti, Daniell, Grove, &c. In the fundamental experiments

\* See *Annali*, t. viii. p. 14.

† See the *Annali*, t. i. p. 45; and the Proceedings of the Second Meeting of Italian Savans, p. 14.



of Volta there is no chemical decomposition, there being no intermediate electrolyte; and thence an electrical disturbance only is shown, an electricity of tension, which to be formed into a current requires the circuit to be completed with a liquid. For if this liquid does not exert any chemical action in any of the metals placed in contact, then no current is manifested on the galvanometer. Gold and platinum show an electrical disturbance on the condenser, but no current when they are immersed in nitric acid, and united with one another *metallically* by means of the wire of the galvanometer. But if some drops of hydrochloric acid are added to the nitric acid, the gold only is attacked, and immediately an electric current is manifested on the galvanometer. In the chemical action of the acid upon gold or on the zinc of the common voltaic pair, electricity is evolved; or this fluid, by virtue of that force, is separated and set free from the ponderable molecules which undergo a decomposition to form a new compound, and the adhesion between the two bodies in contact, which would produce a simple electrical disturbance, gives the discharge and determines the course in which the current of the fluid is directed precisely according to the chemical action. In the circuit of the battery between every pair, where the electricity is simply disturbed and would show only a tension, an electrolyte is placed, which is decomposed by the affinity which one of the metals of the pair exerts upon it: by this decomposition the electricity of the ponderable matter is developed and becomes free, and is emitted from the electro-negative metal into that of the electro-positive. This discharge proceeds from one pair to another, and the electric fluid set free by the chemical action is put into a continuous current along the circuit. From the first experiments of Volta, and from those which have been since instituted in various ways, it is known that the chemical action alone occasions a development of electricity, and a second force is necessary to put this fluid in motion. Moreover we know that, if this chemical action is impeded without taking away the conductivity from the circuit, the current is weakened, and is sometimes in the end entirely destroyed, not being perceptible by the most delicate galvanometers. In a voltaic circuit, where two platinum electrodes are interposed, immersed at a distance from each other in acidulated water, the hydrogen gas is deposited upon one of these, polarizes it, and generates a contrary current, which weakens or annuls the primary current, because, whilst the chemical action evolving the electric fluid still exists in the pairs of the pile, there is opposed to the impelling force, by which the current is determined, another contrary one, arising

100

with the polarization of the platinum electrode, which weakens or entirely annuls the effect of the first. For if the liquid is placed together with the two platinum electrodes, by which the voltaic circuit is completed, under the receiver of the air-pump where a vacuum is effected, the hydrogen gas is forced to diffuse itself by its expansibility in the same receiver, whereby the negative electrode is not polarized, and the electric current continues its course, until the gas can in such manner separate itself from the surface of the platinum, and diffuse itself in the vacuum of the air-pump.

With a view to ascertain whether this mode of explaining the phenomenon of the current in the battery has any probability and contains the cause of the same, it occurred to me to form an entirely metallic circuit, without any intermediate electrolyte, to examine if the simple chemical action was able of itself to generate a current. I shall first observe, that an homogeneous metallic wire, forming a continuous circuit and returning into itself, and perfectly identical throughout its whole extension, if heated in any part, gives rise to no electrical current; because the propagation of the caloric, proceeding uniformly from the two sides of the portion heated, gives rise to two electrical currents in an opposite direction, which become perfectly equal, and thence counteract one another, and obstruct the continuous circulation of the fluid itself. And if any obstacle, as a knot or a defect of homogeneity in the wire, renders the propagation of the caloric more difficult in one direction than in another, then one of the electrical currents which arise prevails over the other, and one results of intensity equal to the difference of the two unequal elements, which shows itself precisely on the galvanometer. In the same way, if the homogeneous metallic wire, forming the closed and re-entering circuit, is afterwards touched by a cold body in a point near the heated part, an electrical current is generated, which is indicated by the galvanometer. It seems then that the cooling produced on one side of the heated part weakens the cause from which the electric current springs, and thence gives room to the other to circulate in the wire and make its effects perceptible on the galvanometer. This case is very different from that in which the thermo-electric circuit is composed of two metals; the one positively electric and the other negatively electric; since then there are two forces from which the electric current springs in a given direction.

Following this principle, applied to the development of electricity by chemical action, I reasoned thus:—If the chemical action were able of itself to generate a current without the aid of another force, it is clear that, on forming a wholly

metallic circuit in one part of which a chemical action is by any matter produced, there ought to arise, in the same circumstances of the wire above described, an electric current. Before entering on experiments on this point, I foresaw that no current would be caused in that entirely metallic circuit, notwithstanding that from one side of the place where the chemical action would be exerted, an obstacle should be interposed by any heterogeneity in the conductor forming the circuit.

*First experiment.*—A strip of copper of the length of 1·30 metre, and 2 centimetres wide, is united by its extremities to the terminals of a long wire-galvanometer; afterwards with those of another with a short wire, and successively with my universal galvanometer disposed as a duplicator. By means of nitric, sulphuric, or hydrochloric acid, solutions of potass, soda or of ammonia, a chemical action was successively exerted in one part of this metallic band; but no indication of current appeared in the needle of these instruments. The experiment was made in a like manner with a similar strip of iron, zinc, and any other metal, and no current appeared which caused the needle to deviate on either of the galvanometers. In this experiment therefore the circuit was quite homogeneous, proving that the thermo-electric current originated from the different conditions of the wire.

*Second experiment.*—The metallic strip is compressed by means of a clamp in one place, then it bends again upon itself in many folds, being held by a clamp; then it is subjected in that place again to tempering, and then it is also hammered in a cold state: in each of these cases, operating in the same manner, a chemical action was exerted with one of the above-named materials in a portion of the strip without obtaining any indication of current on the needle of one of the galvanometers. The strip was divided into two parts, which were joined, bending back upon one another, and held fast together by the clamp: exciting afterwards a chemical action with one of the acids or the alkalies above-mentioned, there resulted no current perceptible by the galvanometer.

*Third experiment.*—The preceding copper strip is connected by means of the clamp with a similar one of zinc. These two strips thus united, by one of their extremities placed beneath, formed a continuous and wholly metallic circuit with the wire of the galvanometer. On causing a chemical action upon one of the strips, no current was manifested. The effect was null upon producing a chemical action on one of the metallic lists with an acid and on the other with an alkali, and also upon alternating this double action.



This third experiment shows that, although there may be disengagement of electricity in the chemical action of liquids upon metals, no current is manifested; because the other force necessary to the production of this exists indeed in the contact of the copper with the zinc; but it acts in two places in a contrary direction; whereas on the one hand the contact of the copper with the zinc tends to discharge the electricity in one direction, whilst on the other hand the contact of the same metals emits it in an opposite direction; and in this manner there are two contrary forces of adhesion, which, as far as regards disturbance of the electric equilibrium, destroy one another. Neither of them can therefore avail in setting in motion the electricity which is developed in the chemical action, by which this fluid is equally distributed throughout the metallic circuit, and in the liquid which is contained in the cup and exerts the chemical action on the metal.

Those who adopt the purely chemical theory, following De la Rive, in order to explain how the current originates in the battery of Volta at so weak a tension, admit, speaking according to the system of the dualists, that the two electricities separated by the chemical action tend to unite by their reciprocal attraction; and it is therefore that in this point, where there is a separation of the two electricities, they in fact unite again in part. The tension therefore does not become very great, because it is only the portions of the two electricities remaining separate that form themselves into a current. The neutralization or the equilibrium of the electricity evolved by chemical action, according to the followers of the pure chemical theory, does not occur at all by virtue of the action itself; whilst, according to us, this disturbance would be produced by a second force, which would be adhesion. Now those who adopt purely the theory of contact, it appears, see in the chemical action itself only a means of augmenting the points of contact of the liquid with the metal, by which the electricity finds various ways of disposing itself in a current. But how will they explain so many facts where the chemical action is manifest by the development of electricity? Moreover the simple combination of the metalloids chlorine, bromine, iodine, &c. with the metals is not accompanied by a development of the electric fluid; and it therefore seems that there is only a development of this fluid, when by affinity one of the elements of the voltaic pair tends to combine with one of the components the electrolyte or conductor of the second class.

The gas battery devised by Grove is easily explained in the above-described manner of considering the generation of



the electric current in the voltaic apparatus. For adhesion, we know, is not only manifested between solids, as the metals, but also between a solid and a liquid or aëriform fluid. And many facts of chemical reactions meet with easy explanation from such a force, by which affinity appears to be excited and thence the electrical current to arise, which determines a decomposition and a new combination. However long a time, in fact, a mixture of hydrogen and oxygen is left to itself, these gases do not combine even when placed in presence of bodies which have a great affinity for water, and which, it would seem, ought to produce their union, as for instance sulphuric acid, potass, lime and the like. We know that if a piece of platina be immersed in the mixture their combination follows on the metallic surface, and extends in certain cases to the whole aëriform mass, so as to cause the explosion of the mixture. Grove himself recognises an analogy in the phænomenon of his gas battery and that of the ordinary combination of the two gases, oxygen and hydrogen, by means of the presence of platina. The decomposition of ammoniacal gas by means of incandescent copper is one of the few instances in which the decomposition of a gaseous body is excited by a solid body. The attraction of gold for chlorine excites the decomposition of nitric acid united with hydrochloric acid when gold-leaf is placed in aqua regia; for aqua regia only contains free chlorine after being heated, or after having been left to itself for a long time. Numerous other facts of this nature have been collected by Mitscherlich\*, and related in the Reports of the Royal Academy of Berlin, of December 1841, and other similar ones by Reiset and Millon communicated to the Royal Academy of Paris in June 1843. All these facts are called by them *phænomena of contact*, and Mitscherlich calls the bodies by which they are produced *contact-substances*. Berzelius has introduced a new name into science, *catalytic force*, to denote the cause of adhesion; but with the new name he has not thrown any light upon the nature of this force. We see in all these facts the adhesion of a solid for a fluid bring into activity the affinity by which, when bodies are conveniently disposed in a complete circuit, electricity is evolved, which then forms a continuous current in the mode explained.

From all that we have said it would follow that, with a single force, mechanical, chemical or physical, there is only an electrical disturbance, or the phænomena of statical electricity: thus the mechanical action of friction produces in the ordinary machine a simple disturbance of electricity; the

\* See Taylor's Scientific Memoirs, Part XIII.

chemical action of an acid solution on a metal gives signs of electrical disturbance on the condenser; the physical force of magnetism produces by induction an instantaneous disturbance in the natural electrical fluid of a metallic wire, without setting it in a continuous current. But when two forces are in action, one of which is capable of disturbing the natural electricity of the ponderable matter, and the other of evolving it from the integrant molecules of the same, that fluid may be set into a continuous current in a complete circuit. Notwithstanding the appearance of truth in this mode of explaining the phenomenon of the continuous electric current, there are some facts which merit a deeper investigation, aided by experiment, before assigning it as the just cause of this phenomenon. A voltaic pair composed of two different metals, for example, gives an electric current in a given direction when it is immersed in one liquid, and the direction may be inverted when another different liquid is substituted for the first. The examination of facts of this kind will probably furnish materials for a second note.

XXII. *On the Equation of Continuity in Fluid Motion.* By ALFRED WILLIAM HOBSON, B.A. St. John's College, Cambridge\*.

THE proof of this equation given by the various authors of treatises on hydrodynamics is the same in all; in fact nearly every writer since Poisson has contented himself with a mere translation of his words in art. 648 of his *Traité de Mécanique*. As however I do not remember to have seen anywhere a statement of the reasons for the assumption from which the equation is obtained, the following investigation may be interesting to some, especially as the equation itself is found by a much shorter process than the usual one, and unencumbered with several steps which are perhaps not quite satisfactory to those reading the common method for the first time.

The fluid is supposed to be divided into small portions or 'elements,' each of which is acted upon by accelerating forces  $X, Y, Z$  parallel to the coordinate axes. The first thing to be remarked is that  $X, Y, Z$  must be the same *for the whole extent* of the element on which they are supposed to act, *i. e.* we must take the element so small that there is no variation in either  $X, Y$  or  $Z$  in passing from any one of its parts to another. Now since these forces are considered as functions of  $(x, y, z)$  varying for any, the slightest variation in either  $x, y$  or  $z$ , it is plain that if we consider the forces *mathematically*

\* Communicated by the Author.

continuous, *i. e.* as changing their value continuously for *any* change of  $x$ ,  $y$  or  $z$ , then in order for  $X$ ,  $Y$ ,  $Z$  to be the same for every part of an element, that element must be also considered a *mathematical* point, having no distinction of parts at all.

In nature, however, the forces are *physically* and not *mathematically* continuous, and the elements on which they act may have an infinite range of magnitude. The reasoning by which such forces are submitted to calculation is however precisely the same as if they were mathematically continuous.

The element of fluid, then, may be of any finite magnitude below a certain limit, and will consist of a number of ultimate particles, on each of which the forces acting are the same. For the whole extent of this element  $X$ ,  $Y$ ,  $Z$  are the same, and it may be taken of any size consistent with this condition.

The next thing to be attended to is, that the element must not be acted upon by any *new* forces during the instant ( $dt$ ). During this interval, therefore, every particle in the element is acted upon solely by the forces  $X$ ,  $Y$ ,  $Z$ , which vary or may vary with the time, but vary *similarly* for each of these particles. In other words, there is no *disturbing* force introduced during ( $dt$ ) into the system of ultimate particles, which together make up the element we are considering; therefore there can be no disturbance of the *relative positions* of these particles. Therefore the *number of particles* composing the element remains the same during the interval ( $dt$ ); and this condition expressed analytically gives us the equation of continuity.

Let  $\rho$  be the density of the element at the commencement of the time ( $dt$ ); this is supposed to be the same for the whole of the element;  $V$  the volume of the element at the beginning of ( $dt$ ) and  $V'$  at the end. We have then ( $\rho'$  being also the density at the end of the time ( $dt$ ))

$$\rho V = \rho' V'.$$

We have now merely to express this condition in another form. The notation is the same as Poisson's.  $V = (x' - x)(y' - y)(z' - z)$ : where for  $(x' - x)$  we may write  $\delta x$ , for  $(y' - y)$ ,  $\delta y$ , and for  $(z' - z)$ ,  $\delta z$ . We have to state in terms of the velocities ( $u$ ,  $v$ ,  $w$ ) parallel to the coordinate axes, that the variation of  $(x' - x)(y' - y)(z' - z)\rho$  is nothing during the instant ( $dt$ ). Now calling the whole differential coefficient of  $(x' - x)(y' - y)(z' - z)$  with regard to the time  $D_t V$ , and similarly for that of  $\rho$ ,

$$D_t V = (x' - x)(y' - y)(w' - w) + (x' - x)(z' - z)(v' - v) \\ + (y' - y)(z' - z)(u' - u)$$

$$\begin{aligned}
&= \delta x \cdot \delta y \cdot \left( \frac{dw}{dx} \delta x + \frac{dw}{dy} \delta y + \frac{dw}{dz} \delta z \right) + \delta x \cdot \delta z \left( \frac{dv}{dx} \delta x + \frac{dv}{dy} \delta y + \frac{dv}{dz} \delta z \right) \\
&\quad + \delta y \cdot \delta z \cdot \left( \frac{du}{dx} \delta x + \frac{du}{dy} \delta y + \frac{du}{dz} \delta z \right) \\
&= \delta x \cdot \delta y \cdot \delta z \left[ \frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz} \right] \\
&\quad + \delta x \cdot \delta y \cdot \delta z \cdot \left[ \frac{du}{dy} \cdot \frac{\delta y}{\delta x} + \frac{du}{dz} \cdot \frac{\delta z}{\delta x} + \frac{dv}{dx} \cdot \frac{\delta x}{\delta y} + \frac{dv}{dz} \cdot \frac{\delta z}{\delta y} + \frac{dw}{dx} \cdot \frac{\delta x}{\delta z} \right. \\
&\quad \left. + \frac{dw}{dy} \cdot \frac{\delta y}{\delta z} \right].
\end{aligned}$$

$$D_t \rho = \frac{d\rho}{dt} + u \cdot \frac{d\rho}{dx} + v \cdot \frac{d\rho}{dy} + w \cdot \frac{d\rho}{dz};$$

∴ we have  $D_t(\rho V) = 0$ , or  $\rho \cdot D_t V + V \cdot D_t \rho = 0$ ;  
or

$$\begin{aligned}
&\rho \cdot \delta x \cdot \delta y \cdot \delta z \cdot \left( \frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz} \right) + \rho \cdot \delta x \cdot \delta y \cdot \delta z \cdot \left( \frac{du}{dy} \cdot \frac{\delta y}{\delta x} + \frac{du}{dz} \cdot \frac{\delta z}{\delta x} + \&c. \right) \\
&\quad + \delta x \cdot \delta y \cdot \delta z \left( \frac{d\rho}{dt} + u \cdot \frac{d\rho}{dx} + v \cdot \frac{d\rho}{dy} + w \cdot \frac{d\rho}{dz} \right) = 0.
\end{aligned}$$

Now since there is no necessary connexion between  $\delta x$ ,  $\delta y$  and  $\delta z$ , the middle term in this equation, viz. that containing the quantities  $\frac{\delta y}{\delta x}$ , &c., cannot affect the other two: according therefore to a well-known principle, the two parts must separately = zero.

Therefore, finally, dividing out by the common factor  $\delta x \cdot \delta y \cdot \delta z$ , we get the usual equation of continuity,

$$\rho \left( \frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz} \right) + \frac{d\rho}{dt} + u \cdot \frac{d\rho}{dx} + v \cdot \frac{d\rho}{dy} + w \cdot \frac{d\rho}{dz} = 0;$$

or

$$\frac{d\rho}{dt} + \frac{d(\rho u)}{dx} + \frac{d(\rho v)}{dy} + \frac{d(\rho w)}{dz} = 0.$$

It is obvious that this equation only requires that the *number* of particles in the element should remain unaltered during ( $dt$ ); but the mode in which it is obtained implies, not only an identity in the *number* of particles at the beginning and end of ( $dt$ ), but an identity of *the particles themselves*. This observation has also been made by Poisson, whose words are (art. 651), “c’est pour abréger que l’on a considéré le volume de cette partie du fluide comme infiniment petit; et si l’on divise



le volume total en parties de grandeur finie, mais insensible, dont chacune renferme néanmoins un nombre extrêmement grand de molécules l'équation [of continuity] exprime réellement *que chacune de ces parties renferme toujours les mêmes molécules*, et que par conséquent sa masse est invariable."

In all cases where new forces are being continually introduced, as in the flowing of rivers or of water through pipes of variable form, it is impossible to calculate the motion, and at the same time the equation of continuity is no longer applicable. But, on the other hand, there are cases in which this equation holds, where nevertheless the other three equations of motion do not hold, and in fact where there may actually occur a split or division in the fluid. As an instance may be mentioned the flowing of a stream of water over the edge of a perpendicular precipice, where gravity suddenly comes into action. In this case the *number* of particles may remain the same in an element which may be conceived to consist of particles that have fallen over as well as of those that remain. But the equation of motion would require the new force of gravity to be taken into account.

XXIII. *On the Integration of some Equations in Partial Differentials.* By the Rev. BRICE BRONWIN\*.

IN the present paper I shall extend the mode of integrating certain differential equations, which I gave in this Journal in December last, to the integration of similar equations in partial differentials. Let  $D$  stand for  $\frac{d}{dx}$ ,  $D'$  for  $\frac{d}{dy}$ ; and as a first example, let

$$x\left(\frac{d^2z}{dx^2} - a^2\frac{d^2z}{dy^2}\right) + 2p\frac{dz}{dx} = 0,$$

where  $p$ , as throughout the paper, is a positive integer. Make

$$aD'\sqrt{-1} = k,$$

then 
$$x\left(\frac{d^2z}{dx^2} + k^2z\right) + 2p\frac{dz}{dx} = 0.$$

This is the first example in the paper above referred to, where by making  $z = (D^2 + k^2)^p u$ , it is reduced to

$$\frac{d^2u}{dx^2} + k^2u = 0;$$

or, restoring the suppressed factor, to

$$(D^2 + k^2)^p x(D^2 + k^2)u = 0.$$

\* Communicated by the Author.

Therefore

$$x(D^2 + k^2)u = (D^2 + k^2)^{-p} 0 = (D + k\sqrt{-1})^{-p} (D - k\sqrt{-1})^{-p} \\ 0 = \varepsilon^{-xk\sqrt{-1}} D^{-p} \varepsilon^{2xk\sqrt{-1}} D^{-p} \varepsilon^{-xk\sqrt{-1}} 0 = \varepsilon^{-xk\sqrt{-1}} D^{-p} \varepsilon^{2xk\sqrt{-1}} \\ (A + A_1 x \dots + A_{p-1} x^{p-1}).$$

We must now put  $aD'\sqrt{-1}$  for  $k$ , and change  $A$ ,  $A_1$ , &c. into functions of  $y$ . Then

$$\varepsilon^{2xk\sqrt{-1}} A = \varepsilon^{-2axD'} f(y) = f(y - 2ax), \text{ \&c.,}$$

and

$$x(D^2 + k^2)u = \varepsilon^{axD'} D^{-p} \{ f(y - 2ax) + x f_1(y - 2ax) \dots \\ + x^{p-1} f_{p-1}(y - 2ax) \} = \varepsilon^{axD'} \{ \phi(y - 2ax) + x \phi_1(y - 2ax) \dots \\ + x^{p-1} \phi_{p-1}(y - 2ax) + \psi(y) + x \psi_1(y) \dots + x^{p-1} \psi_{p-1}(y) \},$$

where  $\psi(y)$ ,  $\psi_1(y)$ , &c. are the arbitraries of the integrations. And here I must observe, that the functions denoted by  $\phi$ ,  $\phi_1$ ,  $\psi$ ,  $\psi_1$ , &c. are supposed to be changed when necessary, although the same symbols are still retained to represent them.

Operating upon the last result with

$$(D^2 + k^2)^{p-1} x^{-1} = (D + aD')^{p-1} (D - aD')^{p-1} x^{-1} = \varepsilon^{-axD} \\ D^{p-1} \varepsilon^{2axD'} D^{p-1} \varepsilon^{-axD'} x^{-1},$$

we have

$$z = \varepsilon^{-axD'} D^{p-1} \varepsilon^{2axD'} \{ x^{-p} \phi(y - 2ax) + x^{-p+1} \phi_1(y - 2ax) \dots \\ + x^{p-2} \phi_{p-2}(y - 2ax) + x^{-p} \psi(y) \} = \varepsilon^{-axD'} D^{p-1} \{ x^{-p} \phi(y) \\ + x^{-p+1} \phi_1(y) \dots + x^{p-2} \phi_{p-2}(y) + x^{-p} \psi(y + 2ax) \} \\ = x^{-2p+1} \{ \phi(y - ax) + \psi(y + ax) \} + x^{-2p+2} \{ \phi_1(y - ax) \\ + \psi_1(y + ax) \} \dots + x^{-p} \{ \phi_{p-1}(y - ax) + \psi_{p-1}(y + ax) \}.$$

Substituting this value in the given equation, we find  $\phi_1$ ,  $\phi_2$ , &c., and  $\psi_1$ ,  $\psi_2$ , &c. to be the successive differential coefficients of  $\phi$  and  $\psi$ ; and ultimately

$$z = x^{-2p+1} \{ \phi(y - ax) + \psi(y + ax) \} + ax^{-2p+2} \{ \phi'(y - ax) - \psi'(y + ax) \} \\ + a^2 \frac{p-2}{2p-3} x^{-2p+3} \{ \phi''(y - ax) + \psi''(y + ax) \} + \&c.,$$

to the term containing  $x^{-p}$  inclusive.

As a second example, let

$$x \left( \frac{d^2 z}{dx^2} + 2 \frac{d^2 z}{dx dy} + 2 \frac{d^2 z}{dy^2} \right) + 2p \left( \frac{dz}{dx} + \frac{dz}{dy} \right) = 0.$$

Or if  $k$  be put for  $D'$ ,

$$x \left( \frac{d^2 z}{dx^2} + 2k \frac{dz}{dx} + 2k^2 z \right) + 2p \left( \frac{dz}{dx} + k z \right) = 0.$$

Making  $z = (D^2 + 2kD + 2k^2)^p u$ ,

this, by the paper before referred to, reduces to

$$(D^2 + 2kD + 2k^2)u = x^{-1}(D^2 + 2kD + 2k^2)^{-p}0,$$

after restoring the suppressed factor and dividing by it.

If  $1 + \sqrt{-1} = \alpha, 1 - \sqrt{-1} = \epsilon$ ,  
we have  $D^2 + 2kD + 2k^2 = (D + \alpha k)(D + \epsilon k)$ .

But

$$(D + \alpha k)^{-p}(D + \epsilon k)^{-p}0 = \epsilon^{-\alpha x k} D^{-p} \epsilon^{2x k \sqrt{-1}} D^{-p} \epsilon^{\epsilon x k} \\ 0 = \epsilon^{-\alpha x k} D^{-p} \epsilon^{2x k \sqrt{-1}} \{A + A_1 x \dots + A_{p-1} x^{p-1}\}.$$

Now, putting  $D'$  for  $k$ , and  $f(y)$  for  $A$ , &c., we have

$$x^{-1}(D + \alpha k)^{-p}(D + \epsilon k)^{-p}0 = x^{-1} \epsilon^{-\alpha k x} D^{-p} \{f(y + 2x \sqrt{-1}) \\ + x f_1(y + 2x \sqrt{-1}) \dots + x^{p-1} f_{p-1}(y + 2x \sqrt{-1})\} \\ = x^{-1} \epsilon^{-\alpha k x} \{\phi(y + 2x \sqrt{-1}) + x \phi_1(y + 2x \sqrt{-1}) \dots \\ + x^{p-1} \phi_{p-1}(y + 2x \sqrt{-1}) + \psi(y) + x \psi_1(y) \dots \\ + x^{p-1} \psi_{p-1}(y)\} = \epsilon^{-\alpha x k} \{x^{-1} \phi(y + 2x \sqrt{-1}) + \phi_1 \\ (y + 2x \sqrt{-1}) \dots + x^{p-2} \phi_{p-1}(y + 2x \sqrt{-1}) + x^{-1} \psi(y) \\ + \psi_1(y) \dots + x^{p-2} \psi_{p-1}(y)\},$$

where  $\psi, \psi_1$ , &c. are introduced as the arbitraries of the integration.

From the last result we easily see that

$$z = \epsilon^{-\alpha x k} D^{p-1} \epsilon^{2x k \sqrt{-1}} D^{p-1} \epsilon^{-2x k \sqrt{-1}} \{x^{-1} \phi(y + 2x \sqrt{-1}) + \&c. \\ + x^{-1} \psi(y) + \&c.\} = \epsilon^{-\alpha x k} D^{p-1} \epsilon^{2x k \sqrt{-1}} D^{p-1} \{x^{-1} \phi(y) + \&c. \\ + x^{-1} \psi(y - 2x \sqrt{-1}) + \&c.\} = \epsilon^{-\alpha x k} D^{p-1} \epsilon^{2x k \sqrt{-1}} \{x^{-p} \phi(y) \\ + x^{-p} \psi(y - 2x \sqrt{-1}) + x^{-p+1} \psi_1(y - 2x \sqrt{-1}) \dots \\ + x^{p-2} \psi_{2p-2}(y - 2x \sqrt{-1})\} = \epsilon^{-\alpha x k} D^{p-1} \{x^{-p} \phi \\ (y + 2x \sqrt{-1}) + x^{-p} \psi(y) + x^{-p+1} \psi_1(y) \dots + x^{p-2} \psi_{2p-2}(y)\} \\ = \epsilon^{-\alpha x k} \{x^{-2p+1} \phi(y + 2x \sqrt{-1}) + x^{-2p+2} \phi_1(y + 2x \sqrt{-1}) \dots \\ + x^{-p} \phi_{p-1}(y + 2x \sqrt{-1}) + x^{-2p+1} \psi(y) + x^{-2p+2} \psi_1(y) \dots \\ + x^{-p} \psi_{p-1}(y)\} = x^{-2p+1} \{\phi(y - \epsilon x) + \psi(y - \alpha x)\} + x^{-2p+2} \\ \{\phi_1(y - \epsilon x) + \psi_1(y - \alpha x)\} + \dots \&c.$$

In these reductions the values of  $\alpha$  and  $\epsilon$  have sometimes been put for these quantities, and  $k$  has been retained as more convenient than  $D'$ .

As in the last example, by substitution in the proposed, we find

$$z = x^{-2p+1} \{ \phi(y-\epsilon x) + \psi(y-\alpha x) \} + x^{-2p+2} \{ a\phi'(y-\epsilon x) + b\psi'(y-\alpha x) \} + x^{-2p+3} \{ a_1\phi''(y-\epsilon x) + b_1\psi''(y-\alpha x) \} + \dots$$

to the term containing  $x^{-p}$  inclusive. The constant coefficients  $a, a_1, \&c., b, b_1, \&c.$  are easily determined, and could not be conveniently put down.

Let 
$$x^2 \frac{d^2 z}{dx^2} + h \frac{d^2 z}{dx dy} - p(p+1)z = 0.$$

Make  $k = hD'$ , and this becomes

$$x^2 \frac{d^2 z}{dx^2} + k \frac{dz}{dx} - p(p+1)z = 0.$$

If  $z = D^{p-1}u$ , by a paper of mine in the Mathematician for November last, this reduces to

$$x^2 \frac{du}{dx} + (k - 2px)u = 0,$$

or rather

$$x^2 \frac{du}{dx} + (k - 2px)u = D^{-p}0 = A + A_1x + \dots + A_{p-1}x^{p-1}.$$

Integrating

$$\epsilon^{-\frac{k}{x}} x^{-2p} u = \int dx x^{-2p-2} \epsilon^{-\frac{k}{x}} (A + A_1x + \dots + A_{p-1}x^{p-1}).$$

Restoring the value of  $k$ , and changing  $A, A_1, \&c.$  into  $f(y), f_1(y), \&c.$ , we have

$$\begin{aligned} u &= x^{2p} \epsilon^{\frac{h}{x} D'} \int dx x^{-2p-2} \left\{ f\left(y - \frac{h}{x}\right) + x f_1\left(y - \frac{h}{x}\right) + \dots \right. \\ &\quad \left. + x^{p-1} f_{p-1}\left(y - \frac{h}{x}\right) \right\} = x^{2p} \epsilon^{\frac{h}{x} D'} \left\{ x^{-2p} \phi\left(y - \frac{h}{x}\right) \right. \\ &\quad \left. + x^{-2p+1} \phi_1\left(y - \frac{h}{x}\right) + \dots + \phi_{2p}\left(y - \frac{h}{x}\right) + \psi(y) \right\} \\ &= \phi(y) + x\phi_1(y) + \dots + x^{2p}\phi_{2p}(y) + x^{2p}\psi\left(y + \frac{h}{x}\right). \end{aligned}$$

We must sometimes change the order of the terms, as in this example; for if in substituting in the given equation we begin at the wrong end of the series, we shall have  $\phi, \int \phi dy, \iint \phi dy^2, \&c.$  instead of  $\phi, \phi', \phi'', \&c.$

Thus

$$\begin{aligned} z &= D^{p-1}u = x^{p+1}\phi(y) + x^p\phi_1(y) + \dots + \phi_{p+1}(y) + x^{p+1}\psi\left(y + \frac{h}{x}\right) \\ &\quad + x^p\psi_1\left(y + \frac{h}{x}\right) + \dots + x^2\psi_{p-1}\left(y + \frac{h}{x}\right). \end{aligned}$$



Substituting this value in the given equation, we find

$$z = x^{p+1} \left\{ \phi(y) + \psi\left(y + \frac{h}{x}\right) \right\} + x^p \left\{ a\phi'(y) + b\psi'\left(y + \frac{h}{x}\right) \right\} +$$

&c., the series in  $\phi$ ,  $\phi'$ , &c. stopping at  $x^0$ , that in  $\psi$ ,  $\psi'$ , &c. at  $x^2$ ; the values of the constants  $a$ ,  $a_1$ , &c.,  $b$ ,  $b_1$ , &c. are not put down, for the same reason as in the last example.

If in this last example we make  $k = PD' + Q$ ,  $P$  and  $Q$  being functions of  $y$ , the equation to be integrated will be

$$x^2 \frac{d^2 z}{dx^2} + P \frac{d^2 z}{dx dy} + Q \frac{dz}{dx} - p(p+1)z = 0,$$

which by  $z = D^{p-1}u$  reduces to

$$x^2 \frac{du}{dx} + P \frac{du}{dy} + (Q - 2px)u = f(y) + x f_1(y) + \dots + x^{p-1} f_{p-1}(y),$$

which is only of the first order. We must not proceed in this case as heretofore, but must expand

$$\epsilon^{\pm \frac{k}{x}} = \epsilon^{\pm} \left( \frac{PD' + Q}{x} \right)$$

by the powers of  $\frac{PD' + Q}{x}$ , if we wish to integrate as in the preceding examples. Thus

$$\epsilon \frac{PD' + Q}{x} f(y) = \left\{ 1 + \frac{PD' + Q}{x} + \frac{1}{2} \left( \frac{PD' + Q}{x} \right)^2 + \&c. \right\}$$

$$f(y) = f(y) + x^{-1} (PD' + Q) f(y) + \frac{x^{-2}}{2} (PD' + Q) (PD' + Q) f(y) + \&c.,$$

the powers, after the expansion, being changed into successive operations. In this case we might not obtain a solution in finite terms.

Sometimes it may be desirable to proceed with  $D'$  in the same manner as with  $D$ . Let

$$x \left( \frac{d^2 z}{dx^2} + \frac{d^2 z}{dx dy} \right) + \frac{dz}{dx} = 0;$$

$$\text{or} \quad xD^2z + xDD'z + Dz = 0.$$

But, as in the papers referred to,

$$xD^2z = D^2xz - 2Dz, \quad xDD'z = DD'xz - D'z.$$

$$\text{Therefore} \quad D^2xz + DD'xz - Dz - D'z = 0;$$

$$\text{or} \quad (D + D') (Dxz - z) = 0;$$

or

$$Dxz - z = (D + D')^{-1} 0 = \epsilon^{-xD'} D^{-1} \epsilon^{xD'} 0 = \epsilon^{-xD'} f(y) = f(y - x);$$

that is 
$$x \frac{dz}{dx} = f(y-x),$$

and therefore 
$$z = \int \frac{dx}{x} f(y-x) + \phi(y).$$

Sometimes we may succeed by partially adopting these processes. Let

$$x \frac{d^2 z}{dx^2} + (xy+1) \frac{d^2 z}{dx dy} + y \frac{d^2 z}{dy^2} + \frac{dz}{dy} = 0;$$

or 
$$xD^2 z + (xy+1)DD'z + yD'^2 z + D'z = 0.$$

Now 
$$yD'z = D'y z - z,$$

therefore

$$xy DD'z = xDD'y z - xDz, \quad yD'^2 z = D'^2 y z - 2D'z.$$

Therefore by substitution

$$xD^2 z + xDD'y z + DD'z + D'^2 y z - xDz - D'z = 0.$$

Or 
$$(xD + D')(Dz + D'y z - z) = 0,$$

which may be put under the forms

$$(xD + D')(D + yD')z = 0;$$

or 
$$(D + x^{-1}D')(D + yD')z = 0.$$

Make 
$$(D + yD')z = u;$$

then 
$$(D + x^{-1}D')u = 0, \text{ and } u = x^{-D'} f(y).$$

Therefore

$$(D + yD')z = x^{-D'} f(y) = e^{-lx D'} f(y) = f(y - lx);$$

which gives 
$$z = e^{-xy D'} \int dx e^{xy D'} f(y - lx).$$

It must be remembered that

$$e^{\pm xy D'} = 1 \pm x(y D') + \frac{x^2}{2} (y D')^2 \pm \&c.$$

$$= 1 \pm xy D' + \frac{x^2}{2} y D' y D' \pm \&c.,$$

the powers being converted into successive operations.

Sometimes an equation may be reduced an order lower, or to a more simple form of the same order, by these transformations, as in the following example.

Let 
$$\frac{d^3 z}{dx^3} + hx^2 \frac{d^3 z}{dx^2 dy} - 2h \frac{dz}{dy} = 0.$$

Make  $z = (D + hx^2 D')u$ , and the above will reduce to

$$\begin{aligned} \frac{d^3 u}{dx^3} + hx^2 \frac{d^3 u}{dx^2 dy} + 4hx \frac{d^2 u}{dx dy} &= (D + hx^2 D')^{-1} 0 \\ &= e^{-\frac{hx^3}{3} D'} D^{-1} e^{\frac{hx^3}{3} D'} 0 = e^{-\frac{hx^3}{3} D'} f(y) = f\left(-\frac{hx^3}{3}\right). \end{aligned}$$

If  $\frac{du}{dx} = v$ , the last equation becomes

$$\frac{d^2 v}{dx^2} + hx^2 \frac{d^2 v}{dx dy} + 4hx \frac{dv}{dy} = f\left(y - \frac{hx^3}{3}\right).$$

Then

$$z = (D + hx^2 D') u = \varepsilon^{-\frac{hx^3}{3} D'} D \varepsilon^{\frac{hx^3}{3} D'} \left( \int v dx + \phi(y) \right).$$

Examples might be given of reduction to a more simple form of the same order.

The omission of the factors

$$x^{-1} (D^2 + k^2)^{-p}, x^{-1} (D^2 + 2kD + 2k^2)^{-p}, \&c.$$

before the cipher of the second member of the equations treated of in my paper in the Number of this Journal for December last was an oversight, which I did not discover till I received the January Number, notwithstanding the present paper was composed immediately after it.

Gunthwaite Hall, Near Barnsley, Yorkshire,  
Jan. 12, 1847.

#### XXIV. Mr. JOHN SOUTHERN'S *Experiments on the Density, Latent Heat and Elasticity of Steam*.\*.

*To Richard Taylor, Esq.*

DEAR SIR,

IN the various statements which have been published and re-published from time to time respecting the nature and properties of steam, the experimental investigations of Mr. John Southern have not become so generally known as they deserve to be, and if you can find space in your Magazine for reprinting his papers they would convey useful information to your readers.

Mr. Southern was an able mathematician†, who was retained by Mr. Watt in the manufactory of steam-engines at Soho, for the purpose of assisting him in investigating the best proportions of the various parts of those machines, and calculating the requisite dimensions which (according to the ascertained strength of the materials employed) would give

\* Several important recent investigations of this subject will be found in the 4th Volume of Taylor's Scientific Memoirs, just completed, among which may be mentioned Holtzmann on the Heat and Elasticity of Gases and Vapours, and on the principles of the Theory of Steam-Engines: Magnus on the Expansive Force of Steam; and on the Force requisite for the production of Vapours; and Regnault on the Elastic Forces of Aqueous Vapour.—Ed.

† Mr. Southern contributed two papers to the early volumes of the Philosophical Magazine. Vol. xi. p. 97, and xvii. 120.

security against breaking, without any unnecessary weight. An experimental inquiry into the properties of steam was necessarily a part of such a course, and Mr. Southern stated the results he had obtained in a letter to Mr. Watt, which was printed by Dr. Brewster in his new edition of the articles written by Dr. Robison for the *Encyclopædia Britannica*. The article Steam-engine in that edition was revised by Mr. Watt, in 1814, and he added some notes and an appendix, which contains Mr. Southern's letter.

I am, dear Sir,

67 Guildford Street, Russell Square,  
London, Jan. 16, 1847.

Yours very sincerely,  
JOHN FAREY.

*Letter from Mr. Southern to Mr. Watt\*.*

The experiments, of which the particular circumstances are hereafter related, were made in 1803, with the view of ascertaining, chiefly, the density of steam raised from water under different pressures above that of the atmosphere, an apparatus having then been made for a different purpose, which seemed pretty well adapted to this object, as it did equally so to that of ascertaining the latent heat of steam.

It may be premised, that the thermometers employed in all the experiments which will be now related, were made and graduated with the greatest care, the tubes having been accurately measured as to the proportional capacity of their different parts, the boiling-point of each ascertained, according to the rules prescribed by a committee of the Royal Society, in 1777, (viz. the bulbs and tubes being in steam when the barometer stood at 29·8 inches, this degree of temperature being called 212°,) and in all cases the bulb and the tube, as high as the mercury ascended in it, were kept in the steam or the water whose temperature was to be noted. This latter circumstance was effected in the case of steam, by sliding the tube of the thermometer through a stuffing-box, or collar, made tight, till the mercury in it could just be seen above it. The tube had known marks on it, from which measurements were taken to the mercury, and thence the temperature known.

The quantity of steam was measured by filling a cylinder with it (inclosed in the steam), whose diameter was about 3·16 inches, and driving it out by the motion of a piston, which had 18 inches stroke regulated by the rotation of a crank. The solid contents of the piston-rod, which was 0·86 inch diameter, diminished the contents of the cylinder, leaving the quantity discharged each stroke by the motion of the piston,

\* In all the experiments, of which an account is given in this letter, Mr. Southern was assisted by Mr. William Creighton.



very nearly 130·7 cubic inches; but as the piston did not rise high enough to touch the top that closed the cylinder, and there was also unavoidably a space between the valve and the cylinder, these spaces together were computed to equal 1·7 cubic inch. Of course, had the elasticity of the steam been just equal to that of the atmosphere, no addition to the 130·7 cubic inches would need to be made; but as in the three successive experiments it was about  $\frac{1}{3}$ ,  $\frac{5}{3}$ , and  $\frac{2}{3}$  greater, these proportions of the spaces would escape when the valve was open that allowed the discharge of the steam to be made into the atmosphere, and must therefore be added respectively to the contents discharged by the motion of the piston.

These additional quantities are  $1\cdot7 \times \frac{1}{3} = \cdot57$ ;  $1\cdot7 \times \frac{5}{3} = 2\cdot83$ ; and  $1\cdot7 \times \frac{2}{3} = 5\cdot1$ ; which, added to 130·7, gives 131·27 in the first experiment; 133·53 in the second, and 135·8 in the third, for the quantity of steam discharged at each stroke of the piston; and therefore the number of strokes which would discharge one cubic foot in each of the three experiments, would be 13·164, 12·941, and 12·724, respectively.

The steam was conducted from the cylinder, after passing the valve, by means of an iron pipe attached to a small copper one, having its end bent down, and immersed a short depth into a cistern of water. The cistern was made of fir-wood, and painted inside and outside with white paint; was about 30 inches square, and 26 inches deep; and the quantity of water in it was ascertained by weighing it, as was also the accession to it by the condensed steam.

The elasticity of the steam was ascertained by measuring an actual column of mercury which it supported; and the number of strokes was ascertained by a machine called a counter.

The following table contains the principal facts of these experiments:

Number of the experiment.	Duration of the experiment in minutes.	Whole number of strokes.	Number of strokes per minute.	Weight of water in cistern at beginning.	Temperature of ditto.	Weight of water gained by condensed steam.	Temperature of water in cistern at the end.	Temperature of steam.	Elasticity of steam in boiler.	Temperature of ditto.
I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.	XI.
1	121 $\frac{1}{4}$	5154	42·3	lb. 721 $\frac{1}{4}$	45 $\frac{1}{2}$	lb. 20·25	76	30 $\frac{1}{2}$	Inches merc. 40	229
2	51 $\frac{1}{4}$	2434	41 $\frac{1}{4}$	722	48	20·00	80 $\frac{1}{4}$	32 $\frac{1}{4}$	80	270
3	38 $\frac{1}{4}$	1599	41·8	722	48	19·45	79 $\frac{1}{4}$	31 $\frac{1}{4}$	120	295

If the whole number of strokes in each experiment be divided by the number, found as above, that were required to discharge one cubic foot of steam, the whole number of cubic feet of steam discharged in each experiment will be given; viz.  $5154 \div 13.164 = 391.53$ ;  $2434 \div 12.941 = 188.09$ ; and  $1599 \div 12.724 = 125.66$ ; the quantity of steam formed and discharged in the first, second and third experiments respectively, in cubic feet.

If the weight of water gained by the condensation of steam in each experiment be multiplied by 27.65, the number of cubic inches of water in a pound weight, and divided by the number of cubic feet of steam which were condensed, the quotient will give the portion of water, in cubic inches, required by each cubic foot of steam for its formation; and hence also the comparative density.

$$\left. \begin{array}{l} \text{Thus } 20.25 \times 27.65 \div 391.53 = 1.430 \\ 20.00 \times 27.65 \div 188.09 = 2.940 \\ 19.45 \times 27.65 \div 125.66 = 4.279 \end{array} \right\} \begin{array}{l} \text{inches of water to} \\ \text{form each cubic} \\ \text{foot of steam;} \end{array}$$

$$\text{and these numbers are proportional to } \left\{ \begin{array}{l} 40.00 \\ 82.24 \\ 119.70 \end{array} \right\} \text{the relative}$$

$$\text{densities, while the elasticities were as } \left\{ \begin{array}{l} 40 \\ 80 \\ 120 \end{array} \right\} \text{respectively.}$$

These results appear to support the conclusion that *the density of steam is nearly, if not accurately, proportional to its elasticity*; at least this may be affirmed of it within the limits of these experiments.

From the above experiments may be calculated the latent heat of steam developed in the three cases; for if the weight of the water which received the augment of heat, be multiplied by the number of degrees of temperature communicated to it, and if this product be divided by the accession of weight to the water (which only could have communicated the accession of temperature), it is evident that the quotient will give the temperature which the steam lost; and if to this be added the temperature which it retained (viz. that of the water in the cistern at the conclusion of the experiment), a number will be obtained showing the *whole heat*, or the *sum* of the latent and sensible heat of the steam. Hence, by subtracting the sensible heat of the steam from this sum, the latent heat will

be found. That is,  $\frac{\text{col. 5} \times \text{col. 9}}{\text{col. 7}} + \text{col. 8} = \text{the sum of the latent and sensible heat; or,}$

if  $W$  = weight of cold water,  $T$  = its temperature  
 $w$  = accession of water by the condensed steam,  
 $t$  = the temperature of warm water, and  
 $x$  = the sum of the latent and sensible heat of the  
steam condensed,

$$\frac{W + w \cdot t - W T}{w} + x.$$

Either of these equations will be found to give in the three experiments  $1157^{\circ}$ ,  $1244^{\circ}$ , and  $1256^{\circ}$ , from which subtract the numbers, col. xi.  $229^{\circ}$ ,  $270^{\circ}$ , and  $295^{\circ}$ , and there remain . . . .  $928^{\circ}$ ,  $974^{\circ}$ , and  $961^{\circ}$ , the latent heat.

Three other experiments were instituted with the intention of ascertaining the latent heat of steam under the three same degrees of elasticity, viz. equal to the support of 40, 80 and 120 inches of mercury. The steam was raised or generated in the same boiler used in the previous experiments, and from the end of a cast-iron pipe of  $1\frac{1}{2}$  inch diameter which united with it, a small copper pipe was taken (its diameter about  $\frac{5}{16}$  inch) and bent down so that its end could conveniently be immersed an inch or two under the surface of the water. The end of this pipe was closed by a thin disc of copper, in which a circular hole was made  $\frac{58}{1000}$ th of an inch diameter, through which the steam from the boiler was blown into the cold water. The water which received the heat was contained in a tinned iron vessel that weighed 3.77 lbs., and its capacity for heat may therefore be called equivalent to  $\frac{1}{2}$  lb. of water. In each of the experiments, the water employed to receive the steam weighed 28 lbs., to which, in the following table, recording the principal facts, is added this  $\frac{1}{2}$  lb., in lieu of the vessel.

Number of experiment.	Duration of the experiment.	Weight of cold water.	Temperature of cold water.	Temperature at the end of experiment.	Temperature gained.	Weight of water gained.	Temperature of steam.	Elasticity of steam.
1	12,45	lbs. 28 $\frac{1}{2}$	48	80	32	lbs. ,878	229	Inches. 40
2	5,50	28 $\frac{1}{2}$	48	81 $\frac{1}{4}$	33 $\frac{1}{4}$	,857	270	80
3	4,00	28 $\frac{1}{2}$	47 $\frac{3}{4}$	81	33 $\frac{1}{4}$	,826	295	120

If either of the equations above be applied here to the facts noted in this table, the sum of the latent and sensible heat will come out  $1119^{\circ}$ ,  $1190^{\circ}$  and  $1228^{\circ}$ ; and the latent heat  $890^{\circ}$ ,  $920^{\circ}$  and  $933^{\circ}$ . It was observed, however, that the tin vessel lost heat to the surrounding air very sensibly, and an experiment was made to determine the amount of this effect; and it

was found when the contained water was at  $80^{\circ}$ ,  $1^{\circ}$  was lost in five minutes; when at  $60^{\circ}$ ,  $1^{\circ}$  was lost in ten and a half minutes; it would therefore probably lose  $1^{\circ}$  in eight minutes during the time of an experiment, the mean temperature being about  $65^{\circ}$ ; and as the excess of temperature at the beginning and end of an experiment above that of the air was nearly the same in all three, the loss would be nearly proportional to the duration of each. Hence, to the acquired heat should be added, in the first experiment,  $1\frac{5}{8}^{\circ}$ ; in the second,  $\frac{3}{4}^{\circ}$ ; and in the third,  $\frac{1}{2}^{\circ}$ ; being severally proportional to the said duration. These being respectively added to the temperatures in column V. and VI., give in the former  $81\frac{5}{8}^{\circ}$ ,  $82^{\circ}$  and  $81\frac{1}{2}^{\circ}$ ; and, in the latter,  $33\frac{5}{8}^{\circ}$ ,  $34^{\circ}$  and  $33\frac{5}{4}^{\circ}$ ; and if either of these sets of numbers be used in the calculation, according as one or the other of the equations is adopted to develop the results, they will be found to be  $1171^{\circ}$ ,  $1212^{\circ}$ , and  $1245^{\circ}$  for the sums of the latent and sensible heat; and consequently the latent heat in each experiment will be  $942^{\circ}$ ,  $942^{\circ}$  and  $950^{\circ}$ .

It may be remarked, that no allowance was made in calculating from the former experiments, for the heat which would be taken by the cistern, but which in the first of them, lasting two hours, would probably be very sensible, and may account for the principal part of the deficiency of latent heat brought out by the calculation from that experiment, in comparison with that from the two succeeding ones.

The opinion which I entertain from these experiments as to the *latent heat* of steam is, that it is a *constant* quantity, and perhaps this opinion obtains support from the modern discoveries of definite proportions. But it is necessary, however, to explain the limitation with which I here use this term, "*constant quantity*." It is well known that if common air be expanded, cold is produced\*; and it must therefore happen, that if a given quantity of it at a given temperature could be gradually expanded, and as it was so expanded, gradually supplied with heat, so as to keep the temperature unaltered, this supply of heat would become *latent*; the thermometer would not show it. It is probable, both from analogy and experiment, that this effect takes place in the expansion of steam. It is *not of this part* of its heat, though latent, and in the experiments above related undistinguishable, that I would

\* An opportunity occurred to me some years back, which enabled me to determine, with tolerable precision, the degree of cold produced by the expansion of common air from the bulk of two to three, which I found to be  $19^{\circ}$  or  $20^{\circ}$ .



be understood to speak when I state my opinion to be as just mentioned; but it is of that which, when water alters its state to that of an elastic fluid, becomes essential to it in every degree of elasticity, besides that which belongs to its expanded state. This latter may be called the latent heat of *expansion*\*, while the other may perhaps properly be called *constitutional*.

Allow me here to illustrate hypothetically this matter: If this essential or constitutional part of *latent* heat be added to water having the necessary portion of *sensible* heat, and perfectly confined in a close vessel, I conceive the water would be in the state of an elastic fluid; would in fact be steam, as dense as water (possibly compressible, and capable of greater density), and would then require no latent heat of expansion; but if the containing vessel be now conceived to expand, for instance into double the space, I then imagine it would require some addition of heat during this expansion to maintain its proportional elasticity. It must be observed, however, that while this expansion was calling for more latent heat, the sensible heat necessary for the diminishing elasticity would be lessening; but it does not follow that these quantities should necessarily balance each other†.

When this fluid, steam, is raised in low temperatures, and of course under a low degree of elasticity, it obtains from its source, at the same instant, not only the constitutional part of its latent heat, but also that of expansion, and thus the two kinds are confounded; and, in experiments where they are developed by total condensation, are only to be detected together in sum; and it *may be* that this sum, together with the sensible heat, in different states of elasticity, may make a constant quantity; but if the latent heat of expansion from a denser to a rarer state be greater than the diminution of the sensible heat necessary only for the latter, the sum of the sensible and total latent heat will be more in steam raised in low temperatures than in high ones, which the result of your experiments made in low temperatures seems to countenance.

In all that I have said above, when speaking of steam, I have always intended that fluid in the state in which it is raised from water, viz. saturated therewith; but undoubtedly this fluid, after it is so raised under any temperature, and being clear from any additional accession of water, may be heated above that temperature, and cooled down to it again with

\* I have no view here to any substances not having the natural power of expansion, as water, ice, &c.—J. S.

† I have, for very many years, entertained a similar hypothesis; but I know of no experiment whereby the truth of it can be demonstrated conclusively."—W. *This note by Mr. Watt.*

changes of elasticity corresponding to those of temperature, like as common air may be, without limitation of temperature, as far as is known. This, however, is a view of the subject which has been totally excluded.

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Besides the experiments first related, in which the temperature of steam raised under *high* pressures was observed, others had been made some years before (in 1797 and 1798) for that purpose only; and as they were made with the greatest circumspection, both the manner of making them and their results may be here described, as may also the results of other experiments, made with equal care, to ascertain the temperature of steam raised under *low* pressures.

The instrument used in the former was a Papin's digester, similar to what you had used in your original experiments, and to that described in the *Encyclopædia Britannica*, art. Steam, No. 22, the leading differences being in adapting a metallic tube to it to contain the thermometer, or rather as much of it as contained mercury, in the manner mentioned in the beginning of this letter, and instead of a valve, by the load on which to measure the elasticity of the contained steam, a nicely-bored cylinder was applied, with a piston fitting it, so as to have very little friction, and to the rod of this was applied a lever, constructed to work on edges like those of a scale-beam, by which the resistance against the elastic force of the steam could be accurately determined; and at your suggestion, to be assured that no inaccuracy had crept into the calculation, by which this resistance through the medium of the lever was ascertained, an actual column of mercury of 30 inches high was substituted, and the correspondence was found to be within  $\frac{1}{100}$  of an inch.

The observations at each of the points of pressure noted were continued some minutes, the temperature at each being alternately raised and lowered, so as to make the pressure of the steam on the under side of the piston alternately too much and too little for the weight with which it was loaded; and thence a mean temperature was adopted, the extremes of which were, as well as I now recollect, not more than half a degree on each side of it. The load on the piston, including its own weight, &c., was calculated to be successively just equal to 1, 2, 4, and 8 atmospheres of 29·8 inches of mercury each, and the temperature of the steam was varied as above till that of each point was determined: the results were thus—

Atmospheres.	Pressure in inches of mercury.	Temperatures.
1	29.8	212.0
2	59.6	250.3
4	119.2	293.4
8	238.4	343.6

The experiments for ascertaining the temperature of steam below the atmospheric pressure were made with an apparatus essentially similar to that which you originally used, and with scrupulous care and attention; and I met with the same incidents as you had done; such as the production of a bubble of air whenever, after any experiment, the tube was inclined to refill the ball; and also the extraordinary suspension of a column of mercury of 35 inches vertical height, and of 7 inches of water above that, although the counterpoise was only that of the atmosphere, then under 30 inches. I found also that the tube required a considerable degree of tabouring or shaking to make the column subside and leave a space in the ball. This phænomenon was not produced till after much pains taken in inverting and re-inverting the tube again and again, nor till it had been suffered, after these operations, to stand for three or four days undisturbed in the exhausting position, and then discharging the air that had been accumulating in the interval.

The results, to be found in the table below, were deduced from the observations as you had done, viz. by adding to the height of the column of mercury in the tube (ascertained by a gauge floating on the surface of the mercury in the basin), that of the water above it, or rather of an equivalent column of mercury, and subtracting their sum from the height of the common barometer at the time. All these results were taken from observations made after the apparatus had been so perfectly exhausted of air as to produce the phænomenon just mentioned.

Tempera- ture.	Elasticity.				Tempera- ture.	Elasticity.			
	1st set.	2nd set.	3rd set.	Mean.		1st set.	2nd set.	3rd set.	Mean.
52	In.	In.	In.	In.	122	In.	In.	In.	In.
62	0.53	0.52	0.52	0.52	132	3.58	3.54	3.58	3.57
72	0.73	0.73	0.73	0.73	142	4.68	4.65	4.72	4.68
82	1.03	1.02	1.02	1.02	152	6.05	6.00	6.14	6.06
92	1.42	1.41	1.42	1.42	162	7.86	7.80	7.89	7.85
102	1.98	1.92	1.95	1.95	172	9.98	9.96	10.04	9.99
112	2.67	2.63	2.66	2.65	182	12.54	12.72	12.67	12.64
						16.01	15.84	15.88	15.91

The following formula will be found to give the elasticity belonging to a given temperature, and *vice versa*, with a sufficient degree of accuracy for most purposes, within the range of the experiments, at least, from which they have been formed.

Let  $t$  = temperature,  $e$  = elasticity, in inches of mercury;

$$T = t + 52, \text{ and } E = e - \frac{1}{10}, m = 94250,000000.$$

Then

$$\frac{T^{5.14}}{m} = E$$

$$\sqrt[5.14]{Em} = T$$

But as the calculation is most easily performed by logarithms, let  $L$  signify the logarithm of the quantity to which it is prefixed: then

$$5.14 LT - 10.97427 = LE$$

$$\frac{LE + 10.97427}{5.14} = LT.$$

The following table shows the observed elasticities, those derived from calculation by the formula, and the differences of the two, which appear to me to be as small as can be expected, taking a general view.

Tempera- ture.	Observed elasticities.	Calculated elasticities.	Differ- ences.	Tempera- ture.	Observed elasticities.	Calculated elasticities.	Differ- ences.
	In.	In.	In.		In.	In.	In.
32°	.....	0.18		142	6.06	6.20	+0.14
42	.....	0.25		152	7.85	7.99	+0.14
52	.....	0.35		162	9.99	10.19	+0.20
62	0.52	0.50	-0.02	172	12.64	12.86	+0.22
72	0.73	0.71	-0.02	182	15.91	16.08	+0.17
82	1.02	1.01	-0.01	192	.....	19.91	
92	1.42	1.42	0.00	202	.....	24.45	
102	1.95	1.96	+0.01	212	29.80	29.80	0.00
112	2.65	2.67	+0.02	250.3	59.60	59.69	+0.09
122	3.57	3.58	+0.01	293.4	119.20	118.32	-0.88
132	4.68	4.74	+0.06	343.6	238.40	237.60	-0.80

I believe it is now generally considered that the temperature 212° is that of water boiling when the barometer is at 30 inches instead of 29.8; and if in the above algebraic expressions the following alterations be made, the results from the formulæ will correspond with the adjustment of that point, and fully as well with the experiments generally.

Let  $T = t + 51.3$ ; the index of the power and of the root be 5.13, instead of 5.14; and  $m = 87344,000000$ . So the two last equations will be:  $5.13 LT - 10.94123 = LE$ ; and  $\frac{LE + 10.94123}{5.13} = LT.$



The table will stand as follows, supposing the thermometer had been graduated for 212° to correspond with 30 inches of the barometer :

Tempe- rature.	Observed elasticities.	Calculated elasticities.	Differ- ences.	Tempe- rature.	Observed elasticities.	Calculated elasticities.	Differ- ences.
	In.	In.	In.		In.	In.	In.
32	*0.16	0.18	+0.02	142	6.10	6.22	+0.12
42	*0.23	0.25	+0.02	152	7.90	8.03	+0.13
52	*0.35	0.35	0.00	162	10.05	10.25	+0.20
62	0.52	0.50	-0.02	172	12.72	12.94	+0.22
72	0.73	0.71	-0.01	182	16.01	16.17	+0.16
82	1.02	1.01	-0.01	192	.....	20.04	.....
92	1.42	1.42	0.00	202	.....	24.61	.....
102	1.96	1.97	+0.01	212	30.00	30.00	0.00
112	2.66	2.68	+0.02	250.3	60.00	60.11	+0.11
122	3.58	3.60	+0.02	293.4	120.00	119.17	-0.83
132	4.71	4.76	+0.05	343.6	240.00	239.28	-0.72

I remain, with the greatest esteem and respect,

Dear Sir, your very obedient Servant,

Oakhill, 26th March, 1814.

JOHN SOUTHERN.

To James Watt, Esq., Heathfield.

P.S. Some circumstances which occurred in the performance of the experiments (made in 1797 and 1798) of which the results are last related, suggested the trials of a mixture of air with the steam; and I made a few, not indeed with the greatest nicety, but as they furnished a strong probability that the following law of elasticity of a given mixture was either nearly or accurately correct, it may be of use to say, that the apparatus used in the steam experiments being prepared as if for a repetition of them, and as perfectly exhausted of air as for them, a known measure of common air was sent up the tube through the mercury and water, and took its place in the ball; the water surrounding which was heated, and its temperature observed at different periods as before; and indeed the process was precisely the same as the former, with the additional notice of the space in the ball occupied by the expanded air and steam jointly. This process was repeated three or four times with different quantities of air, but the notes not being preserved, I can only now mention the conclusion they induced me to form as to the law above-mentioned, viz. that whatever the elastic force of the air admitted would be in its expanded state, supposing it dry and to occupy the whole

\* These are inserted from numerous experiments made by Mr. W. Creighton. Mr. W. Creighton published a theorem for the elasticity of steam at different temperatures in the *Philosophical Magazine*, 1819, vol. liii. p. 266.—Ed.

empty space in the ball (not occupied by water), after adding for the increase of its elasticity by the increase of temperature, it was yet to be augmented by the elasticity which steam alone of the same temperature would possess, to give the elasticity of the mixture.

Let  $b$  = bulk of air introduced into the ball, measured before its introduction.

$A$  = its elasticity (expressed in inches of mercury, or the height of the common barometer).

$B$  = bulk occupied by it jointly with the steam in the ball, when their common temperature, governed by that of the water in the pan, is  $t$ .

$1 : r$  = ratio of elasticity which the air had before introduction to what it would have by augmenting its temperature to  $t$ ; or, which is nearly the same, the ratio of expansion of air by the augment of temperature to  $t$ , when under the same pressure.

$E$  = pillar of mercury (in inches) which steam of the temperature  $t$  would support:

Then  $E + \frac{2bA}{B}$  = pillar of mercury (in inches) which would be supported by the elasticity of the mixture at the temperature  $t$ .

*Example.* Suppose  $b = \frac{1}{2}$  cubic inch;  $A = 30$  inches;  $t = 102^\circ$  10 to 11, ratio of elasticity of air at the temperature at which it was introduced to what it would possess at 102, consequently  $2 = 1.1$ ;  $B = 6$  inches, and  $E$  (by the table) 1.95. Then  $1.95 + \frac{1}{2} \times 30 \times 1.1 \div 6 = 4.70$  for elasticity of the mixture, or column of mercury it would support.

## XXV. *On the Exhalation of Bicarbonate of Ammonia by the Lungs.* By LEWIS THOMPSON, M.R.C.S., &c.

*To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,

HAVING lately had occasion to ascertain the amount of moisture given off by the lungs of several healthy individuals during a fixed period, I was induced to examine the nature of the fluid thus condensed. The result has proved that bicarbonate of ammonia is constantly exhaled from the lungs to the extent of rather more than three grains every twenty-four hours for each individual; and although this quantity may appear trifling, yet the amount arising from a large population like that of London is well worthy of notice,

and must exceed 150 tons of solid bicarbonate of ammonia per annum: and if, as is extremely probable, other animals also exhale this substance, the atmosphere must not only always contain enough of this agent for the purposes of vegetation, but, by a reciprocal action, the mutual increase of vegetables and animals would only tend to render the air better adapted for the due development of both. The existence of ammonia in the breath may easily be demonstrated, by respiring air which has passed through diluted sulphuric acid, and then expiring it through a tube surrounded by water at 32° F., to the further end of which a vessel is attached to receive the fluid which condenses. On acidulating this fluid with one or two drops of pure muriatic acid and evaporating to dryness on a water-bath, a residue will be obtained, which, when dissolved in five or six drops of water and introduced into a small test-tube, will give off ammonia on the addition of two or three drops of a strong solution of potash, as evidenced by its action on turmeric paper and muriatic acid, or by its peculiar smell. The respiratory process should be continued for an hour or two.

It would be interesting to know whether any difference is observable in the amount of ammonia exhaled from the lungs of individuals suffering from disease of the kidney, diabetes, &c.

I am, Gentlemen,

Your most obedient Servant,

Byker Bar, Newcastle-on-Tyne,  
Jan. 13, 1847.

LEWIS THOMPSON.

## XXVI. *Proceedings of Learned Societies.*

### ROYAL SOCIETY.

[Continued from p. 59.]

Jan. 7, “**Q**UELQUES Recherches sur l'Arc Voltaïque; et sur 1847. l'influence qu'exerce le Magnétisme, soit sur cet Arc, soit sur les Corps qui transmettent les Courants Electriques Discontinus.” By M. Auguste De la Rive, Foreign Member of the Royal Society, Professor in the Academy of Geneva, Corresponding Member of the Academy of Sciences of Paris, &c.

In the first section of this memoir the author gives a detailed description of the phenomena exhibited by the luminous voltaic arc produced either in a vacuum or in atmospheric air, or in hydrogen gas, by employing electrodes of different kinds of conducting substances, in the form either of points or of plates. He examines minutely the transfer of particles which takes place from one pole to the other under these various circumstances, and the differences which occur when the currents are reversed. He observed that when a positive metallic point is presented to a negative plate, particles of the former are transported by the voltaic arc, and deposited

on the latter, forming a ring of a regular form, having as its centre the projection of the point on the plate. This happens in atmospheric air whether highly rarified or of the ordinary density, but not in hydrogen gas. This deposit consists always of oxidized particles of the positive metal which forms the pointed electrode. In the case of platinum, the circular spot is of a blue colour, and presents the appearance of the coloured rings of Nobili. This effect the author is disposed to ascribe to the action of the oxygen brought by the voltaic current into that particular condition which Schœnbein first described under the name of *Ozone*. While this deposit is taking place, a vivid blue light is emitted.

In the second section the author investigates the action exerted by a powerful electro-magnet on the voltaic arc. He describes the remarkable modifications which the length, the form, and even the nature of the arc undergoes when the electro-magnet is brought extremely near to it, and the magnetization of the electrodes themselves, when they are susceptible of that affection by their approximation to the electro-magnet. He notices the singular phenomenon of a peculiar sound emitted by the luminous arc, when subjected to this magnetic influence; a sound which varies both in its nature and its intensity according to the nature, the form and the temperature of the electrodes, consisting sometimes of a shrill whistle, and at other times of a series of slight detonations.

The third section is devoted to the investigation of a remarkable phenomenon presented by all the conducting bodies while transmitting discontinuous electric currents, under the influence of a powerful electro-magnet; namely, the emission of a sound resembling that of the revolving toothed-wheel in Savart's experiments. This sound is distinctly heard, and is peculiarly loud with prismatic bars of lead, bismuth, tin, &c., about three-quarters of an inch square and a foot and a half long, whether placed in the direction of a line joining the poles of an electro-magnet, or in a direction at right angles to such line: it was weakened only by increasing the distance between the poles and the bar. The intensity of the sound appeared to depend much less on the nature of the substance which was subjected to this action, than on its form, its volume, and its mass. All conducting bodies, whatever may be their nature, or state of aggregation, are capable of yielding these sounds. They are produced by charcoal of all kinds and shape. Mercury contained in a cylindrical glass tube, of similar dimensions with the metallic bars, emits a sound of great intensity; and a still louder sound arises from a wire coiled as a helix around a cylinder of wood, and also by tubes formed of different metals. Similar phenomena are also observable by the action of a helical coil substituted for the electro-magnet.

On the whole, the author arrives at the conclusion that the phenomena noticed in this paper are altogether molecular, and that they establish the following principles: first, that the passage of the electric current modifies, even in solid bodies, the arrangement of the particles; and secondly, that the action of magnetism, in like manner, produces an analogous modification in the molecular constitu-



tion of all bodies. This has already been demonstrated by Faraday in the case of transparent bodies, in its effects on polarized light; and is now extended by M. De la Rive to opaque conducting bodies, by employing, instead of polarized light, a discontinuous electric current.

"On the Ganglia and Nerves of the Virgin Uterus." By Robert Lee, M.D., F.R.S., &c.

The author states that his recent dissections have enabled him to verify the descriptions he gave of the ganglia and nerves of the uterus in his papers already published in the *Philosophical Transactions*, and also to detect the existence of ganglia situated in the muscular coat of the uterus, and of plexuses of nerves accompanying all the blood-vessels and absorbents ramifying in its walls, between the peritoneum and lining membrane. By examining the hearts of a foetus, of a child of six years of age, of an adult in the sound state, a human heart greatly hypertrophied, and the heart of an ox, he found that there exists a striking analogy between the ganglia and nerves of the uterus and those of the heart. He ascertained by microscopic observation that the muscular and vascular structures of the auricles and ventricles are endowed with numerous ganglia and plexuses of nerves, which, as far as he knows, have not yet been described, and which enlarge simultaneously with the natural growth of the heart, and also continue to enlarge during its morbid conditions of hypertrophy. The author also finds that the size of the ganglia and nerves of the left auricle and ventricle, in the normal state, is more than double that of the corresponding parts on the right side. A description is then given of two elaborate drawings which accompany the paper.

"On a new and practical form of Voltaic Battery of the highest powers, in which Potassium forms the positive element." By John Goodman, Esq. Communicated by S. Hunter Christie, Esq., A.M., Sec. R.S.

The author succeeded in constructing a voltaic arrangement of some power by fixing a piece of potassium to the end of a copper wire, placed in a tube containing naphtha, and bringing it in contact with a small quantity of mercury, held by a layer of bladder closing the lower end of the tube, which was itself immersed in acidulated water immediately over a piece of platinum, and then completing the circuit by establishing a metallic contact between the copper wire and the platinum. This battery acted with energy on the galvanometer, and effected the decomposition of water. A series of twelve pairs of similar plates exhibited a sensible attraction of a slip of gold leaf. Thus it appears that the substance which possesses the highest chemical affinity manifests also the greatest power of electrical tension.

Jan. 21. "On Photographic Self-registering Meteorological and Magnetical Instruments." By Francis Ronalds, Esq., F.R.S., &c.

The apparatus employed by the author at the Kew Observatory, and which he terms the Photo-Electrograph, is described by him in the following words:—"A rectangular box, about sixteen inches

long and three square, constitutes the part usually called the *body* of a kind of lucernal microscope. A voltaic electrometer (properly insulated, and in communication with an atmospheric conductor) is suspended within the microscope, through an aperture in the upper side, and near to the *object* end. That end itself is closed by a plane of glass, when daylight is used, and by condensing lenses, when a common Argand lamp is employed. In either case an abundance of light is thrown into the microscope. Between the electrometer and the ether, or eye-end of the microscope, fine achromatic lenses are placed, which have the double effect of condensing the light upon a little screen, situated at that eye-end, and of projecting a strong image of the electrometer, in deep *oscu*ro, upon it. Through the screen a very narrow slit, of proper curvature, is cut (the chord of the arc being in a horizontal position), and it is fitted into the back of a case, about two-and-a-half feet long, which case is fixed to the eye-end of the microscope, at right angles with its axis, and vertically. Within the case is suspended a frame, provided with a rabbet, into which two plates of pure thin glass can be dropped, and brought into close contact by means of six little bolts and nuts. This frame can be removed at pleasure from a line, by which it is suspended, and the line, after passing through a small aperture (stopped with grease) cut through the upper end of the long case, is attached to a pulley (about four inches in diameter), fixed, with capacity of adjustment, on the hour arbor of a good clock. Lastly, counterpoises, rollers, springs, and a straight ruler are employed for ensuring accurate rectilineal sliding of the frame, when the clock is set in motion.

"A piece of properly prepared photographic paper is now placed between the two plates of glass in the moveable frame; the frame is removed (in a box made purposely for excluding light), and is suspended in the long case; this is closed, so as to prevent the possibility of extraneous light entering with it; the clock is started, and the time of starting is noted.

"All that part of the paper which is made to pass over the slit in the screen, by the motion of the clock, becomes now therefore successively exposed to a strong light, and is consequently brought into a state which fits it to receive a dark colour on being again washed with the usual solution, excepting those small portions upon which dark images of the lower parts of the pendulums of the electrometer are projected through the slit. These small portions of course retain the light colour of the paper; and from the long curved lines or bands, whose distances form each other, at any given part of the photograph, i. e. at any given time indicate the electric tension of the atmosphere at that time.

"By certain additions to the instrument above described, the kind as well as the tension of electrical charge is capable of being registered; and by the employment also of a horizontal thermometer, &c., it is adapted to the purposes of a *Thermograph*, as well as *Photo-barometrograph* and *Magnetograph*."

## CAMBRIDGE PHILOSOPHICAL SOCIETY.

[Continued from vol. xxix. p. 66.]

February 1846.—The Mathematical Theory of the two great Solitary Waves of the first Order. By S. Earnshaw, M.A.

The nomenclature of this paper is adopted from a Report on Waves by Mr. J. S. Russell, printed in the Proceedings of the British Association. From the extreme comprehensiveness of the equations of fluid motion, the author infers a necessity of appealing to experiments for the suggestion of data which may be used in modifying the generality of those equations so as to suit actual cases of known fluid motion. With this view he has made use of the experiments recorded in Mr. Russell's report, and thence selected the two following properties:—1st. The velocity of transmission of a wave in a uniform canal is constant. 2nd. The horizontal velocity is the same for all particles situated in a vertical plane, cutting the axis of the canal at right angles. By reference to Mr. Russell's report, it will be seen that these two properties, selected on account of their simplicity and ready experimental examination, are distinguishing characteristics of what he has denominated the two great solitary waves of the first order. By the aid of them the equations of motion take such modified forms as to admit of exact integration; so that without employing any analytical approximations the author is enabled to obtain theoretical expressions for all the circumstances of the two solitary waves. The results are tested by a comparison of the velocities of transmission of various waves given by theory and by experiment. The greatest difference of these in the case of the positive wave is not found to exceed  $\frac{1}{3}$ th part of the whole velocity; but in the case of the negative wave it is found to be much greater, and to amount in one instance to as much as  $\frac{1}{2}$ th of the whole velocity. The reason of this discrepancy is conjectured; and the agreement in the case of the positive wave is considered to be exact.

It is found in the course of the investigation that one of the necessary conditions of fluid motion is not satisfied; and it is shown that it cannot be satisfied as long as the two principles, adopted from Mr. Russell's report, are supposed to coexist. They are proved in fact to be incompatible with each other. But as the second principle was found by Mr. Russell to be so nearly exact that he could not detect any deviation from it in his experiments, it is shown by theory that from this circumstance there will be a rapid degradation of the summit of the wave, and a consequent loss of the velocity of its transmission, both which results of theory were observed to be true experimentally. The memoir concludes with pointing out the agreement of theory with some minor phenomena noticed by Russell.

May 25, 1846.—Cases of Morbid Rhythmical Movements, with observations. By G. E. Paget, M.D., Fellow of Caius College and of the Royal College of Physicians, London.

Seven cases were related. The movements were vibratory, rotatory, bowing, &c. In some of the cases they were incessant; in others paroxysmal; and in others again they were of both kinds,

the predominant movement being replaced at intervals by distinct paroxysms.

On a comparison of these cases with the few others on record, and with the experiments of Flourens and Majendie, it was inferred as probable, that one class of the movements, viz. the rotatory, depended on disorder in the cerebellum or its transverse commissure, the pons. With regard to the other movements, it appeared that there were no sufficient grounds for even a probable conjecture as to the particular part of the encephalon, the excitement or disorder of which might act as an immediate cause of the movements.

The *remote* causes were such as under other circumstances are known to excite the common convulsive diseases, such as chorea and epilepsy. These remote causes were in most cases *eccentric*.

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#### ROYAL ASTRONOMICAL SOCIETY.

[Continued from vol. xxix. p. 232.]

Nov. 13, 1846.—An Explanation of the observed Irregularities in the Motion of Uranus, on the Hypothesis of Disturbance caused by a more distant Planet; with a Determination of the Mass, Orbit, and Position of the disturbing body. By J. C. Adams, Esq., M.A., F.R.A.S., Fellow of St. John's College, Cambridge\*.

The author introduces the subject by remarking, that when Bouvard constructed his Tables of Uranus (those now commonly in use), he found it impossible to reconcile the ancient observations, made before the discovery of Uranus as a planet, with the modern observations, and that therefore in the formation of his tables he relied solely upon the latter; but that in a very few years the still more modern observations exhibited a departure from the tables nearly as great as the ancient ones, and therefore there seemed now to be no sufficient reason for rejecting the ancient observations. The author then states that his attention was first directed to this subject by reading the report on the recent progress of astronomy made to the British Association at their meeting in Oxford; and that in July 1841 he formed a design of investigating the yet unaccounted-for motions of Uranus, in order to discover whether they could be explained by an exterior disturbing planet. In 1843 he made a first attempt, supposing the orbit of the disturbing planet to be a circle, and its mean distance twice that of Uranus. This investigation was founded exclusively on the modern observations, using, as far as 1821, the errors given in the equations of condition in Bouvard's tables, and for subsequent years the errors given in the *Astronomische Nachrichten*, and the Cambridge and Greenwich Observations. The result showed a good general agreement of the observed disturbance with the disturbance which would be produced by the action of such a planet. In February 1844, the author received from the Astro-

\* This paper was presented to the Society on the evening of Nov. 13, 1846. It has since been published entire as an extract from the Appendix to the Nautical Almanac for 1851.



nomer Royal the results of the general reduction of the Greenwich Planetary Observations.

In the meantime the Göttingen Academy had proposed for the subject of a prize the theory of Uranus, and though the author had no hope of being able to complete an essay in time to compete for the prize, he was stimulated by the publication of this proposal again to enter on the investigation. He now took into account the possible eccentricity of the disturbing planet to the first order, retaining the same assumption for mean distance. For the modern observations, the tabular errors used, as far as 1830, were exclusively those of the Greenwich Observations, except one by Bessel in 1823; after 1830 the Cambridge and Greenwich determinations and those in the *Astronomische Nachrichten* were used. Those for the observations anterior to the discovery of the planet were taken from Bouvard.

Results for the elements of the disturbing planet were obtained, which were communicated in September 1845 to Prof. Challis, and in October 1845 (slightly altered) to the Astronomer Royal. Afterwards the investigation was repeated, supposing the mean distance diminished by about  $\frac{1}{30}$ th part. The results were communicated to the Astronomer Royal in September 1846. They seemed to show that the mean distance ought to be still further diminished.

The author, after adverting to the dates of M. Le Verrier's papers, and showing that his own calculations were earlier in date, says, "I mention these dates merely to show that my results were arrived at independently and previously to the publication of M. Le Verrier, and not with the intention of interfering with his just claims to the honours of the discovery; for there is no doubt that his researches were first published to the world, and led to the actual discovery of the planet by Dr. Galle, so that the facts stated above cannot detract, in the slightest degree, from the credit due to M. Le Verrier.

The investigations proceeded as follows:—First, to diminish the number of equations, the results were collected in groups of three years each; and these were so arranged as to present results nearly independent of the error of radius vector. Thus twenty-one equations were obtained; and these, without extension for the two or three last years (which might subsequently have been included, but which would have disturbed the similarity of the calculations) were also used in the subsequent calculations for a different assumed mean distance. Then all the principal inequalities in the recognised theory of Uranus were verified, and corrections for an error pointed out by Bessel, and for the altered mass of Jupiter, were applied, as well as for some terms of the second order of masses pointed out by Hansen. Other inequalities of higher orders were neglected; as their effects may be represented, either by a very slow alteration of the epoch and mean motion, or by a very slow alteration of the perihelion and eccentricity; both which may, without sensible error, be assumed as constant, during the comparatively short period through which Uranus has been observed. The author then gives a table of the differences between the theoretical longitudes (thus corrected), and the observed longitudes: the maximum values are as follows:—

In 1712	+92.7	In 1804	+24.2
1750	-47.6	1840	-66.6

These are then converted into corresponding errors of mean longitude, which the author finds more convenient.

Then, formulæ are investigated for the effects of small corrections of the elements of the orbit of Uranus, and for the perturbations of mean longitude produced by a disturbing planet, expressed in the notation of Pontécoulant. These are expanded as far as the second order of eccentricities (involving only the first power of the eccentricity of the unknown planet), and the whole is reduced to numbers, with no symbols remaining, except for functions of the corrections of the elements of Uranus, and functions of the epoch, longitude of perihelion, eccentricity and mass, of the disturbing planet. All the numerical quantities are computed on the supposition that the mean distance is double that of Uranus. Any one of these expressions, adapted to a certain time, being made equal to the error in the tabular place of Uranus for the same time, furnishes an equation of condition.

These equations of condition are treated by the method of least squares; and the successive steps of elimination are given. The author considers that the modern observations are scarcely sufficient to give the eccentricity and longitude of perihelion of the disturbing planet; but when the ancient observations (always omitting that of 1690 as uncertain) are combined, there are ample means for determining these elements. The equations, after the elimination had proceeded to a certain degree, were solved by successive substitution. The results thus obtained were—

#### Hypothesis I.

Assumed mean distance = 2 × that of Uranus.

Mean longitude, October 6, 1846 .....	325° 7'
Longitude of perihelion .....	315 57
Eccentricity of the orbit .....	0.16103
Mass (that of the sun being 1).....	0.0001656

which were communicated to the Astronomer Royal in October 1845.

The author then states that he made a second investigation, on the supposition that the mean distance of the disturbing planet = mean distance of Uranus  $\times \frac{1}{0.515}$ . The process, with very little

difference, is the same as that for the former assumption of mean distance. The formulæ, the equations, &c., are given in the same manner as before. The elements obtained thus are as follows:—

#### Hypothesis II.

Assumed mean distance = 1.942 × that of Uranus.

Mean longitude, October 6, 1846.....	323° 2'
Longitude of perihelion .....	299 11
Eccentricity of the orbit.....	0.12 0615
Mass (that of the sun being 1) .....	0.00 015003

The corrections to the elements of the orbit of Uranus are investigated on both hypotheses. Then on substituting the effects of the

corrections and the effects of the perturbations, the residual errors are obtained, of which the following are the maximum values :—

	Hypoth. I.	Hypoth. II.
1712	+ 6.7	+ 6.3
1715	— 6.8	— 6.6
1753	+ 5.7	+ 5.2
1764	— 5.1	— 4.1
1771	+11.8	+12.8 Single observation.

After this time, to the year 1840, the largest error is 2".35.

After 1840, the errors increase on both hypotheses. They are,—

	Hypoth. I.	Hypoth. II.
1843	+ 7.11	+ 5.77
1844	+ 8.79	+ 7.05
1845	+12.40	+10.18

It appears from this extremely probable that the mean distance of the disturbing planet ought to be assumed nearly = mean distance of Uranus  $\times \frac{1}{0.574}$ .

The residual errors for the single observation of 1690 are,—

Hypoth. I.	Hypoth. II.
+44".5	+50".0

It seems probable that these errors would be increased by still further diminishing the mean distance.

Expressions are then investigated for the correction of radius vector produced by the correction of elliptic elements, and by the effects of perturbation. The numerical values are as follows :—

	Hypoth. I.	Hypoth. II.
1834	+0.00505	+0.00492
1840	+0.00722	+0.00696
1846	+0.00868	+0.00825

The author states that no satisfactory results could be found for the node and inclination of the planet's orbit, as deduced from the irregularities in the latitude of Uranus.

The author then remarks that the perturbations of Saturn produced by the new planet will be undoubtedly sensible; and he suggests that it would be interesting to examine anew the theory of Saturn, and to ascertain whether the masses of Jupiter and Uranus deduced from it are consistent with those obtained by other methods. He remarks that the published reductions of the Greenwich Observations now make such an inquiry comparatively easy.

#### December 14, 1846.—Reappearance of *Astræa*.

Extract of a letter from Prof. Schumacher to Mr. Hind, dated Altona, Nov. 27, 1846.

" M. Otto Struve has already reobserved *Astræa*, which appears brighter than he expected she would.

		M.T. Pulkowa.			R.A.			Dec.		
		<sup>h</sup>	<sup>m</sup>	<sup>s</sup>						
1846	Nov. 4	17	58	49	193	55	28.4	-3	14	22.7
	13	18	4	11	198	12	42.8	-4	47	41.6

These positions are free from parallax. The resulting corrections for M. D'Arrest's ephemeris are,—

		R.A.		Dec.	
		<sup>°</sup>	<sup>'</sup>	<sup>°</sup>	<sup>'</sup>
1846	Nov. 4	-1	7.5	+0	39.3
	13	-1	8.1	+0	37.1"

Mr. Lassell forwarded a more complete account of his physical observations on Le Verrier's planet, with his Newtonian reflector, 2-feet aperture, mounted equatorially:—

Oct. 3. He received an impression of a ring, not much open, and nearly at right angles to the parallel of daily motion. Speculum A and plane metallic reflector.

Oct. 10. The same impression of a ring in the same direction. A minute star just steadily visible, with full aperture of 24 inches, powers 316 to 567, distance  $2\frac{1}{2}$  to 3 diameters, a little to the right, and *apparently below* the ring continued. Speculum B and Merz's prism.

Nov. 10. The planet very like Saturn, as seen with a small telescope and low power, but much fainter. Same speculum and prism as before.

Nov. 11. The planet still retains its appearance. A faint point of light considerably distant, in the direction of the ring and below it. Speculum A and plane reflector.

On these nights several persons saw the supposed ring, and all in the same direction, as shown by independent diagrams.

Nov. 30. A minute star above, and a little to the left of the continuation of the ring, distance 2 diameters. Speculum B and Merz's prism.

Dec. 3. The same relative appearance exactly of planet and small star as on October 10th. The direction of the ring estimated at about  $70^\circ$  with the parallel of daily motion. The small star about 3 diameters distant and  $50^\circ$  N. following. Telescope as before.

Dec. 4. No minute star visible, though carefully looked for. Telescope as before.

Mr. Lassell says that the points of light seen on Oct. 10, Nov. 30, and Dec. 3, were, so far as he could judge, the same in appearance and brightness. There were no other stars in the immediate neighbourhood. Hence he conceives the probability is in favour of the star being a satellite.

With respect to the ring, Mr. Lassell says that "he has never looked at the planet, under tolerable circumstances, without receiving the same impression of its existence;" and that so far as he can judge, the direction of the supposed ring makes a constant angle with the meridian, and not with the horizon; but this is not very certain.

In speaking of the quality of his telescope, Mr. Lassell says that he finds Merz's prism gives him more light than a plane reflector,



and with no loss of distinctness ; and that he has succeeded in bringing the foci of his exterior and inner surface to agree within a hundredth of an inch. The telescope shows seven stars in and two near the trapezium of Orion, and so far approaches to *resolving* the nebula, that it shows " stars, the centres of nebulous clouds, scattered all over the nebula."

In confirmation of Mr. Lassell's idea that there is a ring about the new planet, Mr. Hind stated that the South Villa telescope shows it oblong, and that the major axis makes an angle of about  $30^\circ$  with the meridian.

Reduction of Tycho Brahe's Observations of the Comet of 1590, with Elements deduced therefrom. By Mr. Hind.

" The comet of 1590 was first perceived by Tycho Brahe on the evening of March 5, while he was employed in observing the planet Venus. It was situated near the Northern Fish, between Aries and Andromeda. The diameter of the head was three minutes, and a faint tail was visible, extending from  $7^\circ$  to  $10^\circ$ , and directed towards the zenith at about 7<sup>h</sup> 30<sup>m</sup> P.M.

" M. Pingré, in his *Cometographie*, tome i. p. 554, has given a series of observations on this comet made by Tycho, and extracted from a manuscript preserved at the Dépôt de la Marine in Paris. The observations are detailed at considerable length in this manuscript; but the table in M. Pingré's work is an abstract made by Tycho himself, and I have used it as the basis of my calculations. For deducing the places of the comet, we have the apparent times of observation at Uraniburg, the distances from known stars, and the declinations for the same times as these distances: also in most cases, the comet's altitude, and frequently its azimuth from the south towards the west. I have calculated the positions from the observed declinations and distances, duly corrected for parallax and refraction, making use of the altitudes in the preliminary computations only. On two occasions the comet was observed on the meridian *sub polo*; in these cases I have performed the reductions in the usual manner, applying refraction and parallax."

Mr. Hind then presents tabular results of his reductions at the following stages:—

An ephemeris of the sun.

A catalogue of the stars of comparison.

An *approximate* ephemeris of the comet, for computing the effects of aberration and parallax.

The observed distances of the comet from the stars corrected for refraction, aberration and parallax, with the Uraniburg mean time, and the resulting right ascension and declination of comet.

" The following longitudes and latitudes are computed from a mean of each day's observations of right ascension and declination, omitting, however, the third result for March 6, and the first for March 7. They are referred to the apparent equinox of the date :

Mean time at Uraniburg.	Comet's geocentric longitude.	Comet's geocentric latitude.
March 5-36087	18° 15' 25"	+18° 12' 58"
6-29647	25 46 21	19 27 44
7-38396	33 21 14	20 21 32
8-32461	38 56 11	20 51 1
10-32981	48 6 39	21 20 26
11-43862	52 2 3	21 14 33
12-36629	54 49 1	21 9 26
13-35634	57 23 19	21 5 22
14-39625	59 40 14	20 58 14
16-33600	63 10 12	+20 47 55

After a great many trials I at last fixed upon a parabolic orbit, which appears to represent the observations with as much, or nearly as much, accuracy as they admit. It would be manifestly a hopeless business to attempt any determination of the particular conic section described by the comet, from positions extending over so short a period, and at the same time so rough. Should the comet reappear, the observations of 1590 will be in a more tangible form than heretofore."

Parabolic Elements.

Passage through Perihelion, 1590, Feb. 8-0624, Uraniburg Mean Time, New Style.

Longitude of Perihelion on the orbit	217° 57' 12"	} App. Eq. March 10.
Ascending Node .....	165 56 56	
Inclination to the Ecliptic .....	29 29 44	
Log. distance in Perihelion.....	9-7541386	
Heliocentric motion.	Retrograde.	

"These elements, compared with the above longitudes and latitudes, give the following differences :—

Computed—Observed Place.		
March 5	Long. + 34"	Lat. — 75"
6	+ 38	0
7	— 23	+160
8	—126	+ 91
10	+ 75	—346
11	+ 29	+ 5
12	— 10	+106
13	— 95	+ 6
14	+ 8	+ 3
16	+ 69	—272

"The comet of 1590 is described by Martin Mylius in his *Annales Gorlicenses*, published as part of a work entitled *Scriptores Rerum Lusaticorum*, Lipsiæ et Budissæ, 1719, in folio. It was also the subject of a treatise by Matthias Menius, printed in 1591. Lubienietski, in his great work *Theatrum Cometicum*, gives us a chart showing the path of the comet in the heavens : he quotes Ricciolus and other writers."

It is almost unnecessary to say that the reductions have been made on the best existing data, and according to the most approved methods.

On Luminous Rings round Shadows. By the Rev. Baden Powell, Savilian Professor of Geometry, Oxford.

The remarkable optical phenomena attending total solar eclipses, especially the formation of a luminous ring round the discs, are now on the whole so well established, that speculations have been hazarded as to their cause: and though these speculations as yet scarcely amount to more than very general illustrations, yet as experimental facts have been elicited which seem to bear a resemblance to the phenomena in question, they deserve notice, even in the present uncertain state of the inquiry.

The author disclaims any idea of bringing forward such facts as a complete explanation; but he is induced to offer them because *one* experiment devised by him long since was thought by the late Mr. Baily worthy of being laid before the Society, though communicated to him without any such intention. (Ast. Soc. Notices, March 10, 1843.)

The following is the author's historical summary of the observations of the luminous ring:—

The phenomenon appears to have been observed as early as the time of Plutarch, as appears by a passage from him cited in the *Comptes Rendus*, 1842, i. 847. In more modern times, probably the earliest recorded instance is in the eclipse of 1567 [this is clearly wrong in date or name] observed by Kepler. The ring was observed at Naples in 1605 (Phil. Trans. vol. xl. 177); and from the year 1706 we have the full and well-known records of the most eminent astronomers down to the present day, which establish the phenomenon as a regular part of the appearance of a total eclipse. The author refers to instances in the Mem. Ast. Soc., vol. i. p. 144, and vol. x. p. 9–16, 37, &c., and to the Notices of this Society, vol. v. p. 207; as also to the *Comptes Rendus* for 1842, parts 1 and 2. He remarks that remarkable discrepancies are found in the accounts of the same, as well as of different eclipses, and he mentions instances. Some of these discrepancies admit of easy explanation. In the transit of Venus in 1769 light was seen surrounding the part of the planet *off* the sun, both at immersion and emersion; but this has been ascribed to her atmosphere. (Ast. Soc. Mem. vol. x. p. 27.) The singular *protuberances* observed in the eclipses of 1733 and 1842 do not seem to belong to the ring; and it seems to the author hardly conceivable that they can be purely optical phenomena, as suggested by M. Valz and M. Arago.

The author next gives an account of the attempts of different astronomers to assign a *cause* for the luminous ring. Kepler considered it to be due either to combustion round the sun, or to refraction of his rays by the moon's atmosphere. Others have more generally attributed it to some peculiar effect of the solar atmosphere; and some, especially Olbers, expressly attribute it to the zodiacal light. De l'Isle, and more lately Arago and Valz, regard it altogether

*Phil. Mag. S. 3. Vol. 30. No. 199. Feb. 1847.*

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as a phænomenon of diffraction. Several instances are then given of luminous rings surrounding terrestrial objects seen in shadow, as observed by different philosophers.

Experimental imitations of the luminous ring, by artificially eclipsing the sun, were long ago made by De l'Isle and La Hiri (*Mém. Acad. Paris*, 1715, p. 166), and such an intimation is also spoken of by Mr. Baily (*Ast. Soc. Notices*, vol. v. p. 212).

Among the experiments described by Newton in the third book of his *Optics*, one, which is emphatically mentioned as unfinished, seems to bear closely on the present subject. The sun's light being admitted through a hole one quarter of an inch in diameter, and partly intercepted by a knife-edge fixed along one side of a hole three quarters of an inch square in a screen, and falling on a paper beyond, Newton saw "*two streams of faint light* shoot out both ways from the beam of light into the shadow like the tails of comets." Again, placing his eye to receive the light, he saw "*a line of light upon the edge* (of the knife) all along it." . . . "It was contiguous to the edge and narrower than the innermost fringe," and "between the edge and the first fringe." Newton thus clearly distinguishes between this peculiar phænomenon and the ordinary effects of diffraction.

The author then proceeds to detail his experiments made for the purpose of producing the luminous ring, remarking that the essential conditions in all diffraction experiments are, that the origin of the light be as nearly a point as may be, and that the area of the rays diverging from it extend beyond the edge of the opaque diffracting body.

He finds that with apertures up to a quarter of an inch or more, and whether the area of the rays reach beyond the edge of the opaque body, or lie even considerably within it, a small circular disc, seen at a distance, either with or without a telescope, is edged by a *bright luminous ring*, which cannot be seen with the eye-lens, and is therefore *not* an optical image like the diffraction fringes, but is seen in the telescope distinctly when in focus for the opaque disc. With a straight edge the same phænomenon is observed as a line of light running along it. If the origin be a lens of short focus the ring is seen as before, but less perfectly; while the diffraction fringes are seen perfectly with the eye-lens. The origin being a hole, the ring is visible under different variations of the area of the rays, but ceases to be visible when the area is less than about a quarter of the disc. Changes in the distance of the eye from the disc produce very little, if any, change in the breadth of the ring.

The ring was examined by a telescope with cross-wires in its focus. The intersection of the wires was fixed on the edge of the disc before the light was admitted, and on its admission the ring extended sensibly beyond; on its being then fixed on the edge of the ring, the edge of the disc fell within it on shutting out the light. The experiment was repeated in various ways to determine the influence of reflexion from the edge, &c.

When the area is a hole, the disc being within the area of the



rays, if the shadow of the disc be examined by an eye-lens of two inches' focus, there appear at all points of the circumference streaks of yellowish light radiating into the shadow, and crossing at the centre they form there a round bright spot; but the streaks are supposed to be chiefly due to irregularities in the edge. An analogous phænomenon is seen when a straight edge is used. A modification of the same phænomenon is observed in the case described by the author to the British Association, at the meeting in 1846.

The experiments above were all made by means of the sun's light. There are well-known difficulties in performing any diffraction experiments by candle-light; but the author succeeded in exhibiting the ring by means of the light of a flame placed in the focus of a short lens limited by a circular aperture. When the shadow is thrown on a white screen at about one foot distance from the disc, the middle of the shadow appears faintly illuminated; but no streaks, as in the case of solar light, could be detected, nor is the faint illumination perceptible to the eye viewing the disc directly. The author in conclusion makes mention of the theory of M. Babinet, which attempts to explain the phænomenon on the undulatory hypothesis on this principle, that "at points exterior to the area of the rays there is no light, owing to the natural destruction of the secondary waves. If, then, of the two rays proceeding to any such point to destroy each other we intercept one by an obstacle, the other remains, and gives rise to a point of light at that point; that is, just beyond the obstacle, a series of luminous points thus created by the removal of interfering rays will give rise to the luminous borders on the edge." (*Nouv. Bulletin des Sciences*, Nov. 1832. Quetelet, Appendix to Translation of Herschel on Light.)

The author, in conclusion, remarks that, imperfect as the whole investigation confessedly is, it is by thus making the facts known that we may most reasonably hope to elicit some better elucidation of them.

## XXVII. *Intelligence and Miscellaneous Articles.*

### ON NEWTON'S TELESCOPE AT THE ROYAL SOCIETY.

*To the Editors of the Philosophical Magazine and Journal.*

Royal Society, Somerset House,  
Jan. 22, 1847.

GENTLEMEN,

MY attention has been directed to a communication by Mr. Heineken, in the last Number of the *Philosophical Magazine*, wherein he states, in allusion to the destruction of mechanical and other instruments, "Two lamentable examples of this are, I learn from a paper in the *Magazine of Science*, exhibited even at the Royal Society, where the reflectors of Newton and Hadley are in a state of complete dilapidation."

To this statement I beg to give an unqualified contradiction. The reflectors are in excellent preservation; and Sir Isaac Newton's

telescope, which is preserved under a glass case with the greatest care, is (considering its great age) in almost perfect condition. It is much to be regretted that individuals do not, before repeating unfounded statements, make inquiries as to their accuracy, which could easily have been done in this instance.

I am, Gentlemen,

Your obedient Servant,

CHARLES RICHARD WELP,  
Assistant Secretary.

#### THE CENTRAL SUN.

Professor Mädler of Dorpat, has published a pamphlet, in which he announces his belief that the centre of the great nebula in which our system lies, or of the congeries of stars which form the Milky Way, is in the Pleiades; and that the star Alcyone is more likely than any other to merit the title of the Central Sun. This question is not like that of an asserted planet—one which can soon be settled in the affirmative. If within the next half-century opinion on the subject should have arrived at something like either positive reception or positive rejection, it is perhaps as much as can now be expected. Meanwhile the pamphlet is before us (*Die Central Sonne*, Leipzig, 1847), and its leading points are, shortly, as follows:—

The observations of astronomers have made it highly probable, and Professor Mädler considers it as an established fact, that Newton's law of gravitation reigns throughout the sidereal space and governs the movements of all stars; which he thinks chiefly proved by the nature of the orbits of binary systems.

Setting out from this fundamental principle, he shows that, whatever may be the form of a system of fixed stars, the proper movements of the individual bodies must be *accelerated* as the distance from the central point *increases*, and that all the times of revolution of these different bodies around their common centre are nearly equal, as long as the mass in the centre of attraction has not too considerable a predominance over all the other masses.

By a very extensive and laborious set of observations and comparisons, the Professor found that the group of the Pleiades forms the only point in the heavens to which the preceding conclusion is applicable; that really the velocity of the *true proper movements of the fixed stars increases insensibly from this group in all directions*; and that, moreover, most of them are moving in the same sense.

He therefore assigns the Pleiades as the central group of that stellar system which is terminated by the Milky Way (to which our own and all the isolated suns belong), and the star Alcyone as that body which, most probably, is the proper central sun of this system.

From the proper movement and parallax of 61 Cygni (taking the latter =  $0''.3483$ ), he attempts to deduce the distance of Alcyone from our sun, and obtains the following approximate results:—

The parallax of Alcyone =  $0''.006097$ , or the distance from the sun 34 millions of radii of the earth's orbit.

The sun accomplishes one entire revolution around this star in 18,200,000 years, and moves in its orbit at the rate of 36·8 English miles per second.

The sum of all the masses contained within a globe described around Alcyone with the radius vector of the solar system amounts to 117,400,000 times the mass of the sun.

The ascending node of the sun's orbit is situated in long.  $236^{\circ} 58'$  of the ecliptic of 1840; and the sun will pass through this point about the year 154,500 of our chronology.

The inclination of the sun's orbit to the ecliptic of 1840 is  $84^{\circ} 0'$ .

Regarding the constitution of this immense system, Professor Mädler has come to the following conclusions:—

The centre is marked by a group consisting of a great number of stars and considerable individual masses. Around this stretches a narrow zone, comparatively devoid of stars. Then follows a broad and rich ring-shaped layer, then another intermediate zone comparatively poor, and so on, a succession of a still unknown number of rings, the two utmost of which form the Milky Way. These rings are connected in several places with each other by intermediate parts, like bridges; and the rings themselves are not everywhere of the same density, but show now and then something approaching to the formation of groups. In general, however, they consist only of isolated, single or double stars.

In the course of his investigations, the learned Professor points out the importance of studying more closely the proper movements of the fixed stars; and recommends chiefly those in the neighbourhood of the Pleiades to the attention of astronomers.—*Athenæum*, Jan. 9, 1847.

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#### ON THE FERMENTATION OF TARTARIC ACID AND ITS PRODUCTS.

BUTYRO-ACETIC ACID. BY M. NICKLES.

In preparing tartaric acid from crude tartar, it often happens, during the heat of summer, that the tartrate of lime still containing some fermentable matters, suddenly begins to ferment, and is converted, in a short time, into an acid which has long been taken for acetic acid.

On the occurrence of this fermentation, M. Noellner saturated the product with oxide of lead, and obtained fine octahedral crystals, on attentively examining which, he considered them as the salt of a peculiar acid different from acetic acid, and to which he gave the name of *pseudo-acetic acid*. An experiment performed with crude tartar, without the addition of lime, yielded merely acetic acid.

M. Berzelius, in his Annual Report for 1843, p. 132, considers this acid as a mixture of acetic and butyric acids. M. Noellner, however, had obtained a salt of lead crystallizing in octahedrons, a salt of soda also octahedral, and a mammillated magnesian salt, none of which forms occur in the corresponding salts of acetic and butyric acids.

To clear up the point, M. Nicklès saturated eight pounds of crude tartar with lime; the filtered liquor was boiled with gypsum; the

tartrate of lime produced by the second reaction was added to that obtained by the first, with all the organic substances, and exposed to a fine summer sun at a temperature of  $95^{\circ}$  to  $104^{\circ}$  F. and  $113^{\circ}$  F. The disengagement of gas took place very slowly, and it required six weeks to produce a small quantity of a substance possessing all the properties about to be described.

It appears from the experiments of M. Nicklès, that changes effected in tartaric acid by fermentation, may take place in three modes; if this acid be free or combined with potash, it is converted into acetic and carbonic acids; if the potash be replaced with lime, the tartaric acid yields carbonic acid, acetic acid and butyric acid (*pseudo-acetic acid*); and lastly, under circumstances which as yet are undetermined, the tartaric acid is converted into carbonic acid, acetic acid, and a new acid which M. Nicklès calls *butyro-acetic acid*, it containing the elements both of butyric and acetic acids, and these acids occur among the products of its decomposition.

On determining the silver of a salt which M. Noellner had sent to M. Nicklès, the latter obtained the same result as the former; when, however, the acid was set free by the decomposition of the salt of lead with sulphuric acid, then saturated with carbonate of ammonia and precipitated by nitrate of silver at a boiling heat, M. Nicklès obtained, at first, fine crystals of acetates, and then from the residual solution, magnificent dendritical crystals of butyrate of silver. The separation of the two acids may also be effected by chloride of calcium; this salt dissolves the acetic acid only, whereas the butyric acid collects it in an oily form on the surface; M. Nicklès states several new facts as to the mode in which a mixture of acetic and butyric acid acts. Thus butyric acid does not precipitate acetate of lead if mixed with a small quantity of acetic acid. The influence of the latter extends yet further; if a mixture of butyric and acetic acids be saturated with oxide of lead, and excess of ammonia be added, small rose-coloured acicular surbasic crystals of butyrate of lead are formed, which owing to a small quantity of interposed acetate are soluble in water. This surbasic salt absorbs carbonic acid rapidly from the air. A mixture of acetate and butyrate of barytes is capable of dissolving a large quantity of chloride of lead, but without forming any definite compound.

Butyro-acetic acid possesses properties which prevent its being considered as a mere mixture of butyric and acetic acids. It was obtained by the decomposition of a salt of lead which had been exposed to the air more than two years; its odour resembles that of a mixture of the two acids, but less pungent; it is soluble in water, alcohol and æther. It is obtained pure by decomposing its salt of soda by phosphoric acid; it then rises to the surface of the liquid in the state of an oily stratum; its boiling-point is nearly  $284^{\circ}$  F. When heated with sulphuric acid, it is slightly altered with the evolution of sulphurous acid gas.

The crude acid yields a liquid which produces with barytes a prismatic compound; it is very soluble in water, especially when hot, and but slightly soluble in absolute alcohol; it is perfectly per-



manent in the air, but still yields the odour of rancid butter. Exposed to a current of dry air at  $212^{\circ}$ , this salt loses 3.25 per cent. of water; at  $392^{\circ}$  it fuses, and again loses 2.80 per cent. Thus dried, the salt consists of  $[C^3(H^3Ba)O^3]$ . By dry distillation it yields a non-acid oily body; the last mother-waters yield acetate of barytes. Butyro-acetic acid readily yields an æther with alcohol and sulphuric acid. It has the smell of fruit. With respect to this acid, it will be observed that it is isomeric with metacetic acid, obtained during the last year by M. Gottlieb from potash and sugar. M. Nicklès has not had sufficient opportunity to ascertain the identity of these two acids.—*Journ. de Chim. Méd.*, Novembre 1846.

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ON THE IGNITION OF BRASS WIRE IN THE VAPOUR OF ALCOHOL.

BY M. REINSCH.

It has hitherto been supposed that the property of igniting in the wick of a spirit-lamp was peculiar to a coil of platina or palladium wire. The author has lately discovered that all other metals possess this property, which do not melt at a red heat, even whether they oxidize or not. If a coil of piano-wire be placed in the wick of a spirit-lamp (absolute alcohol), and it be allowed to burn till the spiral is sufficiently red-hot and then extinguished, the spiral remains red-hot for a few seconds and then ceases to be so. If the lamp be thus lighted for three or four times, the iron wire remains red-hot exactly like a platina wire, and the peculiar odour of aldehyd and acetic acid is at the same time developed. If the experiment does not succeed, the flame of the alcohol must be covered with a glass tube, and not extinguished till this precaution has been taken. M. Reinsch states that he could not succeed at first with brass, silver and copper wire; brass wire should be thin, and the spirals made as close to each other as possible, but they must not touch; the flame of the alcohol also should not be continued longer than is necessary to ignite the brass wire, and after this it ought to be suddenly extinguished to prevent the wire from melting, and to prevent the spirals from touching each other. Copper and brass wire continue as vividly red-hot as platina wire, though they are gradually acted upon by the acetic acid and afterwards volatilized. The silver wire does not remain so long ignited. The wires of the metals mentioned absorbing heat too rapidly, remain but incompletely red-hot, but a thicker iron wire remains red, being not so good a conductor of heat. Silver wire becomes but imperfectly ignited, because being ignited, it becomes so soft that the spirals soon touch each other. The same occurs with a gold wire.

The author also succeeded in producing ignition with charcoal; for this purpose a fragment of charcoal is placed in the wick; the alcohol is removed from the lamp, except sufficient to moisten the wick, which is to be lighted and the flame is to be cautiously extinguished; these experiments, the author observes, explain a fact which has hitherto been an enigma—the ignition of platina and the inflammation of hydrogen gas by spongy platina. Thus in the first experiment, the vapour of alcohol being imperfectly consumed

by the fire of the brass wire still hot, there is produced exactly the heat necessary to continue it incandescent. If the spirals of a brass wire approach each other without touching, the vapour of the alcohol oxidizes, and can act on a large surface; the heat is so much increased by it, that the spirals remain red-hot; if the spirals be further removed from each other, ignition ceases, the heat produced by the imperfect combustion not being sufficient to heat the spirals, which before almost touched.

As to what relates to spongy platina, the globules are surrounded with an atmosphere of hydrogen, oxidation takes place at the same time, heat is consequently developed by the large surface of the spongy platina, and a reiterated contact of the gases so increases the heat, that the gas is inflamed. Platina would not possess this specific quality if it were not so refractory as not to fuse by the heat of inflamed hydrogen.

M. Reinsch has also found that this property belongs to metals which fuse readily and are not capable of being made into wire. He made the following experiment: some asbestos was moistened with a solution of these metals, and after having made it red-hot, and it had become brittle by the ignition, it was moistened with a little alcohol, after which it was cautiously rolled round the wick; the metallized asbestos was kept for some time red-hot in the flame of the alcohol, a glass tube being held over it: asbestos which had been prepared with chloride of platina remained vividly red-hot. Iron answers best for this experiment: asbestos moistened with a solution of sulphate of iron and then immersed in solution of ammonia, and washed in water and dried quickly, remains red like an iron wire. Asbestos moistened with a solution of gold and heated to redness, remains red-hot in the vapour of alcohol like a platina wire. The same occurs when asbestos is moistened with a solution of silver. It has been above stated that gold and silver wires possess the property of ignition but imperfectly; which shows that they are subject to soften readily. A wire prepared with chloride of tin and oxalate of ammonia, remains only a few seconds red, even under the glass tube, the tin being too suddenly converted into oxide. The case is nearly the same with lead, but it remains red rather longer. Cobalt and nickel possess the property of igniting like iron. Manganese and chromium remain red for a very short time. Arsenic is suddenly volatilized, the wire however remains red for a few seconds; the same occurs with mercury. Bismuth possesses this property of ignition in a rather less degree than iron. Cadmium resembles tin. Zinc is the only metal which does not at all ignite.

The author is of opinion that platinized asbestos may become an important substance for the condensation of several gases, as also for the preparation of vinegar.—*Journ. de Pharm. et de Ch.*, Nov. 1846.

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#### ON THE DISTILLATION OF MERCURY. BY M. M. E. MILLON.

When mercury is distilled, it is observable that the volatilization takes place more slowly at the period in which the last portions of

the metal pass into the receiver. If the mercury distilled at the commencement of the operation be kept separate from that obtained at the end of it, it is easily proved that these two portions are of unequal volatility.

Fifty kilogrammes of mercury were distilled, and the first and last kilogramme distilled were kept separate; both of them were re-distilled and then submitted to the following experiment: four small and similar retorts were selected, each of which when half-filled was capable of holding 100 grammes of mercury. Each of the retorts charged with 100 grammes of the metal was immersed into an alloy-bath in fusion; and the heat was raised till the mercury in all of them boiled. The mercury distilled condensed in the neck, and was collected and weighed. The four retorts did not yield equal quantities; but on putting aside those which differed the most, it was easy to select two, which, subjected to the above proof, furnished nearly an equal quantity of mercury. Thus in three parallel operations with these two retorts distinguished by the letters A and B—

In the same time, in the same bath.

Retort A. First operation.	Mercury distilled	48·5 grammes.	
Retort B. ....	....	47·5	..
Retort A. Second operation.	....	69·0	..
Retort B. ....	....	63·0	..
Retort A. Third operation.	....	66·0	..
Retort B. ....	....	64·0	..

It is to be remarked that in these three operations the retort A. always yielded a little more than the retort B. This circumstance was attended to in the experiments which follow.

The following are the differences obtained by submitting to comparative trial in the alloy-bath, mercury taken from the first and last kilogramme obtained from the distillation of 50 kilogrammes of the metal.

The retort A., which yielded the most, received the mercury collected at the close of the distillation; the retort B. that obtained at the commencement.

Mercury distilled during the same time in the same bath.

Retort A. First operation. Containing 100 grms. of metal	19·0 grms.	
Retort B. ....	....	49·0 ..
Retort A. Second operation.	....	15·7 ..
Retort B. ....	....	41·5 ..

Care must be taken, in these comparative distillations, to moderate the temperature of the alloy-bath as soon as the mercury begins to spot the dome of the retort.

Mercury thus affected in a marked manner in its mode of distillation would appear to possess some difference in its degree of purity; but the author attempted in vain to discover any difference by re-agents; the mercury of the first and last kilogramme was similarly affected in all the examinations to which it was submitted.

It then occurred to the author to try whether the addition of any foreign metal, in quantity so small as to escape analysis, could affect the volatility of the mercury. The results were of considerable in-

terest; the thousandth or even ten-thousandth of a foreign metal is sufficient to cause very characteristic differences in the results of parallel experiments.

A ten-thousandth of lead added to mercury stops its distillation almost entirely. In the following comparative experiments, the mercury which distilled with the greatest difficulty was put into the retort A., which yielded most quickly.

*First operation.*—Retort A., containing 100 grammes of mercury, to which 1-10,000dth of lead had been added, yielded in a given time by distillation in the bath 5 grammes of mercury.

Retort B., containing the same quantity of mercury without any lead, heated for the same time in the same bath, gave by distillation 67 grammes of mercury.

*Second operation.*—Retort A., containing 100 grammes of mercury, to which 1-1000dth of lead had been added, yielded in a given time by distillation in the bath 2 grammes of mercury.

Retort B., containing the same quantity of mercury without any lead, heated for the same time in the same bath, gave by distillation 55 grammes of mercury.

Zinc was substituted for lead, and when in the proportion of 1-10,000dth, the mercury distilled was to that obtained without the zinc as 6·5 grms. to 72.

Some metals have no sensible influence on the distillation of mercury: among these are gold, iridium, silver, copper, tin, nickel, cadmium and arsenic. The action of platina is precisely the reverse of that of lead and zinc: it accelerates distillation, but less than lead and zinc retard it; in order that platina may produce this effect, it must be digested in the mercury for a day or two at the temperature of 122° to 176° F.—*Ann. de Ch. et de Phys.*, Novembre 1846.

#### EASY METHOD OF PREPARING IODIDE OF POTASSIUM.

M. Pypers subjects to a moderate heat a mixture of 100 parts of iodine, 75 of carbonate of potash, 30 of iron filings and 120 of water. The mass is to be dried, and then heated to redness; the resulting reddish powder is to be treated with water, and the solution obtained filtered and evaporated to dryness. One hundred parts of iodine yield 135 of very white, but slightly alkaline iodide of potassium.—*Journ. de Chim. Méd.*, Decembre 1846.

#### FORMATION OF GLYCERINE. BY M. ROCHLEDER.

If castor-oil be dissolved in absolute alcohol and a current of dry hydrochloric acid gas be directed into the heated solution, the oil undergoes decomposition. When the liquor is agitated with water, after the hydrochloric gas has acted for a sufficient time, an emulsion is obtained, which gradually separates into two portions, one of which is oily and floats, the other is watery and extremely acid; the latter is to be poured off and evaporated by a water-bath. At first hydrochloric acid is disengaged, and there remains a syrupy and yellowish mass



which is to be treated with æther, which dissolves one portion and leaves another which is insoluble: this last is glycerine, and the solution contains the æthers of the fatty acids of the castor-oil.—*Journ. de Pharm. et de Ch.*, Decembre 1846.

#### ON THE COMPOSITION OF BOHEMIAN GLASS.

According to M. Peligot, the composition of the white Bohemian glass varies but very little in the different manufactories of that country. He has analysed various specimens which were perfectly pure and colourless, and they all yielded very nearly the same results. Their composition was found to be as follows:—

Silica.....	76
Potash .....	15
Lime .....	8
Alumina .....	1
	<hr/>
	100

The composition of the *verre-agate*, or *verre de pâte de riz* of Bohemia is remarkable; it is a simple silicate of potash, the semi-opacity of which is derived from imperfect vitrification, which has left grains of quartz, not fused, interposed in the mass. It contains, according to M. Peligot's analysis,—

Silica.....	80·9
Potash .....	17·6
Lime .....	0·7
Alumina and a trace of iron ....	0·8
	<hr/>
	100·0

This glass does not attract moisture from the air; when long boiled in water it is not acted upon. It differs from the soluble glass of Fuchs in containing about 10 per cent. more silica.

A specimen of Venetian artificial *avanturine* yielded—

Silica.....	67·7
Lime .....	8·9
Peroxide of iron .....	3·5
Oxide of tin .....	2·3
Oxide of lead .....	1·1
Metallic copper.....	3·9
Potash .....	5·5
Soda .....	7·1
	<hr/>
	100·0

M. Peligot found that a specimen of *verre à glaces soufflées* consisted of—

Silica.....	67·7
Potash .....	21·0
Lime .....	9·9
Alumina .....	1·4
	<hr/>
	100·0

*Journ. de Pharm. et de Ch.*, Novembre 1846.

## ON OXYPICRIC ACID.—STYPHNIC ACID.

In experimenting on Indian yellow, M. Erdmann discovered a new acid, to which he gave the name of oxypicric acid.

This substance has also been obtained by MM. Boettger and Will, without their being aware of the results of M. Erdmann's experiments. It was procured by these chemists by treating several gum-resins with nitric acid, as for example ammoniacum, assafoetida, &c.; they also procured it from the watery extract of Brazil wood, sanders and yellow wood, and they have called it styphnic acid, from *στυφνός*, *astringent*.

They prepare it by heating one part of assafoetida with from four to six parts of nitric acid of 1.20. At first they heat it to about 158° to 163° F.; as soon as the violent action which then takes place is moderated, it is to be boiled till all solid matter disappears. When the solution, on the addition of water, deposits a powder which is granular to the touch, the oxidation is finished; if the water separates a flocculent precipitate, the ebullition with the nitric acid must be continued: it is then to be evaporated to the consistence of a syrup, much water is to be added, and after having boiled it, carbonate of potash is to be added as long as effervescence ensues, taking care to add no excess, in order that the resinous portions which the liquor may still contain be not dissolved. After neutralization the liquor is to be filtered and evaporated, and set to crystallize. There then separates a salt of potash which is very slightly soluble, having the form of a brown crust, or of fine needles aggregated in mammillated forms. This salt is to be dissolved and re-crystallized in water, and the oxypicric acid is to be precipitated by nitric acid.

This acid is precipitated in the state of a white powder, or in scales having the form of fern leaves; it is to be washed with cold water and then dissolved in boiling absolute alcohol; from this it separates in prismatic crystals of considerable size.

The properties of this acid are that its taste is neither sour nor bitter, but slightly astringent; its solution, either in alcohol or water, colours the epidermis permanently.

It is yellow, reddens litmus strongly, and readily decomposes the alkaline carbonates. It requires 88 parts of water at about 144° F. for solution; alcohol and æther dissolve it readily. When cautiously heated on platina foil it fuses, and on cooling becomes a radiated mass: at a higher temperature it yields vapours which readily inflame. When suddenly heated it detonates slightly.

Nitric and hydrochloric acids even when concentrated do not act upon it at a boiling heat. It dissolves readily in concentrated nitric acid, and water precipitates it from solution in the form of a white powder.

Aqua regia decomposes it entirely with the production of oxalic acid. Concentrated sulphuric acid when heated also decomposes it. If a piece of very dry potassium be sprinkled with this acid, and it be slightly pressed with a pestle, the acid inflames: sodium does not produce the same effect.

A concentrated aqueous solution of oxypicric acid, especially when

heated, readily dissolves zinc and iron. Sulphuretted hydrogen has no action upon it; but an alcoholic solution of hydrosulphate of ammonia gives it a deep brown red colour. Assafœtida yields about 3 per cent. of this acid.

MM. Boettger and Will found, as M. Erdmann had done, that its composition is  $C^6 H^3 N^3 O^8$ , or  $C^6 (H^3 X^3) O^8$ . This acid is bibasic. It is capable of forming several salts with two different bases. All the oxypicrates, in common with the picrates, possess the property of exploding with considerable force when heated gently.—*Journ. de Pharm. et de Ch.*, Novembre 1846.

#### REDUCTION OF THE PERSALTS OF IRON BY ZINC.

Metallic zinc reduces the persalts of iron to the state of protosalts, and precipitates at the same time some metallic iron, often in the state of brilliant scales. According to M. Poumarède, it is equal to one-third of that which remains in solution, the reaction taking place according to the following equation ( $Fe\beta = \frac{2}{3}Fe$ ):



or



By superoxidizing the mixture of protosalts which results from a first reduction, a fresh quantity of iron may be precipitated; and by successive operations, the whole of the iron of a solution may be precipitated.

If weak solutions be operated upon, there is usually disengaged a certain quantity of hydrogen, which, according to the author, is derived from a secondary decomposition, operated on water by the iron set free; in fact, the zinc is always covered with a very slight coating of oxide of iron.

According to M. Poumarède, the solutions of nickel, manganese, and even of alumina, yield similar results.—*Journ. de Pharm. et de Ch.*, Novembre 1846.

#### AWARD OF MEDALS.—LINNÆAN SOCIETY.

A Special General Meeting of this Society was held on Friday the 8th of January, to consider the subject of the following Statement and Resolution of Council relative to the Bequest of the late Edward Rudge, Esq., F.L.S.

The Council, after much patient and anxious deliberation, had unanimously come to the following resolution:—

“Resolved,—That in the opinion of this Council, on a full consideration of the terms of the bequest of the late Edward Rudge, Esq., of the interest of a sum of £200, for the purpose of establishing a Medal to be awarded by the President and Council of the (Linnæan) Society, at their discretion, to the Fellow of the said Society who shall write the best communication in each volume which after his (the testator's) decease shall be published by the said Society, in either of the four departments of Natural History, it is inexpedient

to accede to the liberal intentions of the testator under the conditions expressed in his will."

This Resolution, which received the entire concurrence of the President and of every Member of the Council, was chiefly founded on the following considerations :—

The great object of the Linnæan Society, as of all other bodies similarly constituted, is the production and publication of such essays as tend to the advancement of that branch of science which it cultivates. The principal question therefore in reference to Mr. Rudge's bequest, is the manner in which its acceptance would operate on the Society's publications, and the Council has arrived at the conclusion that its tendency would be prejudicial rather than favourable; inasmuch as while the Medal would offer no inducement to some of those Members who have hitherto been in the habit of communicating papers which have had a place in the 'Transactions,' they might, on the contrary, be unwilling to submit their future communications to this new ordeal; and it does not appear probable that the Medal would prove a stimulus to the production of more valuable Essays from any other class of the Society. On the other hand, it is probable that dissatisfaction would arise in the minds of some of those Members, who after contributing papers to more than one volume of the 'Transactions,' should fail in obtaining the award of a Medal.

A second objection to the acceptance of the bequest arises from the absence of any discretionary power of withholding the Medal, which is necessarily to be awarded to the best paper in every volume, and consequently to papers of very unequal value, thereby lowering the character of the Medal, and consequently affecting the scientific reputation of the Society itself.

Differences of opinion, and consequent dissatisfaction, would also be not unlikely occasionally to arise in deciding upon the comparative merits of papers in botany and zoology, the two branches of natural history, of which, for many years past, the Transactions of the Society have exclusively consisted.

Another point may still be noticed as decidedly unfavourable to the acceptance of the bequest, namely, the not improbable award of the Medal by the Council, in some cases to one of its own body, in strict conformity with the conditions of the will; conditions which neither the Council itself, nor (as it appears from the tenor and provisions of the will) any other party has the power to modify.

These objections have appeared to the Council so important as not to admit of any other course but that of respectfully declining to accept a bequest, the operation of which would in all probability be injurious to the best interests of the Society, by lowering the character of its publications, and endangering the continuance of that harmony which has hitherto prevailed in all essential points. The Council is at the same time deeply sensible of the kind and liberal intentions of Mr. Rudge, and entertains a sincere regret that the express terms of his will should have rendered the acceptance of his bequest liable to such grave objections.

The meeting was numerously attended, and the President (the



Bishop of Norwich) having read from the chair the above Statement on the part of the Council, some of the Fellows who had been Members of the Councils of the Royal and Geological Societies stated their opinion of the inconvenience and injurious tendency of the awarding of medals in those Societies, and their inutility for the promotion of science; after which the Resolution proposed by the Council was unanimously approved and adopted.

METEOROLOGICAL OBSERVATIONS FOR DEC. 1846.

*Chiswick.*—December 1, 2. Foggy. 3. Cloudy: frosty. 4. Sharp frost: fine. 5. Cloudy: clear and fine: overcast. 6. Clear: cloudy. 7. Fine: cloudy. 8. Cloudy: fine. 9. Slightly overcast: drizzly: slight rain. 10. Rain. 11. Frosty: snowing: clear and frosty. 12. Frosty: cloudy: clear and frosty. 13. Frosty: cloudy: severe frost at night. 14. Severe frost: clear and frosty throughout. 15. Severe frost: densely overcast: clear and frosty. 16. Sharp frost: clear and cold: frosty. 17. Densely overcast: fine: slight snow. 18. Sharp frost: clear: overcast. 19. Rain: foggy. 20. Rain: cloudy. 21. Rain: clear and frosty at night. 22. Slight frost and fog: fine: clear. 23. Rain. 24. Foggy. 25. Frosty: clear. 26. Clear and frosty. 27. Frosty: cloudy: clear and frosty. 28. Frosty and foggy. 29. Slight frost: overcast. 30. Densely overcast: frost at night. 31. Sharp frost: foggy.

Mean temperature of the month .....	31°·26
Mean temperature of Dec. 1845 .....	40 ·41
Average temperature of Dec. for the last twenty years .....	40 ·04
Average amount of rain in Dec. ....	1·58 inch.

*Boston.*—Dec. 1. Fine: snow on the ground. 2. Cloudy: snow on the ground. 3. Fine: snow on the ground. 4. Cloudy: snow on the ground. 5. Fine: snow on the ground: rain P.M. 6. Fine. 7. Rain. 8. Fine: rain P.M. 9. Cloudy: rain P.M. 10. Fine. 11, 12. Snow: snow on the ground. 13—16. Cloudy: snow on the ground. 17. Cloudy: snow early A.M. 18\*, 19. Cloudy: snow on the ground. 20. Fine: snow on the ground. 21. Cloudy: snow on the ground. 22. Fine: snow nearly all gone: melted snow. 23. Cloudy. 24. Snow: snow on the ground. 25, 26. Fine: snow on the ground. 27—30. Cloudy: snow on the ground. 31. Cloudy: snow on the ground: melted snow.

*Sandwich Manse, Orkney.*—Dec. 1. Showers: clear. 2. Bright: sleet-showers. 3. Hail-showers: sleet-showers. 4. Bright: showers. 5. Showers: sleet-showers. 6. Sleet-showers: cloudy. 7. Rain: clear. 8. Drizzle: cloudy. 9. Drizzle: shower: clear: aurora. 10, 11. Snow-showers: snow-drift. 12, 13. Snow-drift: snow-showers. 14. Snow-showers. 15. Snow-showers: snow-drift. 16. Snow-showers. 17. Snow-showers: snow: clear. 18. Thaw: quick thaw. 19. Frost: showers. 20. Bright: clear: hoar-frost. 21. Rain: showers. 22. Hail-showers: frosty. 23. Hail-showers: clear: aurora. 24. Hail-showers: cloudy. 25. Clear: cloudy. 26. Bright: cloudy. 27. Bright: rain. 28. Bright: drizzle. 29. Drizzle: clear. 30. Fine: clear: halo. 31. Drizzle.

*Applegarth Manse, Dumfries-shire.*—Dec. 1. Thaw. 2—4. Hard frost. 5. Wet. 6. Frost: clear. 7. Frost. 8. Frost, but dull. 9. Frost, slight. 10. Thaw: slight snow. 11—14. Keen frost. 15. Keen frost: slight snow. 16. Keen frost: sleet. 17. Keen frost: slight snow. 18. Frost A.M.: rain P.M. 19—21. Wet. 22. Frost, keen. 23. Frost, keen: slight snow. 24. Frost, keen: more snow. 25, 26. Frost, keen. 27. Thick fog: frost. 28. Thaw: fog: rain. 29. Thaw: thick fog. 30, 31. Thaw: fog.

Mean temperature of the month .....	33°·5
Mean temperature of Dec. 1845 .....	39 ·5
Mean temperature of Dec. for twenty-three years .....	38 ·3
Mean rain in Dec. for eighteen years .....	3 inches.

\* Not so cold a day in December since 28th December 1829, which was 16°·5. Not so cold a month of December for twenty years at least: December 1829 was very cold—average 33°·1.

Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at Chiswick, near London; by Mr. Veall, at Boston; by the Rev. W. Dunbar, at Applegarth Manse, Dumfries-shire; and by the Rev. C. Clouston, at Sandwick Manse, Orkney.

Days of Month.	Barometer.						Thermometer.						Wind.				Rain.					
	Chiswick.			Dumfries-shire.			Orkney, Sandwick.		Chiswick.		Dumfries-shire.		Orkney, Sandwick.		Boston.		Dumfries-shire.		Orkney, Sandwick.			
	Max.	Min.	84 a.m.	9 a.m.	9 p.m.	84 p.m.	Max.	Min.	84 a.m.	Max.	Min.	9 a.m.	84 p.m.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.	
1846. Dec.																						
1.	29.900	29.655	29.60	29.54	29.48	29.39	29.47	25	27	41	27½	40½	38	sw.	calm	calm	nw.	nw.	.....	.....	.....	.....
2.	29.428	29.361	29.27	29.40	29.40	29.40	29.38	29	26	33	23½	37	37	nw.	calm	calm	nnw.	nnw.	.....	.....	.....	.....
3.	29.716	29.564	29.40	29.50	29.62	29.56	29.78	17	27	39½	20	40	36	n.	calm	calm	n.	n.	.....	.....	.....	.....
4.	29.878	29.788	29.57	29.78	29.86	29.90	29.74	25	34	37½	29	38½	42	n.	n.	calm	sw.	sw.	.....	.....	.....	.....
5.	30.003	29.821	29.74	29.67	29.50	29.34	29.43	29	28.5	43	23	44	38	w.	calm	calm	nw.	nw.	.....	.....	.....	.....
6.	29.811	29.732	29.52	29.65	29.85	29.72	29.97	30	32	40½	32	36	39	nw.	calm	calm	nnw.	nnw.	.....	.....	.....	.....
7.	30.127	29.904	29.70	30.00	30.10	30.04	30.11	33	35	40½	34	43	48	n.	calm	calm	wnw.	wnw.	.....	.....	.....	.....
8.	30.231	30.187	30.00	30.20	30.20	30.22	30.21	42	35	39	31	48½	49	ne.	calm	calm	w.	w.	.....	.....	.....	.....
9.	30.161	30.084	29.92	30.12	29.98	30.10	29.82	43	41	42	36½	48½	47	ne.	calm	calm	w.	w.	.....	.....	.....	.....
10.	29.904	29.651	29.56	29.63	29.65	29.80	29.72	44	40	45½	35½	30	32	sw.	calm	calm	n.	n.	.....	.....	.....	.....
11.	29.641	29.591	29.33	29.59	29.57	29.67	29.77	19	30	32	25	33	32	nw.	n.	calm	n.	n.	.....	.....	.....	.....
12.	29.741	29.534	29.46	29.67	29.70	29.89	29.93	24	32	34	29	33	30½	ne.	calm	calm	n.	n.	.....	.....	.....	.....
13.	29.759	29.697	29.54	29.70	29.60	29.80	29.66	30	27	33	24	32	32	ne.	calm	calm	n.	n.	.....	.....	.....	.....
14.	29.610	29.486	29.36	29.44	29.37	29.58	29.56	30	20	33	18	30	31	w.	calm	calm	nne.	n.	.....	.....	.....	.....
15.	29.571	29.425	29.22	29.40	29.54	29.66	29.70	13	25.5	33½	24	32	33½	nw.	nw.	calm	n.	n.	.....	.....	.....	.....
16.	29.749	29.678	29.46	29.60	29.50	29.68	29.65	35	29	36½	27½	36½	35	nw.	calm	calm	n.	n.	.....	.....	.....	.....
17.	29.739	29.590	29.44	29.55	29.88	29.79	29.97	36	28	35½	25	32½	29	nw.	calm	calm	n.	n.	.....	.....	.....	.....
18.	30.166	30.119	29.94	29.91	29.63	29.66	29.55	33	18	40	19	39	41	sw.	calm	calm	.....	.....	.....	.....	.....	.....
19.	29.851	29.814	29.54	29.56	29.51	29.59	29.40	44	37.5	45½	41	35	46	sw.	calm	calm	nw.	nw.	.....	.....	.....	.....
20.	29.765	29.655	29.43	29.30	29.26	29.20	29.14	48	44	45	41	39½	36	sw.	calm	calm	w.	w.	.....	.....	.....	.....
21.	29.190	29.004	28.86	28.83	28.83	29.02	29.03	49	46	43	40	39	35½	sw.	w.	w.	ne.	ne.	.....	.....	.....	.....
22.	29.125	29.011	28.76	28.93	28.89	29.15	29.16	26	35	35	27	33	33½	w.	calm	calm	e.	e.	.....	.....	.....	.....
23.	29.024	28.620	28.60	28.80	28.98	29.23	29.40	39	36	38	31	34	31½	ne.	calm	calm	ene.	ene.	.....	.....	.....	.....
24.	29.332	29.159	29.04	29.22	29.38	29.42	29.52	35	34	37½	29½	33	31	e.	calm	calm	nw.	nw.	.....	.....	.....	.....
25.	30.037	29.593	29.40	29.63	29.93	29.76	29.80	34	23	32½	23½	33	43	n.	calm	calm	nnw.	nnw.	.....	.....	.....	.....
26.	30.249	30.228	30.00	30.10	30.22	30.10	30.23	21	26	41½	24	40	43	nw.	calm	calm	w.	w.	.....	.....	.....	.....
27.	30.494	30.396	30.20	30.26	30.24	30.26	30.08	38	30	35½	21½	38	40	n.	calm	calm	sw.	sw.	.....	.....	.....	.....
28.	30.499	30.432	30.26	30.20	30.22	30.12	30.15	34	25.5	40	32½	44	44	w.	calm	calm	ssw.	ssw.	.....	.....	.....	.....
29.	30.505	30.444	30.20	30.19	30.25	30.04	30.21	40	32	41	35½	45	45	s.	calm	calm	ws.	ws.	.....	.....	.....	.....
30.	30.573	30.552	30.32	30.30	30.32	30.24	30.30	32	31	39½	34½	42	45½	s.	calm	calm	ene.	ene.	.....	.....	.....	.....
31.	30.540	30.434	30.32	30.35	30.34	30.37	30.40	28	32	40½	36	46½	46½	s.	calm	calm	ene.	ene.	.....	.....	.....	.....
Mean.	29.881	29.744	29.58	29.678	29.703	29.732	29.749	24.68	31.2	38.5	29.0	37.93	38.40						1.21	2.00	0.90	3.97

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[THIRD SERIES.]

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XXVIII. *Reply to the Astronomer Royal on the New Analysis of Solar Light.* By SIR DAVID BREWSTER, K.H., D.C.L., F.R.S. and V.P.R.S. Edin.\*

THE remarks of Mr. Airy in the last Number of this Journal, on my Analysis of the Solar Spectrum, have, I doubt not, given as much surprise to its scientific readers as they have given to me, and this independently of the correctness or incorrectness of the results to which they refer. When a philosopher examines, and pronounces an opinion upon the researches of others, especially upon those which competent judges have recognised as sound, he is bound to repeat the identical experiments which he challenges, with similar apparatus and similar materials; to state the differences which he observes, to inquire into the causes by which such discrepancies have arisen, to establish his own views by new and effective experiments, and to publish his researches in vindication of his charges against a fellow-labourer in science. Mr. Airy, however, has not done this; but, as we shall presently see, has followed a course which is as unusual in the history of science as it may be injurious to its progress.

The question (the only one as Mr. Airy says) to which these observations bear reference is this: "*Is it established that the colour of any portion of the spectrum is changed by the use of any absorbing medium?*" To this question I and others have given the answer, that *absorbing media do change the colour of different portions of the spectrum.* I give the same answer still; and rather than believe that I have been blind for the last *fifty* years, I will hazard the supposition that the Astronomer Royal cannot distinguish colours, and is a genuine specimen of an *idiopt*, the name which his friend Dr. Whewell

\* Communicated by the Author.

has invented to designate the unfortunate members of the colour-blind community.

But before I proceed to give a notice of the analysis of the spectrum by absorption, I must refer to the *mode* of making the experiments. Mr. Airy says that "*the eye has no memory for colours,*" and that it is therefore necessary to compare *immediately* the *modified* with the *unmodified* spectrum. It may be true that *the eye* has no memory of any kind, and therefore not for colours; but I know that I *have a memory* for colours, and that the colours are so much committed to my memory by fifty years' schooling, that without any immediate comparison I can tell whether or not the *green* space has become *yellow*, and whether or not, by certain combinations of absorbing media, I see before me a portion of *white* light *standing* close to a portion of *red* light in the spectrum, like an *almond* and a *cherry* stuck together.

But, apart from this consideration, I beg to remind Mr. Airy that I was the first person to use the methods which he considers his of placing the *modified* beside the *unmodified* spectrum, by employing the same slit for both; and that I showed it to him *fifteen* years ago, when he did me the honour of paying me a visit in Roxburghshire. In my paper On the Lines of the Spectrum, read to the Royal Society in 1833\*, I write thus, when speaking of the coincidence of the Nitrous gas lines with those of Fraunhofer:—

"In order to afford ocular demonstration of this fact, I formed the solar and the gaseous spectrum *with light passing through the same aperture*, so that the lines in the one stood opposite those on the other, like the divisions on the vernier and the limb of a circle, and their coincidence or non-coincidence became a matter of simple observation. I then superimposed the two spectra when they were both formed by solar light, and thus exhibited at once the two series of lines, with all their coincidences and all their apparent deviations from it. Professor Airy, to whom I showed this experiment, remarked that he saw the one set of lines through the other, which is an accurate description of a phenomenon perhaps one of the most splendid in physical optics, whether we consider it as appealing to the eye or to the judgement."

On the 15th of April 1822, I laid before the Royal Society of Edinburgh a paper, in which I describe specific experiments, *proving* that the colour of parts of the spectrum is changed by absorbing media, and that a *distinct yellow band* can be insulated on the most refrangible side of the line D†. About the same time Sir John Herschel addressed a letter to me On the Absorption of Light by Coloured Media, &c.,‡

\* Edinburgh Transactions, vol. xii. p. 525.

† Ibid. vol. ix. p. 433-444.

‡ Ibid. pp. 445-460.



containing experiments from which he drew the same conclusion that I did, as appears from that letter, and from the following passages in his Treatise on Light. Viewing the spectrum through a piece of smalt blue glass 0·042 of an inch thick, he makes the following observation:—

"This RED (the innermost of the two red bands)\*\* is free from the *slightest shade of ORANGE*: its most refracted limit came very nearly up to the dark line D in the spectrum. A *small sharp* black line separated this RED from the YELLOW, which was a pretty well-defined band of GREAT BRILLIANCY AND PURITY OF COLOUR, of a breadth exceeding that of the first RED, and bounded on the GREEN side by an obscure but not quite black interval."—Art. 496.

Upon this distinct and well-described observation, which is precisely what I have witnessed a hundred times, Sir John reasons as follows:—

"The two reds noticed in Art. 497 have absolutely the same colour, and cannot be distinguished. On the other hand, the *transition from pure red to pure yellow*, in the case there described, is quite sudden, and the contrast of colours most striking. \* \* \* \* What then, we may ask, has become of the ORANGE; and how is it, that its place is partly supplied by RED on one side and YELLOW on the other? These phænomena certainly lead us very strongly to believe that the analysis of white light by the prism is not the only analysis of which it admits. \* \* \* \* This idea has been advocated by Dr. Brewster in a paper published in the Edinburgh Transactions, vol. ix., and the same consequence appears to follow from other experiments\* published in the same volume of that collection."—Art. 506.

Having had occasion in the course of the ten years subsequent to 1822 to view the spectrum through, literally, hundreds of solid, fluid, and gaseous bodies in different combinations, I was led to the generalisations which I have described in a paper On a New Analysis of Solar Light, read to the Royal Society of Edinburgh on the 21st of March 1831†. I endeavoured to prove by specific experiments that the solar spectrum was compound, and capable of analysis by absorbing media, and I succeeded in insulating a portion of *white light*, which the prism could not decompose. But as these points are not now at issue between the Astronomer Royal and me, I shall make no further allusion to them.

Nearly three years after the reading of this paper, on the 6th of January 1834, the President and Council of the Royal Society made the following announcement:—

"The President announced that the Council had resolved to award the Keith Biennial Prize, for the second period, to Sir David Brewster, for his paper On a New Analysis of Solar Light."

Being then resident in the country, I had no knowledge whatever of the intention of the Royal Society to do me this

\* Edinburgh Transactions, § 5. p. 449.

† Ibid. vol. xii. p. 123.

honour. No doubts were expressed to me about the accuracy of my experiments, and no explanations asked; and being accidentally in Edinburgh on the 6th of January, the reading of the announcement at the meeting of the Royal Society that evening was the principal intimation I received of the adjudication of the prize. Prof. Forbes, the Secretary to the Physical Class, was then the representative of physical science in the Council. I knew that he had made many experiments on the action of absorbent media on the solar spectrum, and I have no doubt that he observed the great fundamental fact described by Sir John Herschel and myself, which, if a truth, settles the question between Mr. Airy and me.

It would appear that in the year 1833 Mr. Airy made experiments on the spectrum. He could not see any change of colour in the spectrum produced by absorbent media, and he mentioned this result ORALLY, and from recollection, at a meeting of the Cambridge Philosophical Society. He did not consider his experiments worthy of being preserved, for he did not even copy them from his pencilled notes: he did not think them deserving of publication; and yet Dr. Whewell, the Historian of science, takes them up *four* years afterwards, and setting aside Sir John Herschel's experiments, which he may not have known, and mine which he did know, and holding cheap the decision of the Royal Society, of which he probably was also ignorant, he adduces the experiments of Mr. Airy, without mentioning his name, as hostile to mine, by stating that my facts were "denied by *other experimenters*."

This gratuitous challenge of the accuracy of my experiments, on anonymous authority, was animadverted upon in one of the reviews of Dr. Whewell's work; and upon publishing a second edition of it, he naturally applies to his anonymous experimenter, who turns out to be Mr. Airy. Without making a single new experiment, without even having recourse to his pencil notes, which he seems to have lost, the Astronomer Royal authorizes the Master of Trinity to refer again to his experiments as invalidating my analysis of solar light. Dr. Whewell has of course done this; but he has done it in a note so exceptionable in its argument, and to me so unintelligible in its interrogatories, that I am most unwilling to criticise it. He asks me, for example, if it is meant, that is, if I mean, that Newton's experiments *prove nothing*? Who ever said that they proved nothing? Newton's *prismatic* analysis of the spectrum is, in my opinion, one of the greatest of his discoveries. "Or," he adds, "is Newton's *conclusion* allowed to be true of light which has not been analysed by absorption?" To this I reply, that Newton's conclusions respecting the apparent

division of the spectrum into seven colours, and their difference of refrangibility, is not in the slightest degree affected by the results of my analysis. "And where," he continues, "are we to find such light since the atmosphere absorbs?" I again answer, that Newton's prismatic analysis must, of course, be the analysis of light after it has experienced all the actions which it has undergone in passing through the atmospheres of the sun and the earth; but if I misapprehend the object of the question, it may be otherwise answered by stating that light which has not suffered absorption, and which does not want a single definite ray, may be found in the white artificial flames with which we are all familiar. I have distinctly stated in my original memoir, what Dr. Whewell seems not to have read, that Newton's analysis of light by the prism is perfect, so far as it goes; but Newton committed a mistake, if mistake is the proper term, when he asserted that to the same refrangibility always belongs the same colour.

Having authorized Dr. Whewell to refer again to his experiments on absorption, the Astronomer Royal has thought it necessary, in order to justify the reference, to publish his *recollections* of these experiments. This is the first time in the annals of science that the *recollections* of experiments have been given to the world. A philosopher in his decline, when his failing sight and his trembling hand are no longer fit for the delicate operations of experimental inquiry, may be excused for calling up the recollections of his manhood in support or in refutation of some exciting speculation; but no apology can be made for those who, with the means and the leisure for repeating their experiments, bring forward their recollections to discredit or to overturn the researches of others who have laboured patiently and successfully in the same field of scientific research.

In the present case these *recollections* have a still more peculiar character: they are admitted by their author to be "*only negative.*" "Of the results of these experiments," says the Astronomer Royal, "*I can give little more than the single negative one,—that no change was produced in the qualities of the colours.*" These *recollections* have still another peculiarity. Relating as they do to colours, they are the recollections of a person who confesses that he has no memory for colours, and so imperfect are they that he forgets the names of the miscellaneous contents of a chemist's shop through which he viewed the spectrum, and he remembers only the "port and porter" of his own cellar. Mr. Airy cannot even tell us what he actually saw when he absorbed the sun's rays by smalt-blue glass, port and porter. He does not describe the spectrum thus

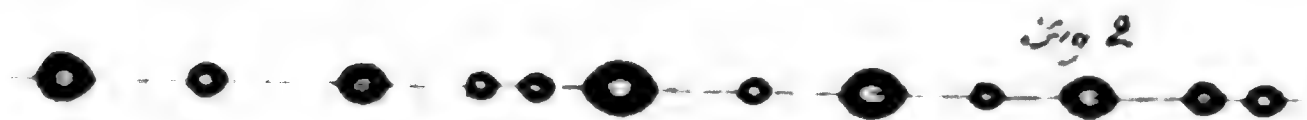
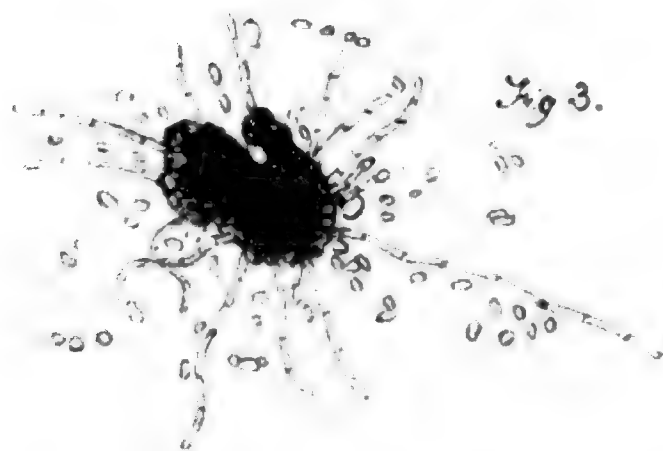
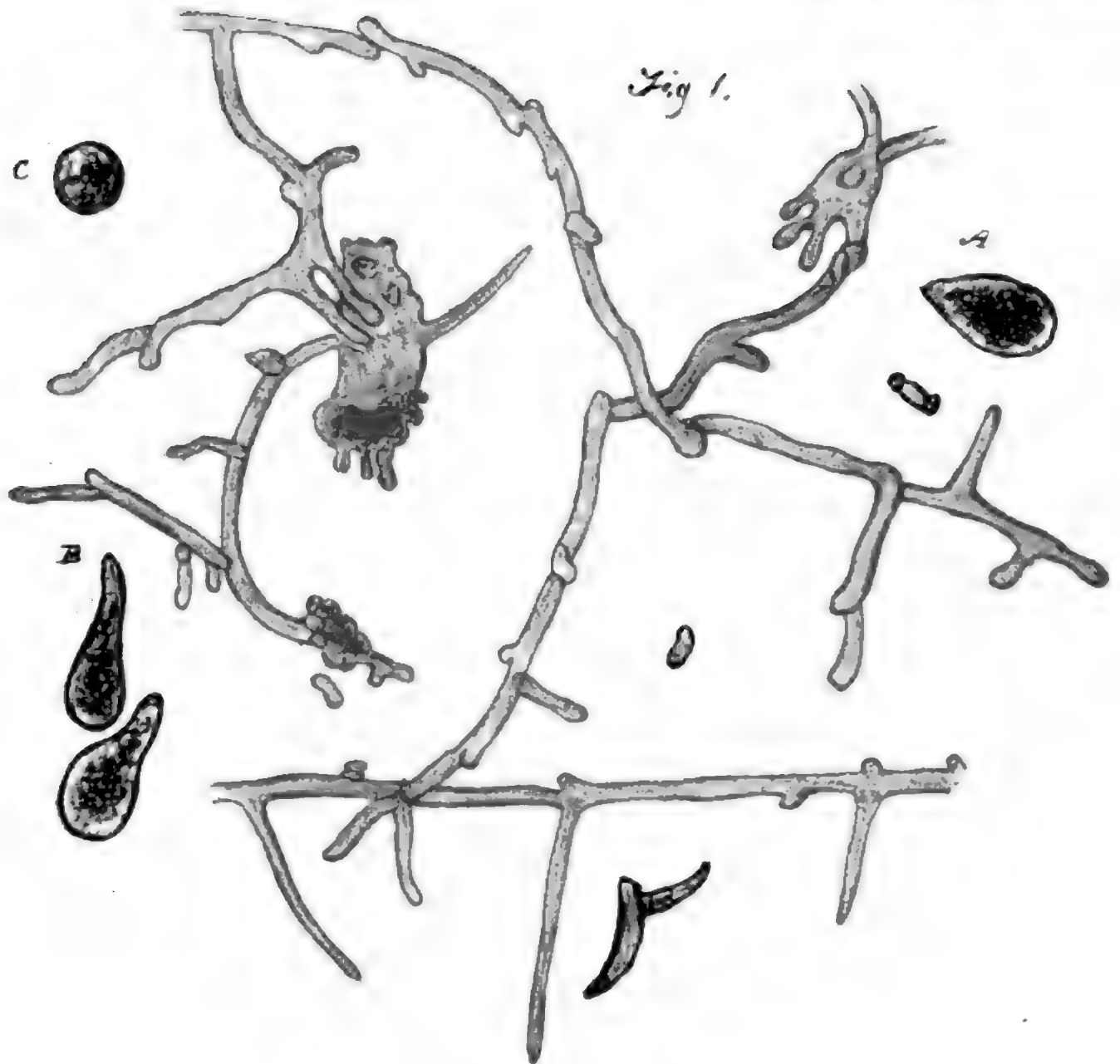
produced; but merely tells us that the qualities of the colours were unchanged. Sir John Herschel, as we have seen, made the experiment with the same glass carefully, and describes the results of it minutely; and I have done the same; and both of us have found *that the qualities of the colours were decidedly changed*. But Mr. Airy may perhaps have made the experiment more skilfully than we did; and he takes credit for comparing the modified with the unmodified spectrum, and with excluding all extraneous light,—precautions which others have taken far more carefully and effectually than he did. With all these precautions, however, he neglected the most important. He received his spectrum upon a *paper screen*! and while he was viewing it, his retina was influenced by all the various colours which shone in his modified and unmodified spectrum. In the experiments of Sir John Herschel, I believe, but certainly in my own, not a ray of light entered the room but what passed through the narrow slit; the *retina was our screen*, and the absorbing medium was held close to the eye and immediately behind the prism, the *only method* of obtaining the purest spectrum from a given prism. I have now before me a modified and an unmodified spectrum. I can compare any portion of the one with any portion of the other, or I can examine it alone; and so completely are the colours changed with the combination of absorbents which I employ, that I should regard the person as colour-blind who does not see the change. In the *striped* spectrum which I have described in my paper, the phænomena are still more beautiful and instructive.

But it is not merely by absorbent substances that the qualities of the spectral colours may be changed. I have obtained, during a series of unpublished experiments, similar effects from the interference of pencils transmitted through perfectly colourless media, and prismatically analysed.

What, then, is the result of all this discussion? Sir John Herschel and I have minutely described certain experiments, in which we have proved to our own satisfaction and that of many others, that absorbing media change the colours of portions of the spectrum. Mr. Airy, by inferior methods as I think, but by superior methods as he thinks, recollects having, fourteen years ago, found that these colours were not changed! The Master of Trinity records the last of these results as undoubted scientific truth, and strives to transmit it to the latest posterity.

St. Leonard's College, St. Andrews,  
February 5, 1847.





XXIX. *Additional Observations on Hail, and on the Organic Bodies contained in Hailstones, &c.* By AUGUSTUS WALKER, M.D.\*

[With a Plate.]

**I**N a preceding Number of this Journal (August 1846) I have stated the results of a microscopic observation of hailstones previous to their fusion, and likewise of the water resulting from them. It was then found that this water contained particles of different shapes, which from their nucleated structure, globular form, and general appearance, were evidently of an organized nature. Since making these observations I have had an opportunity of repeating them, which occurred on the 1st of August, when London and the country in general were visited by that tremendous storm which has been the occasion of so much damage.

*Aug. 1.*—In my neighbourhood (Kensington) there occurred several falls of rain, which continued, with a few intervals, from about 4 P.M. to late in the evening: the fall of water was accompanied with frequent peals of thunder and flashes of lightning. With the drops of water particles of hail were occasionally found, which rarely reached the size of a bean. No damage was done to the hot-houses in this locality. One of these hailstones, perfectly white, when examined under the microscope in the ordinary way, presented the usual appearance of being composed of minute particles of ice, some spherical, others of a more irregular shape, and numerous bubbles of air escaped as it melted. The organized substances contained in the drop of water which resulted were numerous. Most of them were of an irregular shape with angular outlines. Others were globular; and some of these were completely black, with a nucleus in the centre. With higher magnifying powers were seen green globules about  $\frac{1}{5000}$ th of an inch in diameter, which were either collected in clusters or adherent together in single lines, like the beads of a rosary, as represented in fig. 1 of my former paper on hail.

In order to preserve the drop of water for a leisure examination free from dust in the atmosphere, I placed it in a little apparatus nearly air-tight, which will be described hereafter.

*Aug. 2.*—The drop of water thus secured and left under the microscope was found unaltered in size, but the organized matter had entirely altered its appearance. Some of the organized particles had given off filaments in various directions, as represented in Plate I.†, presenting the ap-

\* Communicated by the Author.

† The magnifying powers under which the objects are represented in this plate are as follows:—Fig. 1. magnified 360 diameters, fig. 2, 200 diameters, and fig. 3, 300 diameters.

pearance of hollow tubes, jointed at intervals, with a few branches or subdivisions generally arising at the joints. The particles which gave rise to this vegetation appeared to me to belong to those which I have before described as the irregular angular particles. Among these one or two infusoria were likewise seen, moving about with great rapidity. When at rest their form was oval (A), but when in motion their anterior extremity frequently became elongated, as at B. Their organization was extremely simple; no filaments nor appendages of any kind were to be detected about them. Their interior had a granulated appearance. All these characters indicate that they belong to the genus *Uvella* of the Monadinæ, and probably of the species *Uvella glaucoma* (Ehrenberg). C represents one perfectly circular, which was in a state of constant motion, revolving round its centre.

*Aug. 3.*—The vegetable filaments had greatly augmented in number. Small elongated cells, as represented, could be seen at various places, as if forming the first elements of the tubulous branches spread out around them. The infusoria had likewise greatly increased in number.

*Aug. 4.*—The same appearances were presented as the day before. Scarcely any alteration could be detected in the vegetable and animal bodies: the last were as active in their movements as before. I purposed to continue these observations every day, in order to ascertain their future changes; but unfortunately by some accident one of the slips of glass belonging to the apparatus was broken, and in consequence the water quickly dried up, so that when next examined the infusoria were found to be dead. The further addition of water was ineffectual to restore the vegetable particles to life.

For the purpose of repeating these observations, and likewise for many others of a microscopic nature, the little apparatus which I have made use of will be found very serviceable. It consists of a small square piece of sheet caoutchouc, more or less thick according as it may be required, with an aperture cut in the centre about the size of a shilling. On each side of it are placed two slips of glass of the same size, which, by a moderate degree of pressure, may generally be made to adhere to it so strongly as to render the little air-chamber, where the india-rubber has been cut away, perfectly secure from all access of dust and nearly air-tight. When the glass does not easily adhere to the caoutchouc, this last may be rendered sufficiently glutinous by heating it moderately. Instead of glass the plates may be of thin mica, which adheres still better; and from being so much more thin, will allow of the employment of the highest magnifying powers without the possibility of the access of foreign matter. I have sometimes

used a metallic framework with a screw, by means of which the whole is rendered perfectly air-tight, and the glasses may be pressed nearly into immediate contact, but in most cases this is superfluous. As an instance of its efficiency in preventing all evaporation, I may state that I have preserved vaccine lymph in a fluid state in this manner during more than two months, with merely the assistance of a degree of pressure on the glasses, in the first instance secured by a few turns of wire. This is decidedly the most effectual mode of preserving this substance fit for use at any moment. A few of the same kind of apparatus in the pocket-book will be found most useful companions to the microscopic observer, when the slips of glass in ordinary use are inapplicable.

Although the existence of organized particles in the upper regions of the atmosphere appears at first very improbable, we shall find that several facts exist which much decrease this apparent improbability, and even some that appear to corroborate this statement. M. Boussingault has found that organic matter may invariably be detected in the atmosphere. It is true that his experiments have been confined to the lower atmospheric strata, and it would be very desirable to ascertain whether they hold good for those which are several thousand feet above the sea. The same may be detected in rain-water; and what is still more to the point, I find that M. Girardin (*Académie des Sciences*, April 1834) has detected in the water resulting from the fusion of hailstones, "a strongly azotized organic matter, and also lime and sulphuric acid without any trace of ammonia being present." The frequent vegetations in snow show at what a low temperature the vital principle may be sustained. The red and green appearance assumed by it has been found to be produced by the *Hæmatococcus nivalis* and the *Protococcus viridis*, as these plants have been termed, and which are probably the same species at different periods of growth, which have been found to penetrate many inches below the surface of the snow. These vegetations are found under the microscope to consist of minute globules, some green and transparent, about the size of  $0^{\text{mm}}\cdot01$  to  $0^{\text{mm}}\cdot05$ ; others red, generally larger than the former, and frequently arranged in single rows, like the beads of a necklace, adherent together. The globules are found of all intermediate tints between the green and the red, and likewise others of larger dimensions of a red colour, about  $0^{\text{mm}}\cdot05$  to the  $0^{\text{mm}}\cdot055$ , which have a more compound structure, as within them are seen several granules or smaller globules, which was not the case in the two former. M. Martens states, that in some of these globules microscopic infusoria may be detected (see



Kæmtz's Meteorology). I am not aware that the origin of this vegetation in snow has ever been satisfactorily accounted for; but when we consider how widely this vegetation is diffused, and the depth to which it penetrates, it does not appear unlikely that it may have fallen with the snow, in the same manner as the organic bodies in hail are brought from the upper regions of the atmosphere. Further observations must determine how far these surmises are correct, and whether these bodies are not the same in both cases.

Again, in the hands of Ehrenberg the microscope has been the means of detecting, in dust fallen on ships in the Atlantic, the remains of above sixty different species of infusoria. Mr. Darwin, in his interesting paper in the Transactions of the Geological Society, mentions that dust has been collected in this manner at a point midway between Senegal and Cayenne. The distance to which volcanic ashes may be conveyed by the higher atmospheric currents, in a direction contrary to that of the lower prevailing winds, is well known. These facts being duly weighed, the frequent and even habitual existence of minute cryptogamic sporules and infusorian ovules in the higher regions of the atmosphere, where hail is formed, will not appear more incredible than the continued suspension of globules of water composing clouds; which globules, as measured by diffraction, are found of far greater size than the former. The relation between the presence of these particles and other phænomena of meteorology remains to be determined, and how far they may be regarded as accompanying and detector agents of other atmospheric conditions following or preceding them; and whether the infusoria and the cryptogamia are the same at all seasons of the year; whether the locality in which they are collected has any influence; and lastly, whether they are the same in hail and water falling at about the same time.

In meteorology water likewise presents itself in what is termed the vesicular, as in clouds and fogs, in the condition of rain, snow and frost. In a paper which was read before the Royal Society, and which will appear in the Philosophical Transactions, 1847, will be found some experiments of mine upon water in the first-mentioned condition; with regard to the others I shall confine myself to the following remarks.

The transformation of fogs into rain is frequently taking place around us. Fogs, which at first are what is termed dry fogs, scarcely possessing the power of moistening surrounding objects, after a time are found to deposit numerous small globules of water, which as long as they remain attached to minute filaments retain their spherical form, but when in con-

tact with other bodies moisten them like common rain. This may be regarded as similar to the formation of rain in clouds, except that it is on a much smaller scale, as the thickness of the stratum of the fog is much less than the strata of clouds, as measured by M. Peytier.

We may examine the minute particles of rain or fogs, as I have already shown, by passing the air which contains them through essence of turpentine, lowered a few degrees below the common temperature. They are then left behind in the liquid; but at the same time they are mixed with others which arose from the condensation of the water contained in the air in an elastic condition. Difficulties exist with regard to the measurement of the globules while floating about. This is an objection to the employment of this method of fixing the globules. I have therefore endeavoured to use another means of observing these particles, and for this purpose have examined them as they exist on the filaments which compose the spider's web.

In order to examine these globules *in situ*, it will be found convenient to make use of two square frames of sheet cork of equal size, about two or three inches square. These are to be placed on each side of the web, then pressed together and bound. The whole of the web interposed may then be removed with the globules *in statu quo*, which may then be submitted to the microscope and measured. The globules may sometimes be as small as  $\frac{1}{1500}$ th of an inch, which is less than the globules of the clouds in some instances as measured by Kæmtz. But generally these globules are much larger. They will be found placed at regular intervals, and while undisturbed show little tendency to coalesce; but when taken into a warm room they appear to lose with the increase of temperature, their cohesion to the line, and easily run off, coalesce with each other and rapidly evaporate; while so doing, instead of a globular form they become elongated ellipsoids, with the long diameter in the direction of the spider's line running through them. In the same fog I have found these globules always of the same dimensions. When the webs are examined over the extent of one or two miles at least, but in different fogs, their sizes are very different. It is necessary to warn the observer respecting an error that might be easily made in examining the web of the common spider, because in its natural state it is found covered with minute globules or beads, which contribute probably to secure the prey of the animal. These beads are arranged with great symmetry. Thus at nearly equal distances will be found globules, as in fig. 2, and between them others smaller dis-

posed with equal regularity. We may distinguish them readily, inasmuch as the globules of water soon disappear, whereas these beads are permanent, and unaffected by the vacuum of the air-pump. Mr. Blackwall has described them fully in the Transactions of the Linnæan Society.

Numerous particles of an organic nature may be detected in rain. But this investigation is liable to many errors which do not exist with regard to hail; because a solid body of ice will remain completely free from all foreign particles in the lower strata, and only contains those which were fixed within it at the moment of its solidification; while in rain we find a mixture of foreign bodies derived from various sources, which the drop of water meets on its passage from the point at which it is formed to the moment of reaching the ground, such as dust raised by the wind, pollen of flowers, and in the neighbourhood of towns, particles of soot continually floating in the atmosphere. It therefore requires great care to avoid coming to erroneous conclusions respecting the nature of the body under examination, and also to detect the existence of organic particles in the midst of others which mask its presence. For these reasons I avoid entering any further upon this subject at present, although deposits of rain-water have presented under the microscope several particles of an organic nature.

The most prevailing forms which the particles of snow in flakes assume, are minute acicular crystals irregularly grouped together, small regular rhombic prisms, often modified by secondary planes, and double six-sided pyramids, like the crystals of sulphate of potash. The more regular particles are generally found deposited on others more irregular, arranged in wreaths and various other shapes. A globular nucleus with small pyramids upon it is a frequent form in flakes of snow. Separate particles of snow affect much more regular forms. In these the hexagonal form, or some tendency to assume it, is generally remarked; and examples of all the forms given in Scoresby's voyage, and of others still more complex, may be detected by the microscope in snow of this climate. The spongy deposit formed on the outside of vessels containing a freezing mixture is found under the microscope to present crystalline forms, which resemble the common forms found in snow much more than those of any other deposit, such as frost, or the arborescent deposit often found on the windows in winter.

In these experiments the hygrometric conditions of the atmosphere, the degree of refrigeration, and even the rapidity with which heat was abstracted, appeared to exert great influ-

ence in modifying the crystalline forms that were obtained. In the same fall of snow a great uniformity is found in the forms of the separate crystals and of those which compose the flakes: sometimes there is only one prevailing shape, at others several very different distinct forms exist, falling at the same time. At the commencement of the fall the forms are generally more regular. In different falls of snow the forms of crystallization are scarcely ever alike or liable to be confounded; and even after a temporary cessation these are found to alter. In every respect we find the closest analogy between the formation of these crystals and those obtained from solutions in which the temperature, degree of condensation, and repose of the liquid, are found to have such influence. Some observations made on separate occasions will better elucidate these remarks.

*Jan. 7, 1846.*—There was a fall of snow, the flakes of which consisted of minute crystals of an octahedric form, placed on acicular ramifications of an irregular description. Besides these, small globules of ice studded with minute pyramidal crystals were perceived under the microscope. The snow examined at intervals was found to contain spherical globules without projecting crystals, which towards the end of the fall of snow gradually increased in size, and might be termed sleet, as they were visible to the naked eye: subsequently the sleet gave place to rain, consisting of minute drops. It is impossible to account for the formation of the mace-like particles of snow, without admitting, as in my experiments, two distinct periods or stages in their development. In the first stage the globular particle is deposited, and in the second the projecting pyramids are formed from moisture, condensed from either a gasiform or a liquid state.

*Dec. 11, 1846.*—A heavy fall of snow commencing early in the morning, which, after a temporary cessation at about 11 A.M., continued until 2 P.M. Many of the crystals were very regular, thin plates, which presented a hexagonal arrangement; some closely resembled fig. 15 of Scoresby's figures, as given in Kæmtz's Manual, without the projecting radii. Some were composed of a very thin hexagonal star, around which nearly circular apertures were disposed at regular intervals, and at the exterior of these six radii. The flakes were composed of irregularly-grouped particles; some of a lance-shape, size  $\frac{1}{300}$ th of an inch; others regular six-sided prisms, like those of frost, about  $\frac{1}{300}$ th of an inch; and lastly, others nearly globular.

*Dec. 12.*—At about 10 A.M. there was a slight appearance of snow during nearly an hour. It consisted of separate par-



ticles, which, from their size, form and colour, nearly resembled the eggs of the common blow-fly. At about 2 P.M. the snow fell more thickly, under the form of small conglomerations or balls, varying from the size of half a pea to three or four times larger. Pressed between the fingers, some of these pellets of snow were easily crushed; only a few offered any degree of resistance. To the naked eye they appeared to consist of spongy crystalline particles: under the microscope they were found to be composed of small regular crystals nearly cubical, mixed with others more irregular of complex outlines: the regular forms were exactly similar to the crystals of frost and the cube-like particles found in the snow on the day preceding. One of these balls submitted to the microscope on a slip of glass, began slowly to liquefy, from the contact of the glass and of the surrounding air. As this was taking place the liquid was diffused by capillarity over the entire mass, without any trace of moisture appearing externally until the entire ball was in a state of semi-fusion, and of a dull and semi-transparent white like a small hailstone: at the same time the ball contracted in size and assumed a biconvex or lenticular shape. While these changes were taking place the crystals became fused at the angles and edges, most of them becoming globular and ovular: in this condition several of the balls were exposed to the open air, then several degrees below the freezing-point. When examined the next morning they were found perfectly hard, of a regular lenticular shape, of a dull white colour, in every respect like certain hailstones which I had collected during the summer. Under the microscope elliptic and circular particles were seen, like those composing hailstones. Previous to and during their fusion, bubbles of air were seen exactly as in the hailstones, though in less abundance. Some of these were enclosed, as represented at p. 105, vol. xxviii. *Phil. Mag.* This observation shows in a direct manner, with a much greater degree of precision than I could have anticipated, the process of formation of hail as it takes place in general. In the first place, an agglomeration of crystalline particles, probably in consequence of certain vorticose currents, takes place in a globular shape. The ball of snow, thus formed by the capillary attraction which it possesses from its porous structure, absorbs any moisture that may exist around it. The effect of this moisture is twofold; first, as modifying the angular outlines of the component particles; and secondly, as binding them together into a consistent mass, which any further abstraction of caloric will reduce to the state of a hard and solid hailstone. If the addition of moisture is considerable, the liquefaction of the

snow may continue until all traces of separate crystalline particles have disappeared and the air has been disengaged, the whole becomes transparent, and still capable, by its low temperature, of easily solidifying and forming a transparent mass of ice. In either case the deposition of successive layers of transparent ice, or of confused solid grains, may be accounted for by a continuance of the same actions.

In order to exemplify this, it is not requisite to confine ourselves merely to snow already in a globular state: any kind of snow slightly pressed into a globular shape becomes hardened and modified by diffusion of water through it, in consequence of slight melting of its particles. According as this is more or less continued, we obtain all the dull white tints of various hailstones. If the mass thus obtained be now exposed to the air below the freezing-point, the process is still further completed by its entire solidification.

It still remained to be seen whether the conglomerations of snow contained, like hail, any organic particles. I was desirous to determine this point, because I considered them as being much less liable to contain foreign matter, such as dust &c. of the lower strata of air, than the common flakes of snow: at the same time we must admit the possibility of the introduction of foreign particles, even after the entire formation of the ball, much more than in a solid body of ice like a hailstone.

In order to obtain a ball as free from impurity as possible, one of the hardest was selected, and after the exterior parts had been carefully brushed off, it was inclosed in the little apparatus before described. Several conglomerations were operated upon in this manner. In these the foreign particles were numerous; they all appeared irregular and of an amorphous inorganic nature, except some green globules closely agglomerated together, some in large quantities, others only consisting of groups of three or four: their size was about  $\frac{1}{3000}$ th of an inch, and they were very similar to some found in hail, the vegetation of which was not examined (see p. 105, vol. xxviii. *Phil. Mag.*). The drops of water resulting, with their contents, were examined every day. The first two days they were kept in a room which averaged a few degrees above congelation; no alteration of the particles was detected. The third day they were exposed to the rays of the sun: on the fourth day the green globules had emitted slight filaments. Every succeeding day these filaments increased, and near them were transparent granules scattered about which increased very slowly, as represented at fig. 3. These vegetations were preserved for more than a month by occasion-

ally adding a drop of distilled water, so as to prevent their complete desiccation. At the end of this time the group of vegetation had but slightly increased, but around were several other groups of vegetations of the same nature which were not perceived at the commencement of the examination. Their examination, which hitherto had been made nearly every day, was then abandoned as presenting no further object.

As it is possible to distinguish snow of different formations, its minute examination made simultaneously by different observers over a considerable extent of country, would enable us in all probability to determine several interesting points connected with the formation of snow. By their aid we might ascertain to what distance in certain cases the same crystalline form is preserved, and in what manner this is effected; whether by a simultaneous formation, or by a gradual propagation from one point to another; also at what rate of speed this is effected.

The frequent transformation of the vapours of water from the gasiform to the nebular state, commencing from a small cloud at the horizon and rapidly extending to the whole visible sky, bears a close analogy to the action of crystallization of bodies in solution in a liquid. In both cases a molecular action commencing from a circumscribed point is gradually propagated to the entire mass, when at a proper degree of saturation in the liquid solution the crystals formed are all alike. In the meteorological phenomenon, as it is a liquid that falls, the crystalline action cannot be observed; but the frequent production of hail renders it not unlikely that the clouds may sometimes be composed of solid particles.

From their connection with the preceding remarks I will mention the following experiments:—

Exp. 1. A saturated solution of sulphate of soda covered with a thin coating of oil was allowed to cool, and remained at rest for several hours. On a glass rod being brought in contact with the surface, the well-known phenomenon of the solidification of the solution ensued. The speed at which this solidification travelled from one extremity of the tube to the other was at about the rate of one foot in forty seconds. The crystalline particles thus formed consisted of long aciculæ about  $\frac{1}{200}$ th of an inch in thickness.

Exp. 2. In a similar solution there were deposited, previous to its solidification, beautiful transparent, hard crystals, which are known to differ from Glauber's salts by their containing only eight equivalents of water of crystallization to one of salt. After the solidification had taken place the crystals were found to be changed to a milk-white colour, although they still retained the same shape. Under the microscope they were

found to consist of minute irregular particles, which accounted for the change of colour and opacity. It appears, therefore, as if the molecular action, which causes the solidification, has the power of disaggregating or breaking up the crystalline arrangement of a body in a solid state.

Exp. 3. A solution of sulphate of soda which is not saturated, or has not been allowed to cool entirely, is affected by the contact of a foreign body much more slowly. After the lapse of several minutes, the formation of separate clusters of crystals much larger than the former is observed: after about half an hour, these clusters were seen at numerous intermediate points throughout the length of the tube, which was several feet long.

Frost and snow examined under a magnifying power are much more alike than we should expect from their appearance to the naked eye. I have already mentioned the small, nearly cubical prisms, precisely like those of frost, which are found in snow. In the following instance of hoar-frost the likeness is still more complete.

Dec. 28, 1846.—The ground and all minute filaments were covered with abundant crystals of frost: thermometer  $27^{\circ}$  F. The filaments of the spider's web were studded with quadrilateral prisms ranged transversely along the threads. In the afternoon a slight fall of snow occurred, whose particles were exactly like those of Dec. 12, except that they fell in large flakes. These were examined together with other particles of frost deposited on a web, and found to be precisely the same, except that the particles of snow were about a third smaller than those of frost.

Several salts manifest a peculiar tendency to ascend and crystallize on the sides of the vessel which contains them, but none of them so greatly as water when it solidifies in a cavity formed by moist earth. The first crystals formed are considerably above the surface of the water; and they increase until a solid crust is formed by them and the cavity below remains perfectly dry. Camphor, naphthaline, iodine and hydrosulphate of ammonia, resemble water in their volatile properties, and their tendency to solidify in a crystalline shape. The tendency of the first to deposit itself in crystals on the side of the glass nearest the light is well-known. Naphthaline in the same condition slowly deposits a few minute crystals on the same side: after the lapse of a month a few scattered clusters of them were formed, after which no further increase of them was perceived even at the end of several months. Iodine exposed in a large bottle to the sun during the summer formed deposits of different forms on various occasions, but all composed of rhombic

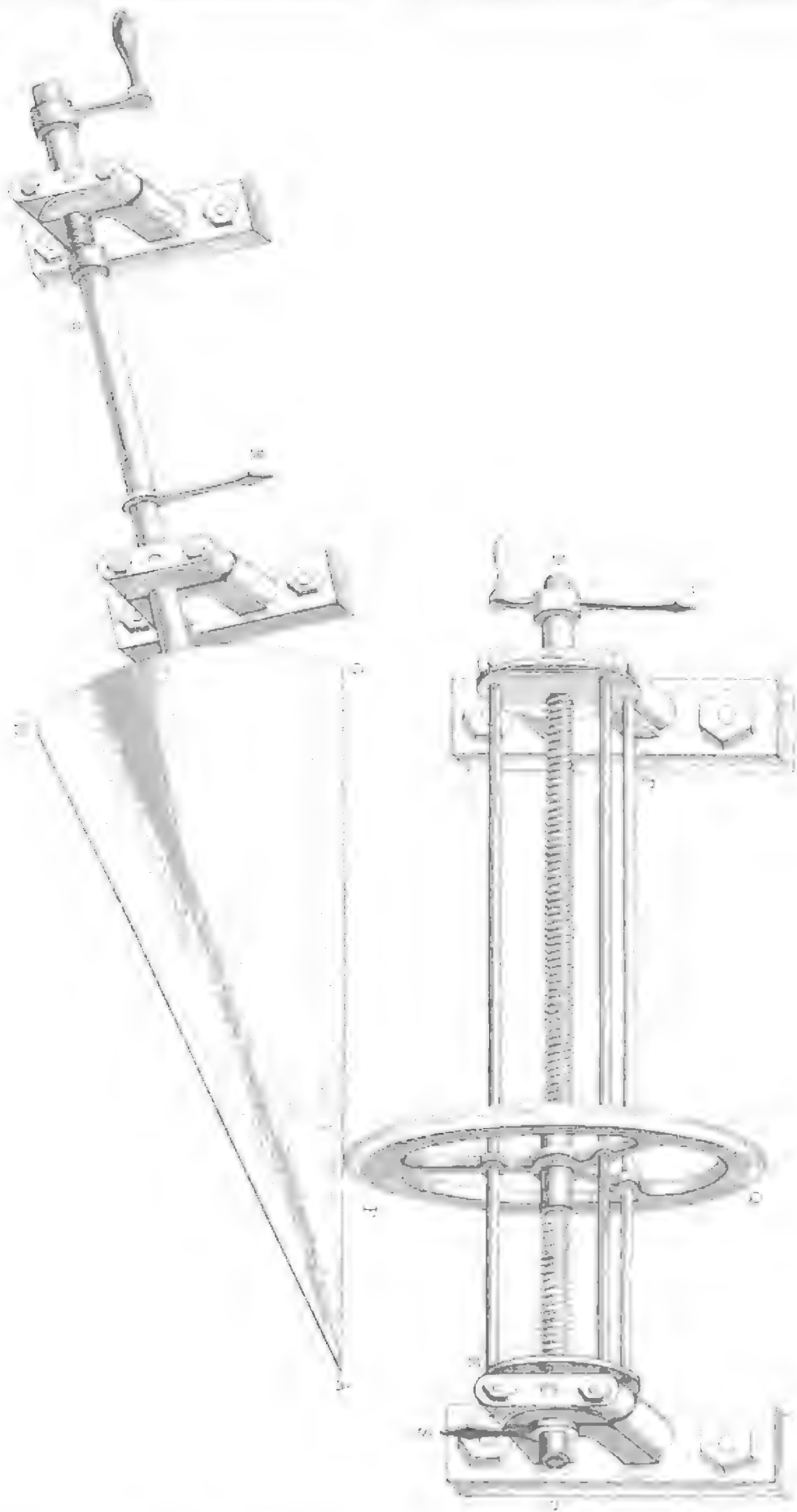
*Phil. Mag. S.3. Vol. 30. No. 200. March 1847.* N



plates slightly modified. Some of these rhombic crystals were separate, about the tenth of an inch in size, and with angles of about  $50^\circ$  and  $130^\circ$ : some of the crystals formed long, thin, narrow blades, above half an inch in length, with a bright metallic lustre: on their edges were two or three indentations indicating the separate crystals.

*Hydrosulphate of ammonia.*—This is to be obtained by bringing into contact ammonia and sulphuretted hydrogen in a gaseous state and perfectly dry at a low temperature. We then observe the formation of a white crystalline deposit on the sides of the vessel in which the gases are received. Any further directions respecting the mode of obtaining it will be found in Thenard's Chemistry, Berzelius, &c.

Hydrosulphate of ammonia is so extremely volatile, that a piece of lint wetted with æther and placed in contact with any part of the bottle which contains it, causes the salt to form a deposit at the inner surface beneath. The crystals in these cases are frequently very regular hexagonal stars, like those of snow. A current of water falling on any part of the bottle rapidly produces the same effect. In summer it generally sublimes to the upper part of the bottle, on account of the greater heat which is communicated to the base of the bottle by contact with the shelf on which it is placed. In a clear night the opposite effect is produced when placed in contact with the ground. But it is particularly when employed as a differential thermoscope that the indications of this salt are interesting. Thus, during a clear night the salt rapidly accumulates towards the points at which the greatest radiation of caloric takes place. A bottle containing this salt placed between two unequal radiating surfaces, indicates, by its change of position, the difference in the radiation of these surfaces. In fact wherever the equable distribution of caloric is disturbed in the same direction for some time, the altered position of the hydrosulphate serves as an indicator of the course it has taken, and the forms of the crystals show to a certain extent the rapidity with which it has been effected, because where the action is rapid the crystals are much smaller and more confused, than when it has been more gradual. It is surprising how small a difference in the radiating power is sufficient to alter the position of the salt when properly disposed. I will not at present enlarge any further on the thermoscopic properties of the hydrosulphate. It presents all the conditions corresponding to those of water. During its preparation it is frequently observed in the air in a state of microscopic crystals representing snow: the crystals formed by rapid evaporation are exactly like the hexagonal plates of snow. A more gentle action gives



rise to crystallization, like those of frost and of congealed water. Although we have rarely seen it in a liquid state, yet during summer, under the influence of the sun's rays, its crystals become perfectly liquid and assume a globular form, and which they retain as they again solidify; forming, in these cases, conditions corresponding to water in a liquid state, ice and sleet.

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XXX. *On a Machine for Calculating the Products, Quotients, Logarithms, and Powers of Numbers.* By the Rev. HENRY MOSELEY, M.A., F.R.S., one of Her Majesty's Inspectors of Schools, and lately Professor of Natural Philosophy and Astronomy in King's College, London\*.

[With a Plate.]

I HAVE proposed to myself in the construction of this machine, to determine mechanically the products, quotients, logarithms, squares, square roots, and other powers and roots of the natural numbers, by means of combinations of greater simplicity than have hitherto been applied to the purposes of mechanical calculation.

The accompanying Plate II. is intended to illustrate the *principle* of the machine, but it does not show the mechanical details of its construction or the due proportion of its parts.

It will be observed that it consists principally of a cone and a screw. The application of the cone and the disc to various purposes of mechanical calculation has long been well known, and particularly by the ingenious applications made of the disc to dynamical admeasurement by MM. Poncelet and Morin.

The novelty of *this* instrument consists in the combination of the screw with the cone.

CD represents a small screw terminated by solid cylindrical axes or gudgeons, of which that shown by the letter D is prolonged, and carries an index T. The screw admits of being fixed in its bearings or made to revolve in them.

PQ is a wheel having a hollow cylindrical axis, into which is accurately fitted a hollow cylindrical piece, whose internal surface is traversed by the thread of a female screw working upon the male screw CD.

The hollow axis of the wheel PQ admits of being fixed upon the cylindrical piece inserted in it, or of being disengaged from that piece, so that the wheel PQ may be made to carry the female screw round with it in its revolutions, or to turn freely upon the outside of the cylinder which contains that screw, as upon an axis.

\* Communicated by the Author.

The three arms of the wheel PQ are pierced by apertures, through which pass, accurately fitting them, the three rods of the rigid frame EF, which frame turns in fixed bearings at C and D upon hollow axes, through which axes pass the cylindrical extremities or gudgeons of the screw CD.

The wheel PQ traverses the frame EF longitudinally, and at the same time carries it round with it in its revolutions: and it admits of being *fixed* upon the frame at any distance from either extremity. The frame carries an index S.

AGH is a cone, whose side AG is parallel to the screw CD, which turns by means of a spindle AB upon fixed bearings, and which is continually pressed upon the edge of the wheel PQ by a spring acting upon the extremity of its spindle. The cone is of some soft metal, brass for instance. The edge of the wheel PQ is very thin, of steel, and milled, so that it imprints a minute tooth upon the soft face of the cone at every point where it revolves in contact with it.

The spindle AB carries an index R. The indices R, S, T point to equidistant divisions upon fixed circles, not shown in the Plate; and each of them is connected with a train of wheels and pinions, whose numbers of teeth are in the proportion of ten to one, and which serve to measure the complete number of revolutions and parts of a revolution made by each index.

Now certain dimensions (hereafter to be determined) being assigned to the different elements of this combination, it may be shown,—

1st. That if the frame EF be fixed, and the screw CD turned until the index T points to any given number  $N_3$ , and then the frame be released, the wheel PQ *fixed* upon the frame, and the cone turned until the index R points to any other given number  $N_1$ , then will the index S point to the product of the numbers  $N_1$  and  $N_3$ .

2nd. That if, as before, the frame EF be fixed, and the screw turned until the index T points to any number  $N_3$ , and then the wheel PQ be *fixed* on the frame, the frame released, and the cone turned until the index S points to any other number  $N_2$ , then will the index R point to the *quotient* of  $N_2$  by  $N_3$ .

3rd. That if the *screw* CD be fixed, the wheel PQ and the frame released, and certain other adjustments made, and if the cone be turned until the index S points to any number  $N$ , then will the index R point to the *logarithm* of that number.

4th. That if the spindle of the cone be connected with the axis of the screw CD by means of two bevelled wheels, so that the rotation of the one shall always bear a given relation to that of the other, then the frame EF and the wheel PQ being



both released, and the cone turned until the index R points to any number, the index S will point to the *square* of that number.

*Theory of the Instrument.*

Let  $\iota$  be taken to represent half the angle at the apex of the cone,  $\rho$  the radius of the wheel PQ, and  $\lambda$  the distance between each two threads of the screw; also let  $n_1, n_2, n_3$  represent severally the numbers of equidistant divisions on the circles to which the indices R, S, T severally point; then will  $\frac{N_1}{n_1}, \frac{N_2}{n_2}, \frac{N_3}{n_3}$  represent the numbers of revolutions made severally by the indices whilst the numbers  $N_1, N_2, N_3$  are registered. Now suppose that when the wheel PQ is at the apex of the cone the indices R, S and T each point to zero, and the frame EF being fixed, let the screw CD be turned until the index T shows the number  $N_3$ ; then will the screw have made  $\frac{N_3}{n_3}$  revolutions, and the wheel PQ will have advanced from the apex of the cone to a distance AP from it, represented by  $\frac{N_3}{n_3} \cdot \lambda$ ; so that the radius PM of the circle described by that point whilst the cone revolves will be represented by  $\frac{N_3}{n_3} \lambda \sin \iota$ , and its circumference by  $2\pi \frac{N_3}{n_3} \lambda \sin \iota$ . Now let the cone be made to revolve (the wheel PQ being fixed upon the frame and the frame released) until the index R shows the number  $N_1$ , or until the cone has made  $\frac{N_1}{n_1}$  revolutions; then will the point P in it have been made to revolve through a space represented by

$$2\pi \frac{N_3}{n_3} \cdot \lambda \sin \iota \cdot \frac{N_1}{n_1},$$

or by

$$\frac{2\pi \lambda \sin \iota}{n_1 n_3} \cdot N_1 N_3.$$

But by reason of the continual pressure of the cone upon the edge of the wheel PQ, that wheel will have been carried round in the revolution of the cone, so that the circumference of the wheel will have described precisely the same space as the point P of the surface of the cone; if therefore  $N_2$  represent the number pointed to by the index S, so that  $\frac{N_2}{n_2}$  represents the number of revolutions made by the frame EF and wheel

PQ, and therefore  $2\pi\rho \frac{N_2}{n_2}$  the space described by its circumference, we shall have

$$2\pi\rho \frac{N_2}{n_2} = \frac{2\pi\lambda \sin i}{n_1 n_3} \cdot N_1 \cdot N_3;$$

or 
$$N_2 = \left( \frac{n_2 \lambda \sin i}{\rho n_1 n_3} \right) N_1 \cdot N_3.$$

Let now the quantities  $n_1, n_2, n_3, \rho, \lambda, i$  be so assumed that

$$\left( \frac{n_2 \lambda \sin i}{\rho n_1 n_3} \right) = 1; \quad \dots \dots \dots (a.)$$

then 
$$N_2 = N_1 \cdot N_3. \quad \dots \dots \dots (1.)$$

that is to say, the number  $N_2$  shown by the index S is the product of the numbers  $N_1$  and  $N_3$  shown by the indices R and T.

Dividing equation (1.) by  $N_3$ ,

$$N_1 = \frac{N_2}{N_3}; \quad \dots \dots \dots (2.)$$

whence it follows that if, proceeding in other respects precisely as before, we cause the cone to revolve, not until its own index R points to a given number, but until the index S points to the given number  $N_2$ , then will the index R of the cone show a number  $N_1$ , which is the *quotient* of  $N_2$  and  $N_3$ .

To adjust the instrument to show the *logarithms* of numbers, let the wheel PQ be brought to the apex of the cone, and let the screw CD be fixed, and the index S then made to point to zero. Let the cone then be turned until the index S points to unity, and in this position of the cone let the index R be made to point to zero. The frame EF being then allowed to turn freely, and the wheel PQ to traverse freely on the frame, the number  $N_1$  shown by the index R will always be the logarithm of the number  $N_2$  shown by the index S. For while the number  $N_1$  is increased by the exceedingly small number  $\Delta N_1$ , let the number  $N_2$  be increased by  $\Delta N_2$ ; the parts of a revolution made by the cone and wheel respectively whilst  $N_1$  and  $N_2$  receive these small increments, will then be  $\frac{\Delta N_1}{n_1}$  and  $\frac{\Delta N_2}{n_2}$ , and the arcs to radius unity,  $2\pi \frac{\Delta N_1}{n_1}$  and  $2\pi \frac{\Delta N_2}{n_2}$ . Moreover, the wheel PQ having made  $\frac{N_2}{n_2}$  revolutions upon the *fixed screw* CD, will have been carried from the apex of the cone to a distance from it represented by

$\frac{N_2}{n_2} \lambda$ ; so that the radius PM of the circle in the act of being described by the point P upon the surface of the cone whilst the numbers  $\Delta N_1$  and  $\Delta N_2$  are being registered, will be  $\frac{N_2}{n_2} \lambda \sin i$ ; and the exceedingly small arc described by that point whilst these numbers are registered, will be represented by

$$2\pi \frac{\Delta N_1}{n_1} \cdot \frac{N_2}{n_2} \lambda \sin i.$$

But this arc, described by P upon the cone, is equal to that described in the same time by a point in the circumference of the *wheel*, which last is represented by  $2\pi \frac{\Delta N_2}{n_2} \rho$ ;

$$\therefore 2\pi \frac{\Delta N_1}{n_1} \cdot \frac{N_2}{n_2} \cdot \lambda \sin i = 2\pi \frac{\Delta N_2}{n_2} \rho;$$

$$\therefore \left( \frac{\lambda \sin i}{n_1 \rho} \right) \Delta N_1 = \frac{\Delta N_2}{N_2};$$

or passing to the limit and integrating between the limits *unity* and  $N_2$ ,

$$\left(\frac{\lambda \sin i}{n_1 g}\right) N_1 = \log_e N_2;$$

$$\therefore \left( \frac{\lambda \sin i}{2 n_1 t} \right)^{N_1} = N_2.$$

Let the quantities  $\lambda, \mu, \nu, \rho$  be so assumed that

$$\frac{\lambda \sin i}{n_1} = 10; \quad . \quad . \quad . \quad . \quad . \quad . \quad (\beta.)$$

$$\therefore 10N_1 = N_2;$$

$$\therefore N_1 = \log_{10} N_2 \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

whence it follows that the number shown by the index R (under this adjustment of the instrument) will in every position be the common logarithm of the number then shown by S.

If the spindle AB be made to carry round with it the screw CD by the intervention of two bevel wheels the numbers of whose teeth bear a given ratio,  $n$ , to one another, so that the screw CD may make  $n$  revolutions or parts of a revolution whilst the spindle AB makes one; if, moreover, the wheel PQ be so released from the female screw, which forms its centre, as that the outer surface of that screw may serve for an axis about which it may turn freely whilst it is still carried along the frame by the longitudinal motion of the screw; and if the in-







then 
$$N_4 = N_1^8, \dots \dots \dots (6.)$$

and 
$$N_1 = \sqrt[8]{N_4}; \dots \dots \dots (7.)$$

so that (under this combination) the number  $N_4$  shown by the index of the second frame is, in every position, the cube of that shown by the index  $R$ ; and conversely, the number shown by the index  $R$  is the cube root of that shown, at the same time, by the index of the second frame.

In like manner, by placing any number of like systems of a frame, screw and wheel, upon the side of the cone, the third screw receiving its motion from the second frame, the fourth from the third, &c., any other powers of the number  $N_1$ , or roots of the numbers  $N_2$ ,  $N_4$ , &c. may be conceived to be determined, the power shown by the last index, or the root by the first, being one greater than the number of frames so applied.

The four equations of condition ( $\alpha.$ ), ( $\beta.$ ), ( $\gamma.$ ), ( $\delta.$ ) between the nine quantities  $n_1$ ,  $n_2$ ,  $n_3$ ,  $n_4$ ,  $n$ ,  $n'$ ,  $\lambda$ ,  $\iota$ ,  $\rho$  leave five of them undetermined. These are to be assumed of such values as may be found most convenient in the construction of the machine, the *general principle* of which it is the object of this paper to explain.

The Plate in particular, as has already been stated, is by no means intended to show the details of the construction of the machine, or even the general proportions of its parts, but simply to illustrate its principle. There are, indeed, some elements of its construction, which, for the sake of simplicity, have been altogether omitted from it. These are the trains of wheels which must be connected with each of the axes carrying the indices  $R$ ,  $S$ ,  $T$  to register the *complete* numbers of revolutions made by those indices, and the divided circles to which the indices point, showing such portions of the numbers  $N_1$ ,  $N_4$ , &c. as correspond to less than one complete revolution.

It will be obvious that, since the number  $N_2$  represents (in the first adjustment of the instrument) the product of the numbers  $N_1$  and  $N_3$ , the divisions on the circles pointed to by the indices  $R$  and  $T$  must be greater than those of the circle pointed to by the index  $S$ , or the numbers  $n_1$  and  $n_3$  less than the number  $n_2$ .

It will, moreover, be observed, that the instrument, whilst it differs from other calculating machines, and claims to be superior to them in the simplicity of its combinations, differs also, and is inferior to them in this respect, that the truth of its registrations is dependent on the mechanical accuracy of its construction.

Those elements of the machine, on the mechanical truth

and adaptation of which its accuracy depends, are,—1st, the screw, the convolutions of whose thread must be of geometrical uniformity; 2nd, the cone, whose surface must be of geometrical truth; and 3rd, the wheel, whose edge must have so intimate a contact *with*, and hold *upon*, the surface of the cone, as to partake accurately in its motion at every point which it traverses.

My experience in the use of a similar cone and wheel in a steam indicator constructed at the expense of the British Association of Science, has convinced me that the required accuracy is in this last respect attainable.

In respect to the two first-mentioned sources of error, it may be observed that there are no mechanical forms of greater simplicity than the *cone* and the *screw*, and probably none in respect to which greater truth of construction is attainable.

Every such instrument must however have its error; its *amount* in respect to this instrument may be determined by an obvious method, and it is probable that it may in every case be corrected by a corresponding *adjustment*.

XXXI. *On the use of a mixture of Spirit of Wine and Camphine, as a Light for Optical Purposes.* By JOHN GEORGE CHILDREN, Esq., F.R.S.

*To Richard Taylor, Esq.*

MY DEAR SIR,

Halstead Place, Feb. 13, 1847.

**I**F you think the following trifle worth inserting in the Philosophical Magazine, it is at your service. I have lately assisted a friend in getting up an apparatus for dissolving views, &c. on a scale too large for sufficient illumination by the best Argand lamp, and we considered condensed hydrogen gas as too dangerous an agent for a plaything; especially in private houses in the country, where much must be left in the preparation of the gases, &c. to servants unused to such duties. We set to work therefore to find a substitute that might answer our purpose; and after a few trials we obtained a tolerable light, by throwing the flame of spirit of wine on a surface of quicklime by a current of oxygen gas, something after the manner of Lieut. Drummond's original experiment\*. The illumination by the flame of spirit of wine alone, however, proved to be too feeble; but by mixing a portion of camphine (spirit of turpentine) with it (which readily dissolves in alcohol)

\* Philosophical Transactions, 1826, p. 330. Drummond's object and ours were somewhat different. He wanted to get an intense but small sphere of light, "adapted to the nature of a (parabolic) reflector:" we also wanted as intense a light as we could obtain, but, as we could not advantageously use a reflector, one which should likewise extend over a much larger surface.

in the proportion, by measure, of one part of camphine to eight parts of spirit of wine, of the specific gravity of  $\cdot 841$  at  $60^{\circ}$  F. (equivalent to the spirit of commerce of  $60^{\circ}$ ), we obtained a light amply sufficient, not only for my friend's dissolving views, but also for his microscope and physioscope, and perfectly free from all danger, or even possibility of explosion. My friend's screen is about  $22 \times 18$  feet; and if it were twice as large, the light is quite capable of illuminating it brilliantly; and it shows a microscopic object, magnified from half an inch to thirty feet, or 720 times, linear, with great distinctness and beauty. I do not mean that our toy can be compared to the almost solar intensity of the oxy-hydrogen light, also invented by the late Lieut. Drummond\*, and now in common (too common?) use; but in a trial I made the other day in Francis Street, with an apparatus hastily put together for the purpose by Mr. Collins, I found its effect, measured by one of Wheatstone's photometers, equal to that of seventy-six of Brecknell and Turner's best platted-wick wax candles; and in several trials with our own apparatus, more carefully arranged, and with thoroughly well and recently burnt lime (an essential caution), we have found it quite equal to 108 of the same candles, and on one occasion to 120! The oxy-hydrogen light at Mr. Collins's, which was splendidly brilliant, being compared in the same manner, was found equal to 121 of the same candles.

In these experiments, no portion of the rays from either of the lights was intercepted; our object being to ascertain the comparative illuminating power of their entire surfaces, and not their comparative intensities only†.

Were it not for the peculiar odour of the so-called naphtha or coal-oil, which to some persons is highly offensive, even in its purest state, it might, when highly rectified, be advantageously substituted, in an æconomical point of view, for the spirit of wine. Four ounces of camphor dissolved in a pint of that liquid, gives, under the same circumstances, nearly as brilliant a light as the spirit and camphine.

The light from camphine alone is for a few seconds intensely brilliant, but it is soon quenched in the enormous mass of unburnt carbon, which partly condenses on the lime and partly escapes into the atmosphere, filling the whole apartment with a dense and almost suffocating cloud of floating black particles.

\* Philosophical Transactions, 1830.

† Some attempts indeed were made in Francis Street to compare the intensities alone; but the apparatus, from want of time to prepare a better, was too imperfect to allow me to place any confidence in the results. As far as they went, they were greatly in favour of the superior *intensity* of the oxy-hydrogen light. I hope to repeat the trials with a more refined apparatus.



A sufficient and well-regulated supply of oxygen gas might perhaps remedy this defect.

Our apparatus consists of a copper lamp with two tubes lying close together, and each containing a wick formed of flat cotton rolled up into a cylinder; and a cylinder of lime, about three-eighths of an inch long and one-eighth of an inch in diameter, inclosed in a thin copper case. The pipe conveying the oxygen gas from the gasometer terminates in a small jet, inclining upwards, which lies between the two wicks slightly parted to receive it, and within rather less than one-eighth of an inch from the circular disc of lime, and about one-fourth of an inch above the lower edge of the copper case. We find that the common chalk of this neighbourhood furnishes a lime which gives a better light than that from the Bristol, or any other limestone we have tried\*.

Mr. Collins, philosophical instrument-maker to the Royal Polytechnic Institution, makes lanterns for dissolving views and microscopes, fitted up with the spirit and camphine light, and will be happy to show its effect to any gentleman who may wish to see it. His address is "26 Francis Street, Tottenham Court Road," and at the Polytechnic Institution.

I am ever, my dear Sir, faithfully yours,

JOHN GEORGE CHILDREN.

XXXII. *On the Newly discovered Planet.* By M. ENCKE†.

THE new planet, the orbit and present position of which had been announced *à priori* by M. Le Verrier of Paris, was discovered almost immediately at the Berlin observatory by Dr. Galle: the very first night the Hora XXI. of the Academy's star-maps (which have been prepared with such extraordinary care and accuracy by Dr. Bremiker) was compared with the heavens, and thus afforded the most brilliant proof of the truth of theory, and the wonderful sagacity with which M. Le Verrier had made use of the existing data. With this confirmation, so far exceeding any expectations which could have been previously entertained, the name of M. Le Verrier will ever be connected, and acquire in consequence of it a celebrity as justly merited as it is unexampled in respect of the mode in which it is gained.

From the slowness of the motion of the planet, the observations hitherto made at the Berlin observatory, although they extend to twenty-five days, may all be connected with a star occurring in Bessel's zones, the mean place of which was preliminarily assumed to be

$$327^{\circ} 56' 56''\cdot 4 - 19^{\circ} 26' 9''\cdot 6$$

\* Drummond also obtained the most brilliant light from chalk lime.

† From the *Berichten des Akad. der Wissenschaften zu Berlin*, Oct. 22, 1846.

until more accurately determined. To estimate the distance of the planet, a circular orbit was assumed by Dr. Galle, which agrees so accurately with all the individual positions, that the tempt to determine an elliptic orbit must be deferred for some time. A comparison with the assumed orbit,—

Epoch Sept. 24.0 mean Berlin time  
Mean longitude. . . . . 326° 58' 23".5  
Ascending node . . . . . 131 1 10.8  
Inclination . . . . . 1 52 51.5  
Semidiameter . . . . . 30.03885  
Mean daily sidereal motion 21".55171

gave the following results :—

1846.	Mean Berlin time.	Observed AR.	Observed Declination.	Difference.	
				AR.	Declination.
Sept. 23.	<sup>h</sup> 12 0 14.6	328 19 16.0	−13 24 8.2	+2.3	+0.9
24.	8 54 40.9	18 14.3	24 29.7	+0.1	+0.1
25.	9 41 45.0	16 59.8	24 55.4	0.0	−0.5
26.	10 11 54.3	15 48.3	25 22.2	−1.1	+0.9
27.	8 29 48.9	14 42.8	25 44.0	−0.6	+0.6
28.	11 31 28.8	13 25.5	26 11.5	−0.3	+0.5
29.	9 14 3.7	12 23.4	26 32.6	+1.2	+0.3
Oct. 2.	11 5 35.2	9 7.0	27 41.4	+0.3	+0.3
6.	8 0 54.2	5 16.2	29 1.3	−0.1	+0.4
10.	8 52 56.3	1 42.2	30 14.7	−1.4	−0.7
15.	7 27 25.7	327 57 52.8	31 32.8	0.0	−0.1
18.	7 37 43.7	55 51.8	32 15.2	+2.5	+1.0
...	7 58 47.6	55 55.0	32 14.2	−1.2	−0.1

The observations made at other observatories, as far as I have become acquainted with them, present a similar agreement, since a constant difference from the Berlin observations must most probably be referred to the assumed position of the star.

1846.	Mean Berlin time.	Observed AR.	Observed Declination.	Difference.		
				AR.	Declination.	
Sept. 27.	<sup>h</sup> 9 41 46.1	328 14 35.8	−13 25 54.0	+2.9	− 8.9	Göttingen.
28.	9 37 37.9	13 28.2	26 15.0	+2.3	− 5.9	Hamburg.
29.	9 33 37.6	12 18.5	26 37.3	+5.3	− 4.7	Hamburg.
...	9 33 45.4	12 21.8	26 39.8	+1.9	− 7.2	Altona.
Oct. 1.	9 25 37.8	10 7.8	27 28.2	+6.4	−10.4	Hamburg.
...	9 25 45.0	10 11.7	27 24.5	+2.4	− 6.8	Altona.
3.	9 1 23.5	8 9.6	28 0.5	+0.2	0.0	Lond. (Hind).
4.	9 13 37.3	7 5.3	28 30.0	+4.4	− 8.4	Hamburg.
5.	8 26 20.5	.....	28 41.4	.....	+ 0.1	Lond. (Hind).
...	10 45 45.9	6 6.9	28 44.7	−0.3	− 1.4	Hamburg.
6.	9 5 37.8	5 10.5	29 9.6	+3.0	− 7.9	Hamburg.
...	9 5 45.5	5 12.6	29 6.3	+0.7	− 4.6	Göttingen.
8.	9 6 43.1	3 22.8	29 40.4	+0.9	+ 0.6	Turin.
9.	9 2 43.5	2 27.0	29 53.4	+4.3	− 4.5	Turin.
10.	8 58 43.9	1 32.2	30 17.9	+8.4	+ 2.5	Turin.

The elements are indeed merely approximative; but since a circular orbit, previously calculated by me from other observations, gave very nearly the same numbers, it may at least be concluded from them that the planet is much nearer than M. Le Verrier had assumed. Its distance from the sun should be 33 according to his elements, while according to the above it will not differ much from 30; consequently the period of revolution will also be shorter. If the orbit were actually circular, the period would be about 165 years.

In another point, however, with respect to which M. Le Verrier had only presumptions to go upon, he has hit the truth with most remarkable accuracy. According to him, the disc of the planet amounts to about  $3''.3$ , while the mean of the measurements gives  $2''.7$ .

I take this opportunity of expressing my views with regard to the name which I intend to assign to the planet for the next year, as I am compelled to come to some decision on account of the publication of the *Astronomical Jahrbuch*, in which the planet must be inserted as soon as its elements are more accurately determined. To every new planet, the astronomers who were countrymen of the discoverer at first added appellations, which were intended to call to mind some peculiar circumstance. Thus the English called and still call Uranus the *Georgian planet*\*, as a mark of gratitude to king George the Third, whose munificence enabled Herschel to construct his great reflectors; and the planet was detected, as is well known, by its disc. Piazzi named his planet Ceres *Ferdinanda*, in honour of the king of Naples, the founder of the observatory at Palermo. In the same manner Pallas was at first called *Olbersiana*, until Olbers himself pronounced strongly against the adjective. Subsequently the epithets have been disused by the German discoverers, Harding, Olbers and Hencke; and according to the suggestion of Bode, the name Uranus has been everywhere introduced except in England, and the name Ceres is now generally used without any addition.

The astronomers of Paris appear also to have had respect to this custom of naming the planets after the ancient divinities, principally of the Romans; since Le Verrier himself, in his first letter to Dr. Galle, Oct. 1, in answer to the announcement of the discovery of the planet, states at the conclusion, "the Bureau des Longitudes has pronounced for NEPTUNE, the sign a *trident*." This too appears to have been after some consideration; for as Dr. Galle had in his letter hinted at the name *Janus*, M. Le Verrier observes that "the name Janus

\* In the Nautical Almanac only. Uranus is the name now in general use with British astronomers.—Ed.

would indicate that this planet is the last of the solar system, which there is no reason whatever to suppose."

Moreover, this name [Neptune] has been widely bruited; for in a letter which I have received from Gauss, our first German authority in astronomy, dated Oct. 7, he says, "I consider the name Neptune chosen by M. Le Verrier to be very appropriate; perhaps a trident might be selected as the sign if it were not improper in any way to interfere with the rights of the discoverer."

Now in a letter which I have received from M. Le Verrier, dated Oct. 6, he states, "I requested my illustrious friend, M. Arago, to choose a name for the planet. I was somewhat startled at the decision (*j'ai été un peu confus*) which he made at a sitting of the Academy." This confirms the information we have received through other channels, that the name *Le Verrier* had been proposed by M. Arago.

Fortified by the high authority of Gauss, and the *Bureau des Longitudes* of Paris, I shall, under these circumstances, retain for the next year the name Neptune, and the sign of the trident, until public opinion in Germany has become sufficiently consolidated to establish a definitive appellation. Our German custom has prevailed in the case of four, we may even say five new planets, as Herschel was a German by birth; and as it cannot be in the least my intention to undervalue the great merits of M. Le Verrier, which I have most cordially admitted, so it is my opinion that his name will ever remain so firmly connected with Neptune, that it is not necessary, in order to keep up the recollection of the discovery, to introduce the somewhat incongruous collocation of the heathen gods with a modern name. Besides, in the present case, a German has the essential merit of having discovered the planet.

In letters which I have since received from Sir John Herschel, the planet is also called Neptune; and Struve, in Pulkowa, has pronounced decidedly for retaining this name. The first astronomical authorities, therefore, in Germany, France, England and Russia, have pronounced in favour of NEPTUNE.

*Note.*—A paper received by the Astronomer Royal from M. Struve, and inserted in the *Athenæum* for Feb. 20, assigns similar reasons for adopting the proposed name. We subjoin the following passage:—

"Far be it from us to have any intention of withholding our entire admiration from the eminent merit of M. Le Verrier. But impartial history will, in the future, make honourable mention also of the name of Mr. Adams, and recognise two individuals as having, independently of one another, discovered the planet beyond Uranus. In the same way, it attributes



the discovery of the infinitesimal calculus at once to Newton and to Leibnitz. Mr. Airy, the Astronomer Royal at Greenwich, has published a complete and authentic report on the labours of Mr. Adams relative to the existence of the Trans-Uranian planet. In that report, we see that in September 1845 Mr. Adams arrived at a result, and that in October he transmitted to Mr. Airy a paper containing elements of the present planet so nearly approximative that it might have been found in the heavens ten months before it actually was. But Mr. Adams's labours were unsuccessful, because the two astronomers (Mr. Challis of Cambridge and Mr. Airy of Greenwich) to whom they were known hesitated to admit them without further examination. Their doubts are explained by the importance and novelty of the object, and by the extraordinary difficulty of the research itself, which might well have been deemed beyond the powers of a young *savant* till then unknown. These doubts were accordingly not dissipated until the moment when M. Le Verrier published the results of his admirable investigations, which led to the most brilliant discovery in the astronomy of the solar system, while the other astronomers of Europe had no suspicion of the existence of Mr. Adams's labours. M. Galle of Berlin, was the first to find the planet indicated by M. Le Verrier. While we consider all these circumstances attendant on the discovery of the new planet, we at the same time conceive that we find the adhesion of M. Le Verrier to the name of Neptune, not only in his announcement to us of the 1st of October, but also in his later letters addressed to the Academy of Sciences and to two astronomers of the central observatory—letters which make no objection whatever to the name of Neptune chosen by the Bureau des Longitudes.

“Consequently, we will retain the name of Neptune; and will make no change unless hereafter the general voice shall determine in favour of another name.

“In the name of the astronomers of the Central Observatory,  
“Polkowa, 17 (29) Dec. 1846.” W. STRUVE.”

Mr. Airy adds, that he quite agrees with M. Struve in his reasons and in his conclusions.

### XXXIII. On $\sqrt{-1}$ . By H. S. WARNER, Esq.

[The following Note, appended to a communication received from Mr. Warner on the use of the symbol  $\theta$  (but not adopted for insertion), relates to a Paper signed “Shadow” in our Number for September 1846.]

**I**N relation to the article of “SHADOW,” I may observe that I did not expect that my conclusions would be admitted  
*Phil. Mag. S. 3. Vol. 30, No. 200. March 1847.* O

by those (of whom "SHADOW" seems one) who deny the correctness of the assumption upon which the argument depends; the agreement of my results with well-known formulas would be merely an additional argument in favour of the new mode of regarding imaginaries.

Opposed as his views appear to be to this manner of interpreting impossible quantities, it seems to me (unless I have mistaken the sense in which he uses the term "*incongruous*," which is the case perhaps) that they in reality support it.

Thus, incongruity in geometrical problems (and of course in those only can we consider  $\sqrt{-1}$  a symbol of perpendicularity) can only affect the *length* or the *direction* of a line: if it affected the length of a line, this would be evidenced by our obtaining such a result as the coexistence of the two equations  $A=B$ ,  $A > B$ .

When the incongruity affected the direction, it would be evidenced by the appearance of  $\sqrt{-1}$ . It being once admitted that  $\sqrt{-1}$  marks an error in estimating direction, it is easy to show that this error is one of a right angle; for the error shown by  $\sqrt{-1}$  being twice repeated, gives  $-a$  in place of  $+a$ , or an incongruity of two right angles; hence the incongruity represented by  $\sqrt{-1}$  must be half of two right angles, that is, one right angle.

Port of Spain, Trinidad,  
Oct. 3, 1846.

XXXIV. *On the Induction of Atmospheric Electricity on the Wires of the Electric Telegraph.* By Prof. JOSEPH HENRY\*.

THE action of the electricity of the atmosphere on the wires of the electrical telegraph is at the present time a subject of much importance, both on account of its practical bearing, and the number of purely scientific questions which it involves. I have accordingly given due attention to the letter referred to me, and have succeeded in collecting a number of facts in reference to the action in question. Some of these are from the observations of different persons along the principal lines, and others from my own investigations during a thunder-storm on the 19th of June, when I was so fortunate as to be present in the office of the telegraph in Philadelphia, while a series of very interesting electrical phenomena was exhibited. In connexion with the facts derived from these sources, I must ask the indulgence of the Society in frequently referring, in the course of this communication, to the results of my previous

\* From the Proceedings of the American Philosophical Society, vol. iv. p. 260.

investigations in dynamic electricity, accounts of which are to be found in the Proceedings and Transactions of this Institution.

From all the information on the subject of the action of the electricity of the atmosphere on the wires of the telegraph, it is evident that effects are produced in several different ways.

1. The wires of the telegraph are liable to be struck by a direct discharge of lightning from the clouds, and several cases of this kind have been noticed during the present season. About the 20th of May the lightning struck the elevated part of the wire, which is supported on a high mast at the place where the telegraph crosses the Hackensack river. The fluid passed along the wire each way, from the point which received the discharge, for several miles, striking off at irregular intervals down the supporting poles. At each place where the discharge to a pole took place, a number of sharp explosions were heard in succession, resembling the rapid reports of several rifles. During another storm, the wire was struck in two places in Pennsylvania, on the route between Philadelphia and New York; at one of these places twelve poles were struck, and at the other eight. In the latter case the remarkable fact was observed, that every other pole escaped the discharge; and the same phænomenon was observed, though in a less marked degree, near the Hackensack river. In some instances the lightning has been seen coursing along the wire in a stream of light; and in another case it is described as exploding from the wire at certain points, though there were no bodies in the vicinity to attract it from the conductor.

In discussing these and other facts to be mentioned hereafter, we shall, for convenience, adopt the principles and language of the theory which refers the phænomena of electricity to the action of a fluid, of which the particles repel each other, and are attracted by the particles of other matter. Although it cannot be affirmed that this theory is an actual representation of the cause of the phænomena as they are produced in nature, yet it may be asserted that it is, in the present state of science, an accurate mode of expressing the laws of electrical action, so far as they have been made out; and that though there are a number of phænomena which have not as yet been referred to this theory, there are none which are proved to be directly at variance with it.

That the wires of the telegraph should be frequently struck by a direct discharge of lightning, is not surprising, when we consider the great length of the conductor, and consequently the many points along the surface of the earth through which it must pass peculiarly liable to receive the discharge from

the heavens. Also, from the great length of the conductor, the more readily must the repulsive action of the free electricity of the cloud drive the natural electricity of the conductor to the further end of the line, thus rendering more intense the negative condition of the nearer part of the wire, and consequently increasing the attraction of the metal for the free electricity of the cloud. It is not however probable that the attraction, whatever may be its intensity, of so small a quantity of matter as that of the wire of the telegraph, can of itself produce an electrical discharge from the heavens; although, if the discharge were started by some other cause, such as the attraction of a large mass of conducting matter in the vicinity, the attraction of the wire might be sufficient to change the direction of the descending bolt, and draw it in part or whole to itself. It should also be recollected that, on account of the perfect conduction, a discharge on any part of the wire must affect every other part of the connected line, although it may be hundreds of miles in length.

That the wire should give off a discharge to a number of poles in succession, is a fact I should have expected, from my previous researches on the lateral discharge of a conductor transmitting a current of free electricity. In a paper on this subject, presented to the British Association in 1837, I showed that when electricity strikes a conductor explosively, it tends to give off sparks to all bodies in the vicinity, however intimately the conductor may be connected with the earth. In an experiment in which sparks from a small machine were thrown on the upper part of a lightning-rod, erected in accordance with the formula given by the French Institute, corresponding sparks could be drawn from every part of the rod, even from that near the ground. In a communication since made to this Society, I have succeeded in referring this phenomenon to the fact, that during the transmission of a quantity of electricity along a rod, the surface of the conductor is charged in succession, as it were, by a wave of the fluid, which, when it arrives opposite a given point, tends to give off a spark to a neighbouring body, for the same reason that the charged conductor of the machine gives off a spark under the same circumstances.

It might at first be supposed that the redundant electricity of the conductor would exhaust itself in giving off the first spark, and that a second discharge could not take place; but it should be observed, that the wave of free electricity, in its passage, is constantly attracted to the wire by the portion of the uncharged conductor which immediately precedes its position at any time; and hence but a part of the whole re-



dundant electricity is given off at one place; the velocity of transmission of the wave as it passes the neighbouring body, and its attraction for the wire, preventing a full discharge at any one place. The intensity of the successive explosions is explained by referring to the fact, that the discharge from the clouds does not generally consist of a single wave of electricity, but of a number of discharges along the same path in rapid succession, or of a continuous discharge which has an appreciable duration; and hence the wire of the telegraph is capable of transmitting an immense quantity of the fluid thus distributed over a great length of the conductor.

The remarkable facts of the explosions of the electricity into the air, and of the poles being struck in interrupted succession, find a plausible explanation in another electrical principle which I have established, namely, in all cases of the disturbance of the equilibrium of the electrical plenum, which we must suppose to exist throughout all terrestrial space, the state of rest is attained by a series of diminishing oscillations. Thus in the discharge of a Leyden jar, I have shown that the phenomena exhibited cannot be explained by merely supposing the transfer of a quantity of fluid from the inner to the outer side of the jar; but in addition to this we are obliged to admit the existence of several waves, backwards and forwards, until the equilibrium is attained. In the case of the discharge from the cloud, a wave of the natural electricity of the metal is repelled each way from the point on which the discharge falls, to either end of the wire, is then reflected, and in its reverse passage meets in succession the several waves which make up the discharge from the cloud. These waves will therefore interfere at certain points along the wire, producing, for a moment, waves of double magnitude, and will thus enhance the tendency of the fluid at these points to fly from the conductor. I do not say that the effects observed were actually produced in this way; I merely wish to convey the idea that known principles of electrical action might, under certain circumstances, lead us to anticipate such results.

2. The state of the wire may be disturbed by the conduction of a current of electricity from one portion of space to another, without the presence of a thunder-cloud; and this will happen in case of a long line, when the electrical condition of the atmosphere which surrounds the wire at one place is different from that at another. Now it is well known that a mere difference in elevation is attended with a change in the electrical state of the atmosphere. A conductor, elevated by means of a kite, gives sparks of positive electricity in a perfectly

clear day; hence if the line of the telegraph passes over an elevated mountain ridge, there will be continually, during clear weather, a current from the more elevated to the lower points of the conductor.

A current may also be produced in a long level line, by the precipitation of vapour in the form of fog at one end, while the air remains clear at the other; or by the existence of a storm of rain or snow at any point along the line, while the other parts of the wire are not subjected to the same influence.

Currents of sufficient power to set in motion the marking machine of the telegraph have been observed, which must have been produced by some of these causes. In one case the machine spontaneously began to operate without the aid of the battery, while a snow storm was falling at one end of the line, and clear weather existed at the other. On another occasion a continued stream of electricity was observed to pass between two points at a break in the wire, presenting the appearance of a gas-light almost extinguished. A constant effect of this kind indicates a constant accession of electricity at one part of the wire, and a constant discharge at the other.

3. The natural electricity of the wire of the telegraph is liable to be disturbed by the ordinary electrical induction of a distant cloud. Suppose a thunder-cloud, driven by the wind in such a direction as to cross one end of the line of the telegraph at the elevation, say of a mile; during the whole time of the approach of the cloud to the point of its path directly above the wire, the repulsion of the redundant electricity with which it is charged would constantly drive more and more of the natural electricity of the wire to the further end of the line, and would thus give rise to a current. When the cloud arrived at the point nearest to the wire, the current would cease for a moment; and as the repulsion gradually diminished by the receding of the cloud, the natural electricity of the wire would gradually return to its normal state, giving rise to a current in an opposite direction. If the cloud were driven by the wind parallel to the line of the telegraph, a current would be produced towards each end of the wire, and these would constantly vary in intensity with the different positions of the cloud. Although currents produced in this way may be too feeble to set in motion the marking apparatus, yet they may have sufficient power to influence the action of the current of the battery so as to interfere with the perfect operation of the machine.

4. Powerful electrical currents are produced in the wires of the telegraph by every flash of lightning which takes place

within many miles of the line, by the action of dynamic induction ; which differs from the action last described, in being the result of the influence of electricity *in motion* on the natural electricity of the conductor. The effect of this induction, which is the most fruitful source of disturbance, will be best illustrated by an account of some experiments of my own, presented to the Society in 1843. A copper wire was suspended by silk strings around the ceiling of an upper room, so as to form a parallelogram of about sixty feet by thirty on the sides ; and in the cellar of the same building, immediately below, another parallelogram of the same dimensions was placed. When a spark from an electrical machine was transmitted through the upper parallelogram, an induced current was developed in the lower one sufficiently powerful to magnetize needles, although two floors intervened, and the conductors were separated to the distance of thirty feet. In this experiment no electricity passed through the floors from one conductor to the other ; the effect was entirely due to the repulsive action of the electricity in motion in the upper wire on the natural electricity of the lower. In another experiment, two wires, about 400 feet long, were stretched parallel to each other between two buildings ; a spark of electricity sent through one produced a current in the other, though the two were separated to the distance of 300 feet ; and from all the experiments, it was concluded that the distance might be indefinitely increased, provided the wires were lengthened in a corresponding ratio.

That the same effect is produced by the repulsive action of the electrical discharge in the heavens, is shown by the following modification of the foregoing arrangement. One of the wires was removed, and the other so lengthened at one end as to pass into my study, and thence through a cellar window into an adjacent well. With every flash of lightning which took place in the heavens, within at least a circle of twenty miles around Princeton, needles were magnetized in the study by the induced current developed in the wire. The same effect was produced by soldering a wire to the metallic roof of the house, and passing it down into the well ; at every flash of lightning a series of currents in alternate directions was produced in the wire.

I was also led, from these results, to infer that induced currents must traverse the line of a railroad, and this I found to be the case. Sparks were seen at the breaks in the continuity of the rail, with every flash of a distant thunder-cloud.

Similar effects, but in a greater degree, must be produced on the wire of the telegraph by every discharge in the heavens ;

and the phænomena which I witnessed on the 19th of June in the telegraph office in Philadelphia, were, I am sure, of this kind. In the midst of the hurry of the transmission of the congressional intelligence from Washington to Philadelphia, and thence to New York, the apparatus began to work irregularly. The operator at each end of the line announced at the same time a storm at Washington, and another at Jersey City. The portion of the circuit of the telegraph which entered the building, and was connected with one pole of the galvanic battery, happened to pass within the distance of less than an inch of the wire which served to form the connexion of the other pole with the earth. Across this space, at an interval of every few minutes, a series of sparks in rapid succession was observed to pass; and when one of the storms arrived so near Philadelphia that the lightning could be seen, each series of sparks was found to be simultaneous with a flash in the heavens. Now we cannot suppose for a moment that the wire was actually struck at the time each flash took place; and indeed it was observed that the sparks were produced when the cloud and flash were at the distance of several miles to the east of the line of the wire. The inevitable conclusion is, that all the exhibition of electrical phænomena witnessed during the afternoon was purely the effect of induction, or the mere disturbance of the natural electricity of the wire at a distance, without any transfer of the fluid from the cloud to the apparatus.

The discharge between the two portions of the wire continued for more than an hour, when the effect became so powerful, that the superintendent, alarmed for the safety of the building, connected the long wire with the city gas-pipes, and thus transmitted the current silently to the ground. I was surprised at the quantity and intensity of the current; it is well known, that to affect a common galvanometer with ordinary electricity, requires the discharge of a large battery; but such was the quantity of the induced current exhibited on this occasion, that the needle of an ordinary vertical galvanometer, with a short wire, and apparently of little sensibility, was moved several degrees.

The pungency of the spark was also, as might have been expected, very great. When a small break was made in the circuit, and the parts joined by the fore-finger and thumb, the discharge transmitted through the hand affected the whole arm up to the shoulder. I was informed by the superintendent, that on another occasion a spark passed over the surface of the spool of wire, surrounding the legs of the horse-shoe magnet at right angles to the spires; and such was its inten-



sity and quantity, that all the wires across which it passed were melted at points in the same straight line as if they had been cut in two by a sharp knife.

The effects of the powerful discharges from the clouds may be prevented in a great degree, by erecting at intervals along the line, and aside of the supporting poles, a metallic wire, connected with the earth at the lower end, and terminating above at the distance of about half an inch from the wire of the telegraph. By this arrangement the insulation of the conductor will not be interfered with, while the greater portion of the charge will be drawn off. I think this precaution of great importance at places where the line crosses a river, and is supported on high poles; also in the vicinity of the office of the telegraph, where a discharge, falling on the wire near the station, might send a current into the house of sufficient quantity to produce serious accidents. The fate of Prof. Richman, of St. Petersburg, should be recollected, who was killed by a flash from a small wire, which entered his house from an elevated pole while he was experimenting on atmospheric electricity.

The danger, however, which has been apprehended from the electricity leaving the wire and discharging itself into a person on the road, is, I think, very small; electricity of sufficient intensity to strike a person at the distance of eight or ten feet from the wire, would, in preference, be conducted down the nearest pole. It will, however, in all cases be most prudent to keep at a proper distance from the wire during the existence of a thunder-storm in the neighbourhood.

It may be mentioned as an interesting fact, derived from two independent sources of information, that large numbers of small birds have been seen suspended by the claws from the wire of the telegraph. They had in all probability been instantaneously killed, either by a direct discharge, or an induced current from a distant cloud, while they were resting on the wire.

Though accidents to the operators, from the direct discharge, may be prevented by the method before mentioned, yet the effect on the machine cannot be entirely obviated; the residual current which escapes the discharge along the perpendicular wires, must neutralize for a moment the current of the battery, and produce irregularity of action in the apparatus.

The direct discharge from the cloud on the wire is, comparatively, not a frequent occurrence, while the dynamic inductive influence must be a source of constant disturbance

during the season of thunder-storms; and no other method presents itself to my mind at this time for obviating the effect, but that of increasing the size of the battery, and diminishing the sensibility of the magnet, so that at least the smaller induced currents may not be felt by the machine. It must be recollected that the inductive influence takes place at a distance through all bodies, conductors and non-conductors; and hence no coating that can be put upon the wire will prevent the formation of induced currents.

I think it not improbable, since the earth has been made to act the part of the return conductor, that some means will be discovered for insulating the single wire beneath the surface of the earth; the difficulty in effecting this is by no means as great as that of insulating two wires, and preventing the current striking across from one to the other. A wire buried in the earth would be protected in most cases from the effect of a direct discharge; but the inductive influence would still be exerted, though perhaps in a less degree.

The wires of the telegraph are too small and too few in number to affect, as some have supposed, the electrical condition of the atmosphere, by equalizing the quantity of the fluid in different places, and thus producing a less changeable state of the weather. The feeble currents of electricity which must be constantly passing along the wires of a long line, may, however, with proper study, be the means of discovering many interesting facts relative to the electrical state of the air over different regions.

### XXXV. *The Form of the Earth no proof of Original Fluidity.*

*By HERBERT SPENCER\*.*

**I**T has been generally considered that the spheroidal form of the earth—indicating as it does obedience to centrifugal force—implies a primary state of fluidity. If however it can be shown that, notwithstanding its apparent solidity, the earth must be at the present moment entirely subject to the influences affecting its general figure, and that so far as the gravitative and centrifugal forces are concerned it is plastic still, the theory of original fluidity, however probable on other grounds, can no longer be inferred from the earth's oblateness.

The facts indicative of a varying relationship between the bulk and tenacity of matter are of every-day observation. We constantly see a drop of water maintain its sphericity in spite of opposing forces—increase the mass, and it flows out in com-

\* Communicated by the Author.

plete obedience to them. The mud in our streets stands in ridges behind the passing cart-wheel—when scraped together it appears liquid and assumes a horizontal surface. On the spade of the excavator, clay retains its square figure and its sharp angles; but when made into a bulky embankment, it will, if the slope be insufficient, spread itself out on one or both sides of the base, occasionally continuing to slip until it assumes an inclination of six to one.

A comparison of the physical powers of large and small animals exhibits a series of facts of an analogous character. A flea jumps several hundred times its own length, and is uninjured by collision with any obstacle. The great mammals, on the other hand, seem to possess no agility whatever; and a concussion borne by the insect with impunity would smash an elephant to a jelly. Between these extremes may be observed a gradation in the ratios of power to bulk; so that commencing with the smaller creatures, every increment of size is, *cæteris paribus*, accompanied by an under-proportionate increase of strength, until we arrive at that limit (to which the elephant has evidently approximated) where the creature is no longer capable of supporting its own framework.

These and innumerable like facts point to the inference that fluidity and solidity are to a great extent qualities of degree; that the cohesive tenacity of any piece of matter bears, as the mass of that matter is increased, a constantly decreasing ratio to the natural forces tending to the fracture of that matter; and that hence any substance, however solid to our perceptions, only requires to have its bulk increased to a certain point, to give way, and become in a sense *fluid* before the gravitative and other forces.

However repugnant to that “common sense” for which some have so great a respect, this proposition is capable of a very simple demonstration.

The strength of a bar of iron, timber, or other material subjected to the transverse strain varies as  $\frac{BD^2}{L}$ ; B being the breadth, D the depth, and L the length. Suppose the size of this bar to be changed, whilst the ratios of its dimensions continue the same; then as the fraction  $\frac{B}{L}$  will remain constant the strength will vary as  $D^2$ , or (since D bears always the same proportion to B and L) as  $B^2$  or  $L^2$ . Hence in similar masses of matter the resistances to the transverse strain are as the squares of the linear dimensions. The same law still more manifestly applies to the longitudinal strain, when

the strength, depending as it does on the sectional area, must, in similar masses, vary as the square of any side. And in the torsion strain we may readily detect the like general principle, that, other things equal, the resistances to fracture bear a constant ratio to the squares of the dimensions.

Not so however with the powers tending to the disruption of matter. The effects of gravity, centrifugal force, and all agencies antagonistic to cohesive attraction, vary as the mass, that is, as the cubes of the dimensions.

However great, therefore, in a given portion of matter, may be the excess of the form-preserving force over the form-destroying force, it is clear that, if during augmentation of bulk the form-preserving force increases only as the *squares* of the dimensions, whilst the form-destroying force increases as their *cubes*, the first must in time be overtaken and exceeded by the last; and when this occurs, the matter will be fractured and re-arranged in obedience to the form-destroying force.

Viewed by the light of this principle, the fact that the earth is an oblate spheroid does not seem to afford any support to the hypothesis of original fluidity as commonly understood. We must consider that, in respect of its obedience to the geodynamic laws, the earth is fluid now and must always remain so; for the most tenacious substance with which we are acquainted, when subjected to the same forces that are acting upon the earth's crust, would exceed the limit of self-support determined by the above law, before it attained  $\frac{1}{1,000,000,000}$ th of the earth's bulk.

Reference to a table of the resistances of various substances to a crushing force will render this manifest.

London, Jan. 1847.

### XXXVI. *Proceedings of Learned Societies.*

#### ROYAL SOCIETY.

[Continued from p. 128.]

*Anniversary Meeting, November 30, 1846.*

THE Marquis of Northampton in the Chair.

The noble President stated that the Council had awarded one of its Medals to M. Le Verrier; two Medals to Mr. Faraday, for his discoveries in the universal action of Electricity and Galvanism; and one of the Royal Medals to Prof. Owen, for his paper on the Belemnite.

After presenting the Medals, the President proceeded to the biographical notices of some of the deceased members, from which we select the following:—

JOHN BOSTOCK, M.D., has long occupied a distinguished station



among the zealous cultivators of Animal Chemistry, Physiology, and other branches of Medical Science. His father had been established as physician at Liverpool, and from his great talents would probably have risen to considerable eminence, had not these brilliant prospects been doomed to sudden extinction by an untimely death, before he had attained the age of thirty. He left an only child, the subject of the present notice, who was born in 1774, the year preceding that fatal event, and whose early education was conducted at the New College at Hackney, at the time when Dr. Priestley was delivering lectures on chemistry. Having imbibed, under such able tuition, an ardent love of science, young Bostock was led to make choice of medicine as his profession. He studied at Edinburgh, where he graduated in the year 1798, and soon afterwards commenced practice in his native town. The activity of his mind was there displayed, not only in numerous contributions to most of the medical and scientific journals, but also in the prominent part which he took in planning and establishing various charitable, scientific and literary institutions, and more particularly the Fever Hospital, and the Botanic Garden; and also the Philosophical and Literary Institution of Liverpool, where, in the capacity of Professor of Physiology, he delivered the first course of lectures there given.

Having secured a competent fortune, Dr. Bostock determined, in 1817, to relinquish his profession and fix his residence in London, where he could possess more extensive means of prosecuting his favourite studies, and enjoy a more enlarged society of scientific friends. He soon became an associate of most of the scientific societies of the metropolis, and an active labourer in their management. In 1818, he was elected a Fellow of the Royal Society; was several times placed on its Councils, and in 1832, filled the office of Vice-President. He was for some years Secretary of the Geological Society, and in 1826 was appointed its President. He long held the office of Treasurer of the Medical and Chirurgical Society, and took an active share in the management of the Fever Hospital, not only as a member of its Committee, but also as one of its House Directors. He was also, during a long period, one of the lecturers on Chemistry at the Medical School of Guy's Hospital, being appointed to that office on the death of his friend Dr. Marcet, in 1822.

Amidst these multiplied public avocations, he found leisure for the accomplishment of a great variety of literary and scientific labours, the aggregate amount of which would appear astonishing to any one who was not acquainted with his methodical habits, his persevering industry, and his advantageous employment of every portion of his time. His contributions to medical and scientific journals, transactions of societies and cyclopædias, amount to no less than sixty-nine; of which twenty are contained in Nicholson's *Journal and Annals of Philosophy*, eighteen in the *Medico-Chirurgical Transactions*, and twelve in the *Cyclopædias of Practical Medicine*, and of *Anatomy and Physiology*. Only one paper by him appears in the *Philosophical Transactions* (in 1829), namely, that "On the spontaneous purification of the Thames Water," recording the observa-

tions he made in the course of some analyses which he undertook at the request of the Commissioners appointed by the Crown to inquire into the supply of water to the metropolis.

Some of the more finished papers and essays which had appeared in these works, were afterwards republished by him in a separate form. Among these are his "Account of the History and present state of Galvanism," originally published in Brewster's *Cyclopædia*, and which appeared in 1818; and his "History of Medicine," which had been prefixed to the *Cyclopædia of Practical Medicine*. One of his earliest publications was an "Essay on Respiration;" his attachment to chemical pursuits having naturally led him to the particular study of this part of the animal economy. At a later period, he engaged in the compilation of a general work, embracing the whole subject of physiology, which he completed in three volumes, the last of which appeared in 1827, under the title of "*Elementary System of Physiology*;" the third and last edition, published in 1837, comprised the whole in one thick octavo volume of nearly 900 pages. It is a work of immense labour and research, containing condensed and elaborate analyses of all that had at that time been published, both as to facts and theories, in the wide field of physiology. It constitutes, in fact, an *Encyclopædia* of this department of medical science, where the student will find indicated, with scrupulous exactness, the authorities for every statement, and the sources which may supply him with whatever further information he might require on any particular subject. After he had completed this work, he projected a new translation of Pliny's "*Natural History*," to be accompanied with notes; in 1828 he printed, for private distribution, a specimen of the work, consisting of the first and thirty-third books; and he afterwards devoted a considerable portion of his time to the prosecution of this undertaking, in which he had made some progress at the time of his death. For the last two or three years his health had been declining, but the immediate cause of his death was an attack of cholera, which proved fatal on the 6th of August in the present year.

Respected and beloved by a wide circle of friends and relatives, his memory will long be cherished with affection by those who survive, and with gratitude by the votaries of those sciences which his labours have promoted and enriched.

JOHN CONSTANTINE CARPUE, a distinguished teacher of Anatomy and Surgery.

JOHN THOMSON, M.D., one of the ablest representatives of the last generation of medical men, was Professor of General Pathology in the University of Edinburgh, and died on the 11th of October last, at his residence in the vicinity of that city, at the advanced age of eighty-two years.

He was born in the town of Paisley in Renfrewshire, and in overcoming the impediments of an humble station, of straitened circumstances, and of a defective education, he early exhibited those vigorous intellectual powers which were afterwards so successfully exerted in the acquirement of information and the promotion of science.

His innate love of knowledge soon became apparent by the strenuous application of his mind, in succession, to various important objects of scientific interest. While yet a youth, and during the short intervals of leisure, stolen from the servile drudgery of a medical apprenticeship in his native town, he devoted himself with such energy and success to the study of Botany, as ever after to retain a lively interest in that pursuit. Having completed his medical education in the Universities of Glasgow and Edinburgh, and in the medical schools of London, he settled, in the concluding years of the century, as a general practitioner in Edinburgh. He there delivered lectures on Chemistry, and published a translation of Fourcroy's *Elements of that science*, accompanied with valuable notes.

Being appointed one of the Surgeons of the Royal Infirmary in the year 1800, he commenced his labours as a Teacher of Surgery, and on the institution, at his recommendation, of a Professorship of Surgery in the Royal College of Surgeons, he was appointed to that office. It was on his representation that the injurious system which then prevailed, of changing the surgeons at short intervals, was abandoned, and the tenure of that office rendered one of reasonable duration; and that the delivery of Clinical lectures by the surgeons in office, of all modes of instruction the most valuable, was commenced. To his counsel the College of Surgeons were indebted for the foundation of a Museum of Anatomy, both healthy and morbid, which, enriched as it has since been by many valuable collections, now ranks second only to that of the Royal College of Surgeons of England.

For a period of sixteen years Mr. Thomson delivered the lectures on Surgery in the Hall of the College to crowded auditories of students and practitioners. In the year 1806 he was appointed by the crown Professor of Military Surgery in the University of Edinburgh, a chair which had been newly created by the government of that period.

The publication on which his permanent reputation rests bears the title of "*Lectures on Inflammation.*" It appeared in 1813, and was in effect a revival and masterly exposition of the views and doctrines of John Hunter, which, partly from a deficiency of perspicuity in that great man's style, and partly from the small degree of attention which they had excited among the profession, had never before obtained their due influence, nor had their truths been sufficiently recognised and established. It is a work which exhibits the results of acute discrimination, unwearied ardour, persevering research, and a clear and careful method of argumentation. It has been translated into many foreign European languages, and speedily became the standard authority on that important subject, constituting as it does the basis of all Pathology.

On the death of Dr. Gregory in 1821, Dr. Thomson resigned his surgical appointments, and having formerly graduated at King's College, Aberdeen, now joined the College of Physicians and practised as consulting physician, commencing at the same time a course of lectures on the practice of physic. This may be regarded as the



second remarkable period of his professional career. The novel and distinguishing feature of this course of lectures consisted in its being founded on pathological anatomy, or the structural changes induced by disease in the several tissues of the body, as auxiliary to the ancient system of pure symptomatology. To accomplish this object in the most effectual manner, Dr. Thomson had recourse to the aid of the draughtsman, and presented to the eye of the student accurate coloured delineations of morbid appearances, arranged on anatomico-physiological principles, and accompanied by their corresponding histories. The result of this spirited enterprise, prosecuted both abroad and at home during a period of several years, and at a very considerable pecuniary sacrifice, has been a splendid collection of anatomical drawings, surpassing all others at present existing in richness and extent.

In 1832, a Chair of General Pathology was added to the University of Edinburgh, to which endowment the suggestions and the evidence of Dr. Thomson before a commission appointed by Lord Liverpool's government in 1826, materially contributed. The appointment of first professor was conferred upon himself; but the pressure of advanced years and infirm health unfortunately compelled his retirement after he had held it for four years.

At various times Dr. Thomson visited the British metropolis and the continent of Europe, chiefly for the purpose of personally examining their pathological collections; and after the battle of Waterloo he repaired to Brussels, in order to avail himself of the advantages which were afforded on that occasion of examining the nature and treatment of gun-shot wounds and field injuries. The result of his observations he shortly afterwards published in a "Report of the wounded at Waterloo."

To Dr. Thomson's spirit of independence, not less than to his ardent love of knowledge, the school of Edinburgh is specially indebted; for in addition to his active exertions in the foundation of Professorships, of a museum and of a library, and the establishment of a dispensary for visiting the poor in their own dwellings, he was ever watchful of its interests, and as fearless in denouncing usages which he considered prejudicial as he was prompt in suggesting and earnest in promoting beneficial changes and reforms. To the jealousies which are ever engendered by merit, and by attempts at reform, and to the opposition he encountered from the magisterial authorities of the city, may be ascribed his failure of success when a candidate for the vacant chair of Dr. Gregory, although he was supported by overwhelming testimonials of superior claims from the most eminent medical men throughout Europe; and the same cause, together with the liberal bias of his political views and opinions, (for though he did not obtrude he never sought to conceal them) will also explain the anomaly, that although no professional opinion was more highly esteemed than his, yet he never attained to extraordinary popularity in practice. Some of the prejudices he had to contend against were the fruits of his scrupulous honesty, of the simplicity of his mind and manners, and of his hostility to every species



of professional charlatanism in whatever quarter it was displayed. It can excite no surprise that he stood high in the estimation of his many celebrated contemporaries of the Scottish metropolis, and that he should have enjoyed a cordial and continued intimacy with such men as Stewart, Allen and Mackintosh. Associated with the projectors of the Edinburgh Review, he was a contributor to some of the early numbers, of articles connected with medical philosophy. In other researches of science not strictly professional, Dr. Thomson evinced an erudition of greater extent and profundity than could have been attained by any individual not possessing his quickness of apprehension and unquenchable thirst for knowledge.

The profession at large are perhaps scarcely aware of the extent of their obligations to Dr. Thomson. Some however yet survive who can attest the peculiar interest he inspired in his pupils by his earnestness as a teacher, and the enthusiasm they imbibed from his precepts and cherished by his example. Unostentatious in his search after truth, he was neither so frequent or voluminous an avowed contributor to the common stock, as many men of much inferior talent; but he was continually engaged in fostering the spirit and directing the pursuits of others, of whom some have largely contributed, in their riper years, to extend the boundaries of medical and surgical knowledge.

The biography of Dr. Cullen, whose character he held in high veneration, occupied the years of his retirement. The vigour of his faculties remained unimpaired to the last, and he contemplated with calm serenity the approaching end of a life devoted to the cultivation and improvement of the profession he had chosen, and of which he was a distinguished ornament.

Astronomy has sustained a heavy loss, in the last year, by the death of BESSEL. FRIEDRICH WILHELM BESSEL was born at Minden, on the 22nd of July 1784. His father held a civil office under the Prussian government, with the title of *Justizrath*, but his means were narrow and his family numerous; and at the age of fifteen the future astronomer began his career as an assistant or apprentice in a mercantile house in Bremen. Having acquired a taste for Astronomy and Mathematics, he assiduously devoted his leisure time to the study of those sciences; and his progress was such that he soon attracted the notice and obtained the friendship of Dr. Olbers, then in the zenith of his fame. His first published essay, which appeared in Zach's '*Monatliche Correspondenz*' in 1804, was a reduction of the observations made by Harriot and Torporley of the comet of 1607. In communicating this paper to Zach, Olbers eulogised, in the warmest terms, the acquirements and industry of his young friend, and expressed his regret that so much talent and zeal, and such powers of calculation, should not have been acquired for the exclusive service of astronomy. An opportunity was soon found of making the desired acquisition; Bessel, in 1806, becoming successor to Harding in the office of assistant to Schroeter, in the Observatory of Lilienthal. In this congenial situation he rapidly acquired for himself a great reputation; and

*Phil. Mag.* S. 3. Vol. 30. No. 200. March 1847. P

indeed, almost from the first, took his place as one of the best astronomers of Europe. In 1810, he was appointed professor of astronomy in Königsberg; a post which he occupied during the remainder of his life, and over which his genius and labours have thrown an imperishable lustre.

The Observatory of Königsberg, now so celebrated, owes not only its celebrity, but its existence to Bessel. The building was begun in 1811 under his direction. It was finished in 1813; and the first published observations are dated in November of the same year. From 1815 the observations have been published regularly; accompanied by full descriptions of the different instruments successively employed, the elements of reduction, some valuable catalogues, and all the information necessary to inspire complete confidence in the results. This publication has exercised a powerful influence on the progress of practical astronomy.

Bessel's scientific life is one of extraordinary fertility, and it is only a few of the more important of his productions which can even be alluded to in this brief notice. His principal work, the '*Fundamenta Astronomiæ*,' is peculiarly interesting to English astronomers, from the circumstance of its being founded on observations made at the Royal Observatory at Greenwich. Bradley's observations, as is well known, were taken possession of by his representatives, and presented by them to the University of Oxford. From various causes, their publication was not completed until 1805, forty-three years after the death of Bradley; and although some results and some imperfect catalogues had been deduced from them, the great mass of the observations still remained in a state in which they were nearly useless. At the instigation of Olbers, Bessel undertook the formidable task of reducing the whole series. The reduction was commenced in the autumn of 1807, and, though frequently interrupted by his other avocations and duties, it was never wholly laid aside till its completion in 1818, in which year the '*Fundamenta*' made its appearance. Of this great work it would be difficult to speak in terms of too high praise. Besides elaborate determinations of all the principal elements of the reduction,—the errors of the instruments, the height of the pole, refraction, parallax, aberration, precession, proper motion,—it contains a catalogue of the mean places of 3222 fixed stars, observed between 1750 and 1762 with the best instruments in existence at that time; and reduced to the epoch of 1755 with a precision and accuracy of which there was no previous example. It now furnishes astronomers with the best existing means of determining all those data which can only be deduced from a comparison of observations made at considerably distant intervals of time, and may be considered, in fact, as having laid the foundations of the principal improvements which have been made in astronomy since the date of its publication.

The '*Tabulæ Regiomontanæ*,' published in 1830, may be regarded in some respects as a supplement to the '*Fundamenta*.' This collection was undertaken with a view to facilitate the reduction of planetary observations, past and future, on a uniform system;

and it contains all the tables necessary for that purpose as well as for ordinary star-reduction, for the century to which it applies, founded on elements derived from the comparison of Bradley's with Bessel's own observations, together with the formulæ for computation. It is in daily use in every observatory, and has led to the general adoption of a uniform and precise system of astronomical calculation which has been attended with signal advantage to the science.

The services rendered to astronomy by Bessel were not confined to the observatory and the closet. In the course of a series of experiments, undertaken in 1826, for the determination of the length of the seconds' pendulum; he detected and demonstrated the existence of a cause of error in the usual reduction to a vacuum, which, though it had long before been clearly pointed out by Du Buat, appears to have been entirely overlooked and forgotten. This consists in the fact that every oscillating body *drags* along with it a certain portion of air, or of the fluid in which it moves, so that the specific gravity of the actually moving mass is different from that of the body at rest; and as the quantity of *dragged* air depends on the form of the moving body, the specific gravity cannot be determined *à priori*, but must be found by experiment for each particular pendulum. It followed, therefore, that the results of all the previous pendulum experiments were erroneous to the extent occasioned by the neglect of this circumstance. Bessel's method of conducting the experiment was extremely ingenious; and it may be asserted that his determination of this important astronomical element—the absolute length of the seconds' pendulum—is the only one yet given which is deserving of full confidence. The details and results were published in the Berlin Memoirs in 1828.

Another very important result of the indefatigable activity of Bessel, is the 'Gradmessung in Ost-Preussen,' published in 1838. A series of geodetical measurements, comprehending the measure of an arc of the meridian of the Dorpat Observatory, had been executed by Struve and Von Tenner in Russia, and it was of great importance, in reference to the question of the figure of the earth, to connect them with those of the west and south of Europe. The operations necessary for this purpose were undertaken by the Prussian government in 1830, and executed under the direction of Bessel, who seized on the opportunity of measuring an arc of the meridian of his Observatory. In consequence of the novel methods employed both in the geodetical and astronomical parts of this operation the details are of extreme interest, more particularly with reference to the ingenious apparatus for measuring the base line, the mode of observing and connecting the terrestrial angles, and the application of a method of computation by which every geometrical relation subsisting among the angles over the whole triangulated surface is expressed by equations of condition, and the results, with their probable errors, deduced by the method of least squares. The 'Gradmessung' may be described as having done for geodesy what the 'Fundamenta' did for astronomy; both works exhibiting the application of the best and surest methods of deducing results from ob-

servations which science has yet placed within the reach of the computer.

Several of his minor papers on the subject of geodesy, published in the '*Astronomische Nachrichten*,' are of great interest and value. He recomputed the French triangulation between Montjouy and Formentera (No. 438), and the sector observations of the English and Indian arcs measured by Mudge and Lambton (334, 336); and from the whole of the meridional arcs hitherto measured with admissible precision, deduced elements of the terrestrial spheroid which give probably the nearest approximation which has yet been made to the true form and magnitude of the earth.

In connexion with the measurement of the pendulum and arc of meridian, he also undertook a comparison of the standard measures of Prussia; determined their relation to the unit of the French measures; and directed the execution of a new standard which will probably become an object of great interest in some future age. The details of these delicate and difficult comparisons are described by him in an interesting work published by order of the Prussian government in 1839.

The question of the annual parallax of the fixed stars,—a vexed question in astronomy since the days of Bradley,—has of late years acquired an extraordinary interest through the labours of Struve, Henderson, and Bessel. Having previously made some unsuccessful attempts, Bessel at length determined to attack the problem in a new way, and to subject one individual star to a scrutiny which would infallibly decide whether any appreciable parallax existed in that particular case. The star which he selected was 61 *Cygni*, a binary system whose two members had been proved to be physically connected, and which, by reason of the very considerable distance between the component bodies, and its large proper motion, afforded strong presumptions of its being one of the nearest to the earth. With the splendid heliometer of the Königsberg Observatory, an instrument peculiarly adapted for such a purpose, he commenced a series of micrometrical measurements of the distances of the star from two others in its vicinity, which were continued from August 1837 to March 1840, and conducted with all the appliances and precautions for attaining extreme accuracy which were to be expected from the importance of the inquiry and the consummate skill of the observer. The results exhibited a periodic and systematic fluctuation of the places of the double star, agreeing exactly in its law and period with the effects of parallax, and referable to no other known cause. The parallax thus indicated was indeed an extremely minute quantity, amounting only to 31-100ths of a second; but no one in a condition to appreciate the evidence on which it rests will doubt the possibility of detecting such a quantity by such means; and astronomers, accordingly, regard the conclusion as one which, if not absolutely certain, is at least so extremely probable, that it must command assent until disproved by another equally good, and longer continued, series of observations.

The fruits of the extreme precision which Bessel so greatly con-



tributed to introduce into astronomical observations and reductions, have already begun to make their appearance. In an interesting disquisition published in some recent numbers of the '*Astronomische Nachrichten*' (514, 515, 516), he seems to have established the existence of a very remarkable phenomenon which had been indicated, at different times, by Pond, namely, the variability of the proper motion of two of the principal stars, *Procyon* and *Sirius*. It is unnecessary to point to the boundless field of speculation respecting the constitution of the sidereal heavens which such a discovery lays open.

The annals of astronomy afford many illustrious examples of indefatigable industry and prolific labour, but few more remarkable than Bessel. From the date of his first publication in 1804, the astronomical periodicals of Germany teem with his productions. Zach's '*Monatliche Correspondenz*,' Lindenau's '*Zeitschrift*,' Bode's '*Jahrbuch*,' the '*Berlin Ephemeris*,' the '*Memoirs of the Berlin Academy*,' and, more especially, Schumacher's '*Astronomische Nachrichten*,' have all been enriched by his communications. The last-named journal, indeed, contains upwards of 180 bearing his signature, embracing almost every subject connected with the science and practice of astronomy, and questions of every order of difficulty. Some of his papers in the '*Berlin Memoirs*' are extremely remarkable, not only by reason of the results arrived at, but as showing the resources and originality of the author, and his skill in the highest departments of analysis and physical astronomy. Among these may be mentioned, in particular, his researches on the great comet of 1807 (published in 1810), in which he proposed a method, which is now generally followed, of determining the orbit when it is necessary to take the perturbations into account; a most important memoir on the precession of the equinoxes (1815); another on the planetary perturbations (1824); and three on the right ascensions of Maskelyne's 36 stars (1818, 1819, 1825). Among his later productions are two quarto volumes of '*Untersuchungen*' (or Astronomical Researches), and a third is said to have been nearly prepared for the press. Many other titles might be cited, but those now mentioned may suffice to give an idea of the wide extent over which he ranged. Indeed it may be said of Bessel, that there is no department of astronomy which he did not handle, and that nothing passed through his hands without receiving extension or improvement. But, perhaps, that which stands out as most remarkable in his writings is his profound knowledge of the theory of instruments; and perhaps also the influence of his example in teaching observers to trust less to the mechanical perfection of instruments, and to bestow more pains in determining their errors so as to remove them by numerical corrections, have contributed as much to the advancement of astronomical science as his own proper labours and discoveries. His countrymen adopted his methods as the models of their practice; the present state of German astronomy shows with how much advantage.

Bessel was elected a Foreign Member of this Society in 1825. He died at Königsberg on the 17th of March 1846, after a long and

painful illness occasioned by an internal disease. The history of his labours will occupy a large and prominent place in the history of astronomy during the first half of the present century.

The BARON DE DAMOISEAU was one of the most distinguished astronomers of the age. His most considerable work was his "*Mémoire sur la Théorie de la Lune*," which was presented to the Institute in 1821, but not published before 1827, when it appeared in the "*Mémoires des Savans Etrangers*." The methods which are used in this important memoir are, generally speaking, the same as those adopted by Laplace: the moon's true longitude being assumed as the independent variable, and the final equations solved by the method of indeterminate coefficients; the solutions being given in numerical and not in literal coefficients, as in the great work of Plane on the same subject. The approximations, also, are carried to a greater extent than in the "*Mécanique Céleste*." This memoir was followed by the celebrated "*Tables of the Moon*," which were founded upon it, and which appeared in 1824 under the title "*Tables de la Lune, formée sur la seule Théorie de l'Attraction et suivant la division du cercle en 400 degrés*." They form the first, and indeed the only expanded tables of the moon which are founded entirely upon theory, borrowing nothing whatever from observation but the simple elliptic elements, the proportion of distances of the sun and moon, and the masses. All preceding tables, such as Mayer's, Borg's and Burckhardt's, had derived many of their coefficients empirically from observation. These tables are the basis of those which are used by the present Astronomer Royal in the great lunar reductions which are now in progress under his superintendence.

The Baron de Damoiseau was also the author of "*Tables of the Satellites of Jupiter*," and of many other works and memoirs connected with the advancement of astronomical science: he was a very profound analyst, a most laborious and faithful calculator, and the author of the most important advancements which the lunar theory received in the period which intervened between the appearance of the great works of Laplace and Plane to which I have before referred.

Jan. 28, 1847.—"On the Lunar Atmospheric Tide at St. Helena." By Lieut.-Colonel Edward Sabine, R.A., For. Sec. R.S.

The results of the observations made by Captain Lefroy, of the Royal Artillery, Director of the Magnetical and Meteorological Observatory at St. Helena, are here given; from which it appears, on the examination of the barometrical changes during seventeen months, that a maximum of pressure corresponds to the moon's passage over both the inferior and superior meridians, being slightly greater in the latter case, and that a minimum corresponds nearly to the rising and setting, or to six hours before and after the former periods. The average atmospheric pressures are 28.2714 inches in the first case, and 28.2675 in the last; the difference being 0.0039 inch. The height of the cistern of the barometer above the sea is 1764 feet; and the latitude of the Observatory  $15^{\circ} 57'$  S. These results were still further confirmed by those of a series of observations during

two years. These observations also establish the conclusion that the moon exerts a greater influence on the amount of atmospheric pressure at the periods of her perigee than at those of her apogee.

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ROYAL ASTRONOMICAL SOCIETY.

[Continued from p. 139.]

Jan. 8, 1847.—Reduction of the Observations of Halley's Comet, made at the Cambridge Observatory in the years 1835 and 1836. By the Astronomer Royal.

These observations were chiefly made with the 5-foot equatoreal. They were reduced as far as was practicable at the time, and the corrections for refraction and parallax applied. In this state they were published in the Cambridge Observations for 1835.

The instrumental errors, though pretty well known, and known to be small, were not then corrected. It was thought better to put off this part of the complete reduction until correct places of the small stars of comparison could be obtained. In 1836 and 1837 most of these stars were observed at Greenwich, and they are sufficiently numerous to establish all the results which can be drawn from the observations of the comet. Some stars could not be seen, though carefully looked for.

In exhibiting the results two methods have been followed. First, the place of the comet has been determined *differentially* with respect to small stars, which have themselves been subsequently determined. Secondly, the error of the instrument has been computed from the observations of known stars, admitting of accurate observation, and the error so deduced has been applied to correct the instrumental observation of the comet. A comparison of these results will often show which is to be preferred.

The final results are compared with the Ephemeris circulated by the Superintendent of the Nautical Almanac, under the date Dec. 30, 1835, which is the same, as to places unaffected by perturbation, with the Ephemeris in the Appendix to the Nautical Almanac of 1839. In this Ephemeris the effects of aberration are fully included, contrary to the supposition of the Astronomer Royal in the Cambridge Observations, 1835, p. lxiv.

It has not been thought advisable to deduce the elements of Halley's comet from these observations. That task will be best performed by some future Rosenberger, who, with all the observations from both hemispheres before him, may hope to bring a satisfactory result out of the whole mass.

The work is divided into sections :—

1. A catalogue of stars of comparison.
2. Errors of Ephemeris in R.A. from differential observations only.
3. Computation of the index errors of the equatoreal in R.A.
4. Errors of the Ephemeris in R.A. from the data of the previous section.

5. Errors of the Ephemeris in R.A. from differential transits at the mural circle.

6. Errors of the Ephemeris in N.P.D. from differential observations only.

7. Computation of the index errors in N.P.D. of the equatoreal.

8. Errors of the Ephemeris in N.P.D. from the data of the last section.

9. A tabular collection of all the previous results, with remarks.

On a proposed Alteration of Bessel's Method for the Computation of the Corrections by which the Apparent Places of Stars are derived from the Mean Place. By the Astronomer Royal.

After mentioning the great superiority in uniformity and simplicity of Bessel's method over those previously in existence, the Astronomer Royal remarks, that the strict attention to *sign* required both in the partial additions and in collecting the sum is exceedingly troublesome, and that more errors in observatory business arise from oversight as to *sign* than from any other cause. He has therefore been led to consider the possibility of avoiding changes of sign, and suggests the following method as probably an improvement in giving the data of the *corrections* in the Ephemeris and Catalogue.

On examining the *maximum* values of the quantities A, B, C, D in the Nautical Almanac, and of  $a, b, c, d, a', b', c', d'$  in the British Association Catalogue, it will be seen that A, B, and D can never be equal to 25, that  $c'$  is always less than 25, and that up to a north polar distance of  $3^\circ 45'$ ,  $c$  must also be less than 25. All the other numbers are less than 1.2.

$$\begin{array}{lll} \text{Let } E=A+25 & \text{Let } e=a+1.2 & \text{Let } e'=a'+1.2 \\ F=B+25 & f=b+1.2 & f'=b'+1.2 \\ G=C+1.2 & g=c+25 & g'=c'+25 \\ H=D+25 & h=d+1.2 & h'=d'+1.2 \end{array}$$

All the introduced symbols are necessarily positive.

Arranging and multiplying

$$\begin{array}{l} Aa=Ec-1.2E-25e+30 \\ Bb=Ff-1.2F-25f+30 \\ \text{\&c.} \qquad \qquad \text{\&c.} \end{array}$$

And

$$\begin{array}{l} Aa'=Ee'-1.2E-25e'+30 \\ \text{\&c.} \qquad \qquad \text{\&c.} \end{array}$$

Hence, collecting and arranging, it will be found that the sums of the corrections in R.A. and in north polar distance will be respectively,—

$$\begin{aligned} \text{In R.A.} &= 120.0 + Ec + Ff + Gg + Hh \\ &\quad - (1.2E + 1.2F + 25G + 1.2H) \\ &\quad - (25e + 25f + 1.2g + 25h) \end{aligned}$$

$$\begin{aligned} \text{In N.P.D.} &= 120.0 + Ee' + Ff' + Gg' + Hh' \\ &\quad - (1.2E + 1.2F + 25G + 1.2H) \\ &\quad - (25e' + 25f' + 1.2g' + 25h') \end{aligned}$$

where the numbers are seconds of time in the first group, and seconds of space in the latter.



It will be seen that the second line in each group is identical, and depends solely on E, F, &c.; that is on A, B, C, and D. This therefore could be tabulated in the *Ephemeris* for the day, under one symbol K. The third lines depend on  $a, b, \&c.$  and  $a', b', \&c.$ ; and therefore admit of tabulation in the *Catalogue* as  $k$  and  $k'$  respectively for each star. The *corrections* could now be in this form:—

$$\text{In R.A.} = 120^{\circ}.0 + Ee + Ff + Gg + Hh - (K + k)$$

$$\text{In N.P.D.} = 120^{\circ}.0 + Ee' + Ff' + Gg' + Hh' - (K + k').$$

The greatest possible value of K is about 182, and the greatest values of  $k$  and  $k'$  are about 170. Let

$$L = 190 - K, l = 170 - k, l' = 170 - k';$$

then substituting these values in the last equations, the corrections are,

$$\text{In R.A.} = Ee + Ff + Gg + Hh + L + l - 240^{\circ}.00$$

$$\text{In N.P.D.} = Ee' + Ff' + Gg' + Hh' + L + l' - 240^{\circ}.0$$

in which every symbol is necessarily positive.

The Astronomer Royal finds that logarithms with five decimal places would be wanted for the corrections in R.A., and that four would be sufficient in N.P.D.

An additional column would thus be required for the *Ephemeris* and two columns for the *Star Catalogue*, but there would be no sign employed either in the tabulation or computation. The trouble of deducing E, F, &c.,  $e, f, \&c.$ , L, l, &c. is trifling, and might be made still easier by subsidiary tables. The change proposed is not recommended for uncatalogued stars observed only once. Stars which are nearer the pole than  $3^{\circ} 45'$  require special treatment.

Orbits of Double Stars. Computed by Capt. W. S. Jacob, B.E.

These orbits have been computed from all the observations accessible to the author, including his own for the current year. The orbit of Castor has only been roughly deduced, as the observations are not yet sufficient to define the elements with precision. It was undertaken by Captain Jacob for the purpose of ascertaining what alteration was required in Mr. Hind's elements to make them represent his own observations, and those of Captain Smyth in 1838 and 1843.

In computing the orbits of the other stars, the method of least squares has been employed to correct the elements which were obtained in Sir J. Herschel's manner.

The stars which Captain Jacob has computed are Castor,  $\xi$  Ursæ,  $\gamma$  Virginis, and 70 Ophiuchi.

Notice respecting a pair of Chinese Planispheres brought from Chusan, and presented to the Society by Captain Sir E. Home, Bart. R.N. By Mr. Woolgar.

The hemispheres are of twenty-five inches internal diameter, projected stereographically on the plane of the ecliptic. The magnitudes of the stars are represented *conventionally* by a method which makes a star of the first magnitude less conspicuous than one of the third or fourth. There are six magnitudes. The principal stars are

connected by right lines. The groups thus formed sometimes do, and sometimes do not, agree with those found in some European maps.

The map is executed coarsely by block-printing. The positions and magnitudes are incorrect. There is no appearance of European origin. The selection of stars of the fifth and sixth magnitude could not well have been copied, and some stars are inserted which are not to be found in any common catalogue or map. The epoch seems to be about A.D. 1735.

**A Historical Survey of Comets.** By Dr. Michelsen.

The author commences with a general review of the early history of cometary astronomy, and notices the uncertainty attaching to ancient European accounts of comets: the information given us by the Chinese annalists, Ma-tuon-lin, as sketched by Mailla, Gaubil and De Guignes, presenting a more definite aspect. He remarks that the comet which appeared in the time of Anaxagoras may be considered the first established historically, though as yet unconfirmed by any astronomical calculation.

The author then proceeds to give a detailed description of the most celebrated comets, commencing with Halley's, which he endeavours to trace back as far as the year 426 B.C. The various confirmed appearances of the comet since 1456 are then described, and also circumstances relating to comets in previous centuries, which might possibly be identical with Halley's.

The comet of Encke is traced from its discovery by Mechain, in 1786, to the present time. The detection of a resisting medium in space from the motion of this comet, and the determination (from its perturbations) of the mass of Mercury, are also noticed.

Biela's comet is described at its different appearances since the year 1772. The author notices the supposed identity of the comets of 1264 and 1556, and of those of 1532 and 1661. The great comet of 1843 is described, and, assuming the period of revolution to be about 175 years, some ancient comets are mentioned which might possibly be the same.

Those comets which, though only observed at one appearance, yet remained visible long enough to allow their periodicity to be determined, are placed in a separate class. The comet of Olbers in 1815 is computed by Bessel to have a period of 74 years, and the next return is fixed for February 9, 1887. The comets of 1740 and 1666 do not show the least resemblance to this. The great comet of 1811 was found by Argelander to have a period of 3066 years. The second comet of 1811 was computed by Nicolai; it was much fainter than the first: the period assigned is about 763 years. Encke made the time of revolution of the comet of 1812 about 71 years. The comet of 1807 was computed by Bessel, who fixed the period at about 1714 years.

The author describes the celebrated comet of 1680, that of Lexell in 1770, the comets of 1769, 1780, 1783, 1793, &c., and concludes that there are three comets whose return is certain; five probably periodical, from the similarity of their elements with those of pre-

ceding comets ; and nineteen for which elliptical orbits have been calculated with some degree of probability, making the total number of periodical comets twenty-seven.

In the latter part of the paper a general account of remarkable comets is given, commencing with that mentioned by Ovid, and continued down to 1843. The author has collected together the best determinations of the orbits of periodical comets, and many particulars relating to the physical appearance of these bodies.

### XXXVII. *Intelligence and Miscellaneous Articles.*

ON NEWTON'S TELESCOPE AT THE ROYAL SOCIETY.

GENTLEMEN,

Sidmouth, Feb. 9, 1847.

**Y**OUR correspondent Mr. Weld charges me, in a somewhat angry tone, with having made an *unfounded* statement respecting the telescopes of Newton and Hadley. For my own justification I subjoin the extract from which I derived my information, and leave the result in the hands of Sir James South and Mr. Weld.

With respect, I am, Gentlemen,

Yours obliged,

N. S. HEINEKEN.

"The Newtonian reflecting telescope was discovered by the head, and made by the hands, of Sir Isaac Newton in 1671. Its large speculum was two inches and three-tenths in diameter; its focal length was about five inches, and magnified thirty-eight times : it is in the possession of the Royal Society. I regret to say it is in a most dilapidated condition, and its eye-glass is lost. The next of any importance was made by Hadley in 1728 ; its large speculum's diameter was about six inches ; its focal length about sixty-three inches ; it magnified 230 times ; in performance it equalled the great Huygenian refracting telescope of six inches diameter and 123 feet focus. He gave it to the Royal Society. Its metal is ruined, and its tube, its stand, and other of its appurtenances are lost, &c."—*Times*.

I have extracted the above from the Magazine of Science, vol. vii. p. 36.—N. S. H.

*Note.*—We learn, on making inquiry of eminent practical astronomers, on whose judgement we can rely, that there is no foundation for the representation given in the above-cited letter to the *Times* newspaper. At the same time we cannot see that any blame whatever attaches to our much-respected correspondent Mr. Heineken, relying as he has done on the authority which he quotes.—ED.

#### FORMATION OF SULPHOVINIC ACID. BY M. M. E. MILLON.

The author states, that some years since wishing to prepare sulphovinic acid with peculiar care, he made a mixture of sulphuric acid and alcohol in a platina crucible surrounded with ice and common salt : the acid mixture being afterwards saturated, did not yield the slightest trace of sulphovinate.

Wishing afterwards to ascertain precisely the conditions under

which the formation of sulphovinic acid occurs, the author discovered that it depended on several circumstances, the principal of which are, —1st, the relative proportions of acid and alcohol; 2nd, the heat applied to the mixture; 3rd, the heating of the two liquids at the moment of mixing them; 4th, the time during which the acid and alcohol remain in contact. To ascertain these points, the author at first employed monohydrated sulphuric acid  $\text{SO}^3, \text{HO}$ , and absolute alcohol  $\text{C}^4 \text{H}^6 \text{O}^2$ , or else hydrated  $\text{C}^4 \text{H}^6 \text{O}^2, \text{HO}$ ; there is no difference in the progress of the reaction, whether absolute or hydrated alcohol be employed.

To determine the quantity of sulphovinic acid formed, the same quantity of sulphuric acid as that used with the alcohol was mixed with water; and this last was used in such proportion as to make the diluted acid equal in volume to the mixture of acid and alcohol. Equal portions of these were then saturated with an alkaline solution of known strength, and by the difference the proportion of combined sulphuric acid was determined; it being ascertained that sulphuric, on becoming sulphovinic acid, saturates one-half less base.

When equivalents of sulphuric acid and anhydrous alcohol are employed, the following results are obtained: if the alcohol be rendered extremely cold, and the sulphuric acid be gradually added, so as to avoid raising the temperature, no combination whatever takes place. The sulphuric acid retains all its saturating power; and if it be kept constantly cooled by melting ice, the strength of the acid remains the same for several successive days. A temperature of  $50^\circ$  to  $60^\circ$  F. is sufficient to cause the conversion of 77 parts out of 100 of sulphuric acid into sulphovinic; thus about three-quarters of what the sulphuric acid is capable of yielding are produced; this proportion cannot be exceeded. It remains the same for several months, and it is useless to raise the temperature of the mixture. When equivalents of sulphuric acid and alcohol are used, 77 out of 100 of the acid is the largest proportion that can be formed into sulphovinic acid. This formation requires several days at  $50^\circ$  to  $60^\circ$  F.; but it occurs in a few hours at  $86^\circ$  to  $95^\circ$  F.; and it takes place in a few minutes if the mixture be immersed in boiling water; and it is a remarkable circumstance that the combination is instantaneous, if the alcohol be suddenly poured into the sulphuric acid so as to produce great heat.

It appears, therefore, that there are in reality, in this formation of sulphovinic acid, three sources of chemical action which are equivalents; they are time, the temperature applied, and the natural heating of the mixture. The action of light was repeatedly attempted to be substituted; but the direct application of the most intense solar rays never accelerated the formation of sulphovinic acid.

The mixture of two equivalents of alcohol with one equivalent of sulphuric acid acts exactly in the same manner as the preceding mixture; only it is more easy to avoid in it the formation of sulphovinic acid; the influence of time and heat is similarly exerted. The sulphuric acid produces a little less sulphovinic acid, from 73 to 74 in 100 parts; the non-action of light is similar.



If however two equivalents of sulphuric acid and one equivalent of alcohol be employed, circumstances are altered ; sulphovinic acid is always formed ; and still more, the proportion of sulphovinic acid is always the same, whether the acid be poured into the alcohol or the reverse ; whether metallic vessels be used or not ; and whether they are or are not cooled. The whole of the acid and alcohol do not however combine ; the reaction does not take place on much more than half of the mixture, for not more than 54 of sulphovinic acid can be formed from 100 of sulphuric. This proportion does not change by prolonging the contact, or even by the application of a heat of 212° F. continued for several hours ; the capacity of saturation of the acid mixture remains the same. In heating to 212°, the volume is scarcely diminished ; it follows, therefore, that the alcohol which is not changed into sulphovinic acid must be retained by a peculiar affinity.—*Ann. de Ch. et de Phys.*, Fevrier 1847.

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ON A METHOD OF PRODUCING LIGHTS AND SHADES IN EQUAL PERFECTION IN DAGUERRETYPE PICTURES. BY MM. BELFIELD LEFEVRE AND LEON FOUCAULT.

It was observed soon after M. Daguerre had made known his wonderful discovery, that the iodized plate was not suited for producing a perfect image of every object, but that if there was a great variety of light and shade, *i. e.* very light and very dark parts, they would not be simultaneously brought out with correctness : one of these two alternatives must be chosen ; either to stop the process when the light parts of the picture are brought out (in which case the dark parts will not be clearly distinguishable), or to allow the light to act for a longer time, in order to render the dark parts distinct ; but in this case the light parts will be rendered indistinct, or, as it is called, burnt.

These evils have fortunately been remedied by the employment of certain substances, which not only allow of the operation being performed with greater rapidity, but also produce sensitive surfaces capable of receiving a much greater variety of tints. These are, however, far from being perfect ; and therefore if any process could be discovered which would bring out the light and dark parts of the picture with equal distinctness, without increasing the sensitiveness of the plates, it would be advisable to have recourse to it in certain cases. If the operator knows how to work well upon these different sensitive surfaces, without confining himself exclusively to the most sensitive, he will be able to produce any effect desired ; for instance, if requisite, he will moderate the intensity of the rays of the sun bearing upon objects of inadequate reflective powers, or heighten the tone of a picture.

With a view to facilitate the attainment of good results, Mr. Belfield and myself determined on making known a new mode of preparing the sensitive surfaces, the effect of which is to impart to the plates the property above-mentioned, and which assimilates them to the human retina.

Our method requires the employment of iodine and bromine, and is easily performed by persons who are accustomed to employ those substances separately. It consists in polishing and iodizing the plate in the ordinary manner, and afterwards causing it, by any convenient means, to absorb three times as much vapour of bromine as is usually thought sufficient to render the plates as sensitive as possible. Whilst the ordinary quantity of bromine does not visibly alter the tint of the iodized layer, that which we recommend causes it to assume a deep bluish violet tint.

The sensibility of the plates thus surcharged with bromine is reduced to a third of what it would be if the ordinary quantity only were used ; but at the same time they are rendered capable of producing a perfect picture of subjects presenting the greatest variety of shade. This will be seen by inspecting a small picture, presented herewith, which was produced when the sun was shining. There will be perceived the clouds in the sky, white houses, with the shadows well-defined, and trees, the foliage of which is delineated much in the same manner as if executed by an artist.

We recommend the ordinary quantity of bromine to be exactly tripled ; as if less than this quantity be used, the picture will not be properly brought out ; and if more than this quantity were used, the mercury would not be properly condensed and the image would not be so well-defined.

This new property, communicated to the iodized plates by an excess of bromine, may be very usefully applied ; and besides, as it appeared to us that this statement might be useful to the chemical world, we have been induced to make this communication to the Academy.—*Comptes Rendus des Séances de l'Académie des Sciences*, (as inserted in the *London Journal*.)

OBSERVATIONS ON THE EXPERIMENTS OF MESSRS. FOUCAULT AND FIZEAU, RELATIVE TO THE ACTION OF THE RED RAYS UPON DAGUERRETYPE PLATES. BY M. EDMOND BECQUEREL.

Messrs. Foucault and Fizeau, at the sitting of the Academy on the 5th inst. (October 1846), presented some observations concerning the action of the red rays upon Daguerreotype plates, from which it would appear that the least refrangible part of the solar spectrum acts upon the iodide of silver in an inverse manner to the most refrangible portion. As it appears that these gentlemen are not acquainted with the experiments which have been made during the last few years on this subject, and as the results stated by them do not appear to me to lead to the conclusions which they deduce, I take the liberty of submitting some remarks thereon to the Academy.

Dr. Draper (*Philosophical Magazine*, Nov. 1842), on examining the image produced by the action of the spectrum upon iodized plates of silver, made known, before these gentlemen, the existence of protecting rays modifying the influence of the solar rays, and even acting negatively upon the iodide of silver. Sir J. F. W. Herschel examined the pictures on that occasion, and in a very interesting experiment

(Philosophical Magazine, Feb. 1843) on the different appearances which the iodized plates assume when exposed for the same space of time to varying intensity in the light, and submitted to the vapour of mercury, attributed these effects to the unequal thickness of the substances deposited upon the plates of silver serving as reflecting surfaces. He showed, moreover, that on operating upon paper covered with iodide of silver, nothing was observed which indicated the action of negative rays, but that all the active parts of the solar spectrum acted chemically in the same manner on the iodide.

The experiments which I have made on the chemical action of the solar rays, from 1841 to 1844, have all led me to the same conclusion. Attention ought not therefore to be directed to the deposits formed on the surface of Daguerreotype plates, as if these were the only data to lead to the conclusion that the rays acted in various ways; for if so, there would be risk of defective results.

In support of the foregoing I will cite the following experiment, of the accuracy of which any person will be able to judge. "Let a Daguerreotype plate be prepared with iodine only (in order to avoid the admixture of active substances), and let the blue, indigo, and violet parts of a purified blue spectrum, presenting Fraunhofer's black lines, be thrown upon it. If the action only lasts a short time, after submitting it to the mercury vapour, the black lines will be seen to fix themselves upon a white ground, which represents those parts affected by the active parts of the spectrum\*." But if the plate be exposed to the spectrum for an hour or more, then the appearance of the plate changes, on passing it through the mercury vapour; the lines of the spectrum are scarcely marked, and the action has been nearly uniform throughout its surface, but the lines which are visible appear white, and show very distinctly upon the ground, which approaches to blackness: the effect is quite contrary to what it was before. That portion of the plate which is acted upon by the violet part of the spectrum has, under these circumstances, the same appearance as the portion of the plate exposed to the red rays by Messrs. Foucault and Fizeau; and to produce this effect, it was only necessary to vary the time of exposure of the plate to the same portion of the spectrum. Must it be inferred, in the second case, that the iodide of silver had been acted upon by negative rays? Certainly not; for if the experiment be repeated upon iodide of silver laid upon paper, the paper will become darker and darker, in proportion to the time it continues exposed to the spectrum: and, besides, I have found (*Annales de Chimie et de Physique*, 3rd series, vol. ix. p. 268 *et seq.*) that the electrical effects arising from the chemical decomposition of the iodide always act in the same direction.

It will be seen that the conclusion to which Messrs. Foucault and Fizeau came, viz. that there exist in the red prismatic rays negative rays, cannot be received, simply from the fact that the Daguerreotype plates are not always the same in appearance. The contrary effects, as will be seen hereafter, are secondary effects produced by

\* E. Becquerel on the Constitution of the Solar Spectrum.—*Bibliothèque Universelle de Genève*, August 1842.

several chemical reactions taking place simultaneously, and are not due to contrary action, exercised on the part of the solar rays, on the iodide of silver alone.

Another fact which I will call attention to is, that the least refrangible part of the spectrum, instead of possessing a negative action upon the iodide of silver, exercises a continuous influence upon most of the salts of silver alone, such as the iodide, the bromide, and the chloride; and also that the experiments upon which this proposition is founded have been verified by the commissioners of the Academy, charged with the examination of one of my memoirs.

In the foregoing, the only point discussed was the influence of light upon iodide of silver, or the simple salts of that metal. When plates of silver are exposed successively to the vapours of iodine, bromine or chlorine, the mixtures obtained may give rise to various kinds of chemical reaction, of which the result only is appreciable. For this reason these mixtures must not be employed without great caution, and the Daguerreotype plates must be used as little as possible in experimenting as to the nature of the active rays.

In order to show how far the mixture of sensitive materials is capable of influencing the effects of the spectrum, I will direct attention to the following observation of Sir J. Herschel:—

If paper be prepared, first with a strong solution of lead and afterwards with bromate of potash and nitrate of silver, a surface will be produced which will speedily become black on exposure to the light; on being presented to the spectrum, the black tint will be produced in the most refrangible rays, as far as green. But if the paper has been blackened by previous exposure to the light, on being covered with a dilute solution of iodide of potassium and exposed to the blue part of the spectrum, the paper will become white. This result proves that the iodide of potassium is decomposed, and that the silver which stained the paper being iodized, and coming in contact with an alkaline iodide, ceases to be affected by the light; the paper will therefore remain of a yellowish-white in that portion of the spectrum on which the reaction takes place.

If the layer of iodide of potassium employed is produced from a dilute solution of that salt, the paper not only becomes white in the violet part of the spectrum, but also becomes darker in the red rays, and even beyond, a neutral line being in the middle. It would appear therefore, from an examination of the image thus obtained, that two contrary results were produced; viz. the destruction of the colour in the violet part and its augmentation in the red. These effects may be easily explained as two distinct chemical reactions: first, the action of the light upon the iodide of silver, the colouring of which had commenced; second, the action of the light to effect the decomposition of the iodide of potassium, and the iodizing of the silver arising from the sub-iodide formed by the first reaction. As the red part of the spectrum contains those rays which continue the chemical action commenced upon the salts of silver, and as the first reaction is only commenced, the latter has most influence in that part of the spectrum. The second reaction is, on the contrary,



at its height in the violet. Thus these appearances of inverse action in the colouring do not arise from two distinct effects, *positive* and *negative*, produced by the rays upon the same sensitive surface, but are owing to two distinct chemical reactions, which predominate respectively in the red and violet parts.

If the blackened paper be covered with a fresh layer of iodide of potassium, it will begin to turn white at the least refrangible part, and the neutral line will again approach the red; if a sufficient quantity of iodide be used, the paper will turn white from the violet to the red; but if a very strong solution of iodide were employed, the paper would whiten, even in the dark; so violent is the action of the iodide of potassium upon metallic silver.

These results clearly prove that several chemical actions may take place simultaneously in the mixtures of sensitive substances, the results only of which are observable. Analogous effects must necessarily be produced on employing iodized plates of silver, and afterwards exposing them to the vapour of bromine or to chlorine; and perhaps even when using plates iodized according to M. Daguerre's plan. In fact, under these circumstances, the iodide, chloride, or bromide of silver, are in direct contact with the metallic silver; and as, by the decomposition of these salts, through the action of the light, subsalts are formed, the result is that iodine, chlorine and bromine are exposed directly to the above-named salts, and even to the metallic silver itself, at the moment when the solar action makes its influence felt. These reactions, which are sufficiently complex, become more so by the iodides, chlorides, and bromides of silver being submitted to the action of rays which always act with the same energy in the violet part of the spectrum; whilst in the red portion the rays react with greater energy, owing to certain chemical actions having commenced.

It is therefore essential to distinguish between the chemical reactions effected under the influence of light upon sensitive substances alone, and upon combinations of them. This has not been done by Messrs. Foucault and Fizeau: they have considered a Daguerreotype plate as offering a *separate sensitive surface*, whilst it is only by a mixture of substances that different effects can be produced in the various parts of the spectrum, as is proved by Herschel's experiment, and without the existence of rays acting in an inverse direction. Thus it has been proved by experiment, that the solar rays, although of various degrees of refrangibility, only act in one way upon iodide of silver; whilst a mixture of this substance with other matters may occasion several chemical reactions acting conjointly and hiding the principal effect.

If the light acts only in one way upon iodide of silver (the continuous rays included), the case may be different on other sensitive substances, and the rays may act sometimes positively and sometimes negatively. It is known, in fact, that each sensitive substance is differently affected by the solar rays; this might be explained by stating that each sensitive substance receives the rays in a manner peculiar to itself. I will cite as an example an observation of Wol-

*Phil. Mag. S. 3. Vol. 30. No. 200. March 1847.* Q

laston's, with regard to the sensitive substance *guaiacum*, which becomes blue beyond the violet part of the spectrum, and again becomes colourless in the red and yellow rays.

I have confined myself to the foregoing observations, on the subject of Messrs. Foucault and Fizeau's note, to show that the complicated phenomena produced by the appearance of the Daguerreotype plates (although very important for the photographic images produced in the camera-obscura, and for the observation of active rays of very little intensity) cannot lead to definite and unvarying results, as regards the nature of the chemical action produced. It is therefore necessary to operate, as has been heretofore done, by means of simple products, with sensitive paper, regard being had to the electrical effect due to the chemical reaction produced under the influence of the solar rays.—*Ibid.*

#### RESEARCHES ON MELLON AND ITS COMPOUNDS.

BY MM. AUG. LAURENT AND CH. GERHARDT.

The authors observe that in their last memoir on this subject they had not repeated the experiments of M. Liebig; that they had merely modified a part of his theory, the authority of his name appearing to be a sufficient guarantee for the accuracy of his results.

According to M. Liebig, this substance is composed of—

Six atoms of carbon . . . . .	=458.61	39.36
Eight atoms of azote . . . . .	=708.16	60.64
One atom of mellon. . . . .	=1166.77	100.00

According to him, it is a radicle composed of carbon and azote, capable of uniting with hydrogen, oxygen and metals, so as to form compounds analogous to those into the composition of which cyanogen enters.

The authors state, and they do it with regret, that the reliance which they placed on the results of M. Liebig has totally misled them; and that it is not a part only of his labours on this subject which is erroneous, but that this is the case with the entire history of mellon, with all its transformations and all its reactions. The authors assert that they are also in a condition to prove that the supposed analogy with cyanogen does not exist; for instead of consisting only of carbon and azote, mellon contains besides one and a half per cent. of hydrogen. Various processes have been employed for obtaining mellon. That analysed by MM. Laurent and Gerhardt was obtained very pure by calcining chlorinated cyanamide (*cyanamide chlorée*) as long as hydrochloric acid and hydrochlorate of ammonia were disengaged.

The following are the results of four experiments performed to ascertain the composition of mellon:—

	I.	II.	III.	IV.
Carbon . . . . .	36.0	35.8	36.4	35.73
Hydrogen . . . . .	1.7	1.8	1.7	1.77
Azote . . . . .	61.3	62.4	61.9	62.50
	<u>99.0</u>	<u>100.0</u>	<u>100.0</u>	<u>100.00</u>

The authors state that theory would give—

C <sup>6</sup> .....	450·00	36·0
H <sup>3</sup> .....	18·75	1·5
N <sup>9</sup> .....	787·50	62·5
	<hr/> 1256·25	<hr/> 100·0

These numbers are confirmed by the results obtained by M. Voelckel in analysing glaucene, which, according to MM. Laurent and Gerhardt, is merely mellon derived from the dry distillation of poliene (A.), and of persulphohydrocyanic acid (B.). The results of M. Voelckel are as follows :—

	A.		B.
Carbon .....	35·4	35·7	36·2
Hydrogen .....	1·6	1·6	1·8
Azote .....	63·0	62·7	62·0
	<hr/> 100·0	<hr/> 100·0	<hr/> 100·0

From numerous experiments, MM. Laurent and Gerhardt have arrived at the following conclusions :—

1. Mellon is not, as supposed by M. Liebig, a binary compound, comparable with cyanogen ; for besides carbon and azote, which are not in the proportions stated by M. Liebig, it contains one and a half per cent. of hydrogen, and is represented by C<sup>6</sup> H<sup>3</sup> N<sup>9</sup>.

2. The formation of mellon by chlorinated cyanamide does not occur, as stated by M. Liebig, accompanied simply with hydrochlorate of ammonia, but there is also disengaged an enormous quantity of hydrochloric acid, amounting to about 16 per cent.

3. Chlorinated cyanamide contains double the quantity of chlorine stated by M. Liebig, and ought to be represented by C<sup>3</sup> H<sup>4</sup> Cl N<sup>5</sup>.

4. By the influence of potash, chlorinated cyanamide is converted into ammeline C<sup>3</sup> H<sup>5</sup> N<sup>5</sup> O, or ammelide C<sup>3</sup> H<sup>4</sup> N<sup>4</sup> O<sup>2</sup>.

5. The new substance, the formation of which has been recently announced by M. Liebig from urea, is a body previously known by the name of ammelide.

6. Ammelide does not possess the composition attributed to it by M. Liebig ; it contains C<sup>3</sup> H<sup>4</sup> N<sup>4</sup> O<sup>2</sup>, as had been previously stated by one of the authors.

7. The melam of M. Liebig is an impure mixture of poliene C<sup>3</sup> H<sup>6</sup> N<sup>6</sup> and mellon.

8. The dry distillation of persulphohydrocyanic acid, as stated by M. Liebig, does not occur ; but the assertions of M. Voelckel respecting this phænomenon are correct.

9. The sulphuret of cyanogen does not possess the composition adopted by M. Liebig : it contains hydrogen, and is represented by C<sup>3</sup> HN<sup>3</sup> S<sup>3</sup> ; it cannot therefore be the radical of the sulphocyanurets.

10. Heat does not decompose mellon, as stated by M. Liebig, into three volumes of cyanogen and one volume of azote ; but the products vary according to the temperature, and always contain hydrogenated compounds, to the complete destruction of the mellon.

11. Potash does not dissolve mellon and yield the mellonide of M. Gmelin, as stated by M. Liebig; but it produces a tribasic salt  $C^6(HM^3)N^8O^3$ , containing hydrogen and oxygen, at the same time that ammonia is developed. Other products are also formed at the same time.

12. Mellon does not simply and purely combine with potassium; but the combination occurs with the evolution of ammonia, and the product is a bibasic salt  $C^6N^8M^2$ , a salt which on dissolving in water probably gives  $C^6N^8M^2H^2O$ , and appears to be identical with the mellonides of M. Gmelin.

13. Mellon does not integrally dissolve in sulphuric acid, for the liquid contains sulphate of ammonia.—*Ann. de Ch. et de Phys.*, Janvier 1847.

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ON THE PREPARATION, PROPERTIES AND COMPOSITION OF  
ACETAL. BY M. STAS.

The author states that by following precisely the directions of M. Liebig for preparing acetal, he obtained a product which, when re-distilled, did not possess any fixed boiling-point. The liquid procured at  $203^{\circ}$  F. recommenced boiling at about  $190^{\circ}$ , and the temperature gradually increasing, finished at  $226^{\circ}$  or even  $230^{\circ}$ .

The product distilled at  $190^{\circ}$  was reduced to about half by a concentrated solution of potash; this action takes place in a few hours without the contact of air. The portion collected at about  $220^{\circ}$ , and below it, does not undergo any appreciable alteration by the alkaline solution without the contact of the air.

The potash solution, obtained by treating the first product, remains colourless; when saturated with carbonic acid and evaporated *in vacuo*, a residue is left, which yields to anhydrous alcohol a white salt, which is very soluble in water, and precipitates nitrate of silver in pearly white laminæ; it is merely acetate of potash without any trace of formiate.

These facts prove, according to M. Stas, that the acetal described by M. Liebig is a mixture of at least two substances, one of which is acetic æther, and the other is acetal, properly so called, and comparable to the methylal of M. Malaguti, as will hereafter be shown.

M. Stas observes that some chemists still doubting of the existence of acetal, he states the method of procuring it in a certain and rapid manner, and constant in composition, before he describes its properties; the process is as follows:—

Fragments of pumice-stone are to be washed with hydrochloric acid and then heated to redness; afterwards the pumice is to be moistened with alcohol which is nearly anhydrous, and to be introduced into a balloon capable of containing 80 to 100 pints. This balloon should have a short neck, but large enough to allow of introducing the arm. As many capsules as the balloon will contain are to be placed on the pumice; these capsules are to be as flat as possible, and covered with a thin stratum of platina black. The neck is to be perfectly covered with a glass plate, and the balloon is



then to be exposed to a temperature of about  $68^{\circ}$ , till nearly all the alcohol is converted into acetic acid.

Fresh air is to be occasionally admitted into the balloon, and care taken that the liquid is always below the pumice, and that the latter is never covered by the former. In about a fortnight or three weeks the liquid becomes more or less viscid, its consistence resembling that of sulphuric acid on being poured. At this period the liquid is to be removed, and replaced by an equal quantity of alcohol of 60 per cent. When several pints of very acid liquid have been thus obtained, it is to be neutralized by carbonate of potash, and as much chloride of calcium dissolved in it as it is capable of taking up. Dried acetate of potash may be substituted for chloride of calcium; in this case it is unnecessary to saturate the liquid by the carbonate of the same base.

In whatever mode the liquor is saturated, it is to be submitted to cautious distillation; only one-fourth of it is to be distilled, and in a well-cooled receiver; this is to be saturated with fused chloride of calcium, which immediately separates a considerable portion of a very volatile fluid of an extremely suffocating odour; this is to be separated by a pipette, and water is to be cautiously added to the saline solution as long as it continues to separate fresh quantities of æthereal fluid, which is to be added to that previously obtained. By careful distillation of the solution of chloride of calcium, a still further small portion of the organic matter may be extracted: as already remarked by M. Liebig, the substance thus separated is a mixture of aldehyd, acetic æther, alcohol and acetal.

In order to isolate the acetal, powdered chloride of calcium is to be added to the mixed fluids, as long as it continues to dissolve; when this is completed, the liquid is to be distilled from a water-bath, but always below its boiling-point, until the product ceases to reduce ammoniacal acetate of silver. The residual matter, deprived of aldehyd, but still containing some acetic æther and alcohol mixed with the acetal, is added to a great excess of very concentrated solution of potash, which completely destroys the acetic æther: in operating upon about 750 grains of the fluid, and very frequently agitating the mixture, it requires three or four days' action to decompose the last portion of the æther. It now remains only to wash the acetal with once or twice its volume of water, to digest it with recently-fused chloride of calcium, and to distil it, in order to have it perfectly pure.

The properties of acetal are, that it is a colourless æthereal and fluid liquid, but less so than æther, to which it has been compared. It has a peculiar sweet smell; its taste is cooling with a distinct nutty after-taste. Its density is 0.821, at about  $72^{\circ}$  F. It boils between  $219^{\circ}$  and  $223^{\circ}$  F. Water at  $77^{\circ}$  dissolves about  $\frac{1}{18}$ th of its volume, and less as its temperature is increased. Chloride of calcium and all very soluble salts in general separate the acetal from solution in water; æther and alcohol dissolve acetal in all proportions; chloride of calcium does not separate it from alcohol unless water be added; aldehyd, as observed by M. Liebig, exhibits the same phænomenon.

Under the influence of platina-black or of the air, acetal is very rapidly converted, first into aldehyd and then into concentrated acetic acid; the action is extremely rapid when the platina is moistened; oxidizing bodies in general produce the same effect; thus diluted nitric acid furnishes aldehyd at first and afterwards acetic acid; chromic acid gives acetic acid only. A solution of ammoniacal acetate of silver has no action upon it at any temperature; out of the contact of air neither saturated solutions of potash, soda, the solid alkalies, nor potashed lime, have any action upon it, either at a low or high temperature; sulphuric acid at first dissolves it, then decomposes and blackens it; chlorine acts upon it, removes its hydrogen and forms chlorinated compounds, which the author has not had time yet to examine. By analysis it yielded—

C <sup>6</sup> .....	450·0	61·01
H <sup>7</sup> .....	87·5	11·85
O <sup>2</sup> .....	200·0	27·14
	<u>737·5</u>	<u>100·00</u>

According to Liebig it consists of—

C <sup>8</sup> .....	611·480	59·72
H <sup>18</sup> .....	112·315	10·97
O <sup>3</sup> .....	300·000	29·31
	<u>1023·795</u>	<u>100·00</u>

M. Stas observes, that on comparing the results of his analyses with the composition of alcohol, the latter, under the influence of the oxygen of the air and platina-black, has lost one-third of its oxygen, and two-ninths of its hydrogen; these quantities being equivalent to one of oxygen and two of hydrogen, as is readily ascertained. Acetal may be represented by a combination of two molecules of æther with one molecule of aldehyd, according to M. Stas, C<sup>12</sup> H<sup>14</sup> O<sup>4</sup> = 2(C<sup>4</sup> H<sup>3</sup> O), C<sup>4</sup> H<sup>4</sup> O<sup>2</sup>; while according to Liebig its formula is C<sup>4</sup> H<sup>6</sup> O, Ae O + H<sup>2</sup> O = C<sup>6</sup> H<sup>16</sup> O<sup>3</sup>, or one equiv. of aldehyd and one equiv. of æther.—*Ann. de Ch. et de Phys.*, Feb. 1847.

#### PRESENCE OF SULPHUR IN METALLIC SUBSTANCES STRUCK BY LIGHTNING.

On Sunday, the 14th of June 1846, the parish church of Saint Thibaud-de-Couz, three leagues from Chambery, was struck by lightning; the church was filled with a dense smoke, accompanied by a strong smell resembling that of gunpowder. The gilt frame of a large picture was almost entirely blackened, and six gilt chandeliers were all rendered as black as copper would be after long exposure to sulphuretted hydrogen.

M. Bonjean procured some powder by scraping the surface of the chandeliers which had been most strongly coloured; by treating it with aqua regia he obtained a solution in which solution of nitrate of barytes gave a white precipitate insoluble in nitric acid.—*Journ. de Pharm. et de Ch.*, Decembre 1846.

ON CHROMATE OF CHROMIUM.

M. C. Rammelsberg states that when a solution of chrome-alum is mixed with one of neutral chromate of potash, the first portions produce a red brown colour; afterwards a brown precipitate is formed, on the surface of which a very yellow liquid floats.

This precipitate dissolves in hydrochloric acid with a yellowish-green colour; ammonia precipitates oxide of chromium, leaving chromic acid in solution. When digested with potash, it readily yields chromate of potash and oxide of chromium.

M. Rammelsberg gives the annexed formula  $[3 \text{Cr}^1 \text{O}^3, 2\text{Cr}^2 \text{O}^3, 9\text{H}^2 \text{O}]$ .—*Journ. de Pharm. et de Ch.*, Novembre 1846.

METEOROLOGICAL OBSERVATIONS FOR JAN. 1847.

*Chiswick*.—January 1. Frosty; overcast. 2. Hazy and cold. 3. Dry haze: snow at night. 4. Cloudy. 5. Hazy. 6, 7. Foggy. 8. Hazy. 9. Cold: hazy. 10. Sharp frost at night. 11. Frosty: fine: sharp frost. 12, 13. Foggy. 14. Sharp frost: foggy: frosty. 15, 16. Dense fog: frosty at night. 17. Dry haze: foggy. 18. Cloudy and cold. 19. Overcast: hazy. 20. Slight haze: snow at night. 21. Snowing. 22. Dense fog throughout. 23. Slight fog: cloudy: rain. 24. Densely clouded: rain: clear. 25. Fine: slight showers in the evening. 26. Partially overcast: fine: boisterous at night. 27. Clear: rain at night. 28. Boisterous: clear and frosty. 29. Fine, with sun: clear and frosty. 30. Clear and frosty: fine: densely overcast. 31. Light haze: cloudy: fine.

Mean temperature of the month .....  $34^{\circ} 26$

Mean temperature of Jan. 1846 .....  $43^{\circ} 54$

Mean temperature of Jan. for the last twenty years ...  $36^{\circ} 81$

Average amount of rain in Jan. .... 1.60 inch.

*Boston*.—Jan. 1. Foggy: snow on the ground. 2, 3. Cloudy. 4. Cloudy: snow early A.M. 5. Cloudy: rain early A.M. 6. Cloudy. 7. Rain. 8—11. Cloudy. 12. Cloudy: rain P.M. 13. Cloudy. 14—16. Fine. 17—20. Cloudy. 21. Cloudy: snow P.M. 22. Cloudy: snow on the ground. 23. Cloudy. 24. Cloudy: snow nearly all melted. 25. Fine: rain midday. 26. Fine. 27. Windy. 28. Rain. 29—31. Fine.—This January has been the coldest since January 1842, and the driest since 1833.

*Sandwich Manse, Orkney*.—Jan. 1. Fog. 2. Cloudy. 3. Damp: showers. 4. Showers. 5. Showers: rain. 6, 7. Damp. 8. Cloudy. 9. Bright: clear. 10—12. Bright: frost: clear. 13. Bright: clear. 14. Bright: frost: clear. 15. Bright: clear. 16. Drops: cloudy. 17—19. Cloudy: clear: aurora. 20. Fine: frost: clear: frost: aurora. 21. Sleet-showers. 22. Bright: clear. 23, 24. Cloudy: rain. 25—27. Bright: clear. 28. Clear: frost: cloudy. 29. Bright: clear. 30. Bright: snow-showers: aurora. 31. Hail-showers: snow-showers: aurora.

*Applegarth Manse, Dumfries-shire*.—Jan. 1. Clear, but moist. 2. Dull and raw: slight snow. 3. Slight frost: very chilly. 4. Dull: slight frost: rain P.M. 5. Dull: rain. 6. Fine and fair. 7, 8. Fair, but cloudy. 9, 10. Frost: clear. 11—13. Frost, hard. 14. Frost, hard, but cloudy. 15. Thaw. 16. Thaw: drizzle. 17—21. Frost again. 22. Frost: snow: thaw. 23. Thaw: sleet. 24. Heavy rain. 25. Slight frost A.M.: rain. 26. Heavy rain. 27. Rain: cleared P.M. 28. Fair, but dull. 29. Slight frost A.M. 30. Slight frost A.M.: cloudy. 31. Hard frost: snow P.M.

Mean temperature of the month .....  $35^{\circ} 97$

Mean temperature of Jan. 1846 .....  $43^{\circ} 0$

Mean temperature of Jan. for 25 years .....  $34^{\circ} 9$

Mean rain in Jan. for 20 years ..... 2.60 inches.

*Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at Chiswick, near London; by Mr. Veall, at Boston; by the Rev. W. Dunbar, at Applegarth Manse, Dumfries-shire; and by the Rev. C. Clouston, at Sandwick Manse, Orkney.*

Days of Month.	Barometer.						Thermometer.				Wind.				Rain.																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																	
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THE  
LONDON, EDINBURGH AND DUBLIN  
PHILOSOPHICAL MAGAZINE  
AND  
JOURNAL OF SCIENCE.

[THIRD SERIES.]

APRIL 1847.

XXXVIII. *On the Effects of Magnetism, &c. upon the Dimensions of Iron and Steel Bars.* By J. P. JOULE, Esq., Secretary of the Literary and Philosophical Society of Manchester.

[Continued from page 87.]

WITH a view to ascertain whether the lengthening effects observed in the experiments detailed in the former part of this paper were entirely independent of the diameter of the bars, I made a very extensive series of experiments, in which fine wires both of iron and steel, bundles of very fine iron wires, chains composed of copper and iron links, &c. were employed. In order to keep these flexible articles exactly in the axis of the coil, a weight was placed upon one of the levers, so as to exert upon them a force of tension equal to about eight ounces.

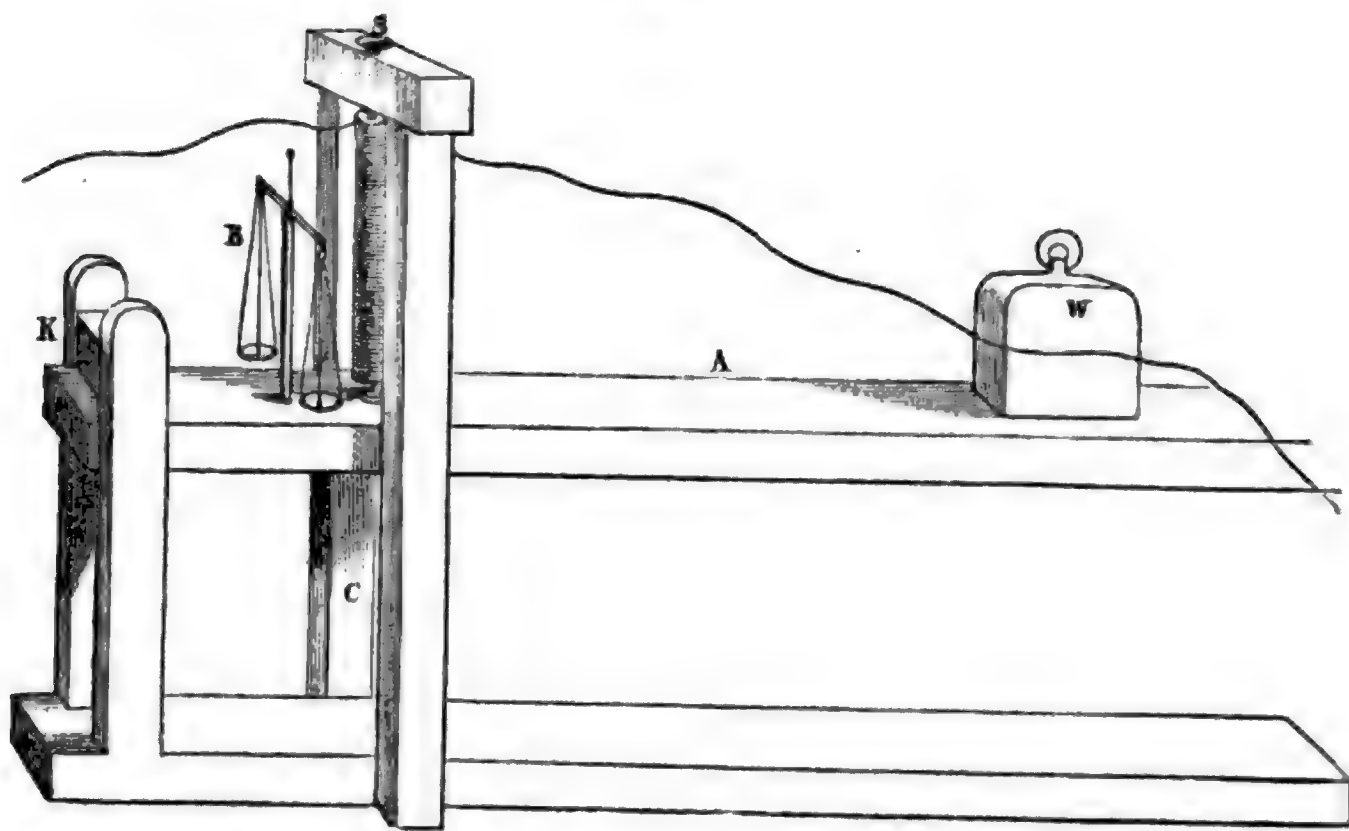
The results of the experiments in which wire of  $\frac{1}{20}$ th of an inch thickness was employed, accorded very well with the previous experiments with thicker bars; but on employing iron wire which was only  $\frac{1}{120}$ th of an inch thick, the phenomena assumed quite a different character; for on transmitting the current through the coil, the length of the wire became suddenly *diminished* instead of being increased. This phenomenon appeared to me exceedingly anomalous, and it was some time before I found out its cause. At last, thinking that the wire was attracted by the coil, I varied its position from the centre to either side, and increased the amount of its tension. The former of these operations produced no sensible effect; but the increase of tension caused the shortening effect of the current to be considerably augmented. It became manifest, therefore, that the weight of eight ounces, acting upon very fine wire, produced the anomalies in question. This was further demonstrated to be the case by diminishing

*Phil. Mag.* S. 3. Vol. 30. No. 201. April 1847. R

the tension as far as possible. I then found that the phænomena of elongation took place as in the case of the iron bars, only to a smaller extent, which was obviously owing to the degree of tension necessarily left in order to keep the wire in the axis of the coil.

The new field of inquiry thus opened appeared to me to be one of great importance, and calculated eventually to become the means of throwing a great deal of light upon some of the most interesting questions connected with molecular actions and the real character of magnetism. I therefore constructed an apparatus whereby the effects of tension and pressure might be further studied. This apparatus, which is represented by the adjoining woodcut, consisted of a strong wooden lever A,

Fig. 1.



furnished with a hard steel knife edge at K resting on a hard steel plate. At the distance of one foot from the knife edge a brass plate was fastened to the lever, into which a piece of iron or steel wire, one-fourth of an inch in diameter, could be screwed: the upper end of the wire could also be screwed into a brass bolt, the head of which rested upon the cross piece S. Weights could be placed on the lever at W to increase the tension. The magnetic balance, consisting of a bar-magnet eight inches long, properly furnished with scales, was situated at B. The further extremity of the lever was connected with a fine lever multiplying eight times, the index of

which was examined by the microscope employed in the former series of experiments. Each division of the micrometer passed over by the index indicated an elongation of the wire under examination equal to  $\frac{1}{750000}$ th of an inch.

When pressure instead of tension was employed, a pillar of iron or steel wire, one quarter of an inch in diameter, was placed at C so as to support the weight of the lever; its ends abutted upon flat plates of copper or brass.

Every precaution that I could think of was taken in order to give accuracy to the results. In particular I may mention that the coil was not permitted to touch either the wire under examination, the lever, or any other part of the apparatus to which it might communicate motion through the change of its own molecular condition in consequence of the passage of the electrical current. In spite of these precautions the experiments were very troublesome, owing to the almost incessant vibrations of the index. Although my laboratory is situated quite out of the town, and detached from every dwelling, these vibrations were so extensive during the day time, that the experiments had, in general, to be carried on after eight o'clock P.M., when the greater part of the traffic had ceased. It was at all times impossible to make an observation when a cart was passing along a road at the distance of one or two hundred yards; nor could anything be done as long as much wind was blowing. Owing to these circumstances it was frequently very difficult to estimate an effect equal to one-tenth of a division of the micrometer. I believe, however, that the results of the tables are in no case more than two-tenths of a division wide of the truth.

The experiment which I first made was with a piece of soft iron wire, one foot long and one quarter of an inch in diameter. It was made as straight as possible, and its ends were ground perfectly true and flat. It was placed as a pillar under the lever, so as to support its pressure, which was equal to 82 lbs. The coil by which it was magnetized was formed out of a copper wire thirty-three yards long and one-tenth of an inch in diameter, well-covered with cotton-thread. Its length was  $11\frac{1}{2}$  inches, and its interior diameter one inch. The same coil was employed in all the experiments with wires of a foot long. The temperature of the wire was in every case about  $45^{\circ}$ .

The method of experimenting was the same as I employed in the former section of this paper. A current was passed through the coil, the effects of which, both on the length of the iron pillar and on its magnetism, were noted. The current was then cut off; and the effect of so doing on the length of the pillar noted, as well as the quantity of magnetism remain-

ing in it. A more powerful current was then passed, and the observations repeated as before; and so on with still more powerful currents. The fifth column of the table gives the effect upon the magnetic balance in grains, abstraction being made of the effect due to the coil itself, which had been previously carefully determined by experiment.

## Experiment 9. Pressure 82 lbs.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of pillar.	Total elongation.	Magnetic intensity of pillar.	Square of magnetic intensity divided by total elongation.
+ 6° 5'	106	0	0	+0.6	1960
0	0	0	0	+0.3	
+11 0	194	0.1 E.	0.1	+1.4	784
0	0	0.1 S.	0	+0.4	
+21 0	383	1.0 E.	1.0	+2.8	534
0	0	1.0 S.	0	+0.5	
+35 5	702	3.3 E.	3.3	+4.2	455
0	0	3.2 S.	0.1	+0.6	
+47 0	1072	5.4 E.	5.5	+5.0	429
0	0	5.1 S.	0.4	+0.7	
+53 45	1364	6.4 E.	6.8	+5.4	
0	0	6.0 S.	0.8	+0.7	
- 6 45	118	0	0	-0.3	720
0	0	0	0	+0.1	
-11 18	200	0.2 E.	0.2	-1.2	676
0	0	0.2 S.	0	-0.3	
-21 25	392	1.0 E.	1.0	-2.6	480
0	0	1.0 S.	0	-0.4	
-35 33	715	3.5 E.	3.5	-4.1	461
0	0	3.2 S.	0.3	-0.4	
-45 40	1023	4.9 E.	5.2	-4.9	449
0	0	5.0 S.	0.2	-0.5	
-54 5	1380	6.3 E.	6.5	-5.4	
0	0	6.3 S.	0.2	-0.5	

In the following experiment the same bar was subjected to a pressure of 480 lbs. It possessed -0.5 of permanent magnetism to begin with.



## Experiment 10. Pressure 480 lbs.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of pillar.	Total elongation.	Magnetic intensity of pillar.	Square of magnetic intensity divided by total elongation.
+ 6 10 0	108 0	0.15 S. 0	0 0	+0.6 +0.2	720
+10 45 0	190 0	0.2 E. 0.2 S.	0.2 0	+1.2 +0.5	
+20 0 0	364 0	0.8 E. 0.8 S.	0.8 0	+2.3 +0.7	661
+32 53 0	646 0	2.5 E. 2.5 S.	2.5 0	+3.6 +0.8	518
+42 35 0	919 0	4.6 E. 4.6 S.	4.6 0	+4.5 +0.8	440
+49 55 0	1188 0	6.8 E. 6.8 S.	6.8 0	+5.2 +0.8	398

The numbers in the last columns of the preceding tables show that the elongation follows a rather higher ratio than the square of the magnetic polarity. In the former section, in which all the bars employed were a yard long, the ratio was somewhat lower than that of the square of the polarity in the case of well-annealed iron. I am inclined therefore to think that the anomalies referred to at p. 83, were occasioned rather by the too great length of the iron bars, which prevented them from being magnetized as much at the ends as at the middle part, than by their different magnetic conditions at the centre and surface.

From the above tables it appears evident that the augmentation of *pressure* does not make much difference in the amount of elongation for the same quantity of polarity. However, I thought it desirable to try the effect of a greater pressure. For this purpose I employed a piece of soft iron wire, six inches long and one-fourth of an inch in diameter. This iron pillar stood upon a small piece of flattened brass, resting upon a block of hard wood six inches high, in order that it might be sufficiently elevated to support the lever. It was placed in the axis of a suitable coil,  $5\frac{1}{2}$  inches long and one inch in interior diameter, constructed of a covered copper wire twenty yards long and one-tenth of an inch in diameter.

## Experiment 11. Six-inch Pillar. Pressure 82 lbs.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of pillar.	Total elongation.	Magnetic intensity of pillar.	Square of magnetic intensity divided by total elongation.
+ 6° 30'	114	0	0	+0.8	
0	0	0	0	+0.2	
+11 15	199	0	0	+1.3	
0	0	0	0	+0.4	
+23 15	430	0	0	+2.9	504
0	0	0	0	+0.5	
+41 55	898	0.6 E.	0.6	+5.5	
0	0	0.6 S.	0	+0.5	
+51 50	1272	1.5 E.	1.5	+7.2	346
0	0	1.5 S.	0	+0.5	
+62 20	1907	2.6 E.	2.6	+9.4	340
0	0	2.6 S.	0	+0.5	

## Experiment 12. Six-inch Pillar. Pressure 1380 lbs. Permanent magnetism of the pillar to begin with -0.6.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of pillar.	Total elongation.	Magnetic intensity of pillar.	Square of magnetic intensity divided by total elongation.
+ 6° 40'	117	0	0	+0.1	
0	0	0	0	-0.3	
+11 30	203	0	0	+0.6	
0	0	0	0	-0.1	
+23 25	433	0	0	+2.1	506
0	0	0	0	+0.3	
+42 35	919	0.4 E.	0.4	+4.5	
0	0	0.4 S.	0	+0.5	
+51 30	1257	0.9 E.	0.9	+5.9	387
0	0	0.9 S.	0	+0.7	
+63 30	2005	2.1 E.	2.1	+8.6	352
0	0	2.1 S.	0	+0.7	

A comparison of the last columns of the two preceding tables will show that pressure has no sensible effect upon the extent of the elongation. I had not sufficient voltaic force to saturate the short bars, but there appears no reason to doubt, that their elongation, when saturated, would be one-half that of the pillars one foot long, just as the latter were found to experience one-third of the elongation observed in the bars of a yard long employed in the first section. I may remark in

this place, that the greater proximity of the magnetic balance to the coil increased the numbers in columns 5, representing the magnetic intensity of the six-inch pillars. The two preceding tables are therefore only comparable with themselves. In all the other experiments with one-foot bars, the magnetic balance was at the uniform distance of  $4\frac{1}{2}$  inches from the centre of the bar, so that they are strictly comparable with one another.

I now proceed to give an account of some experiments on the effects of the force of tension. The bar employed was a piece of soft iron wire  $12\frac{2}{3}$  inches long and a quarter of an inch in diameter. Its extremities were formed into very fine-threaded screws, extending one-third of an inch, for the purpose of screwing into the brass plate and bolt, as shown in the figure already described. The effectual length of the bar, when screwed into its place, was exactly one foot. In the first experiment of this kind, the tension employed, being that occasioned by the weight of the lever alone, amounted to 80 lbs. In the subsequent ones the tension was further increased by placing weights on the lever.

**Experiment 13. Iron wire one foot long and a quarter of an inch in diameter. Tension 80 lbs.**

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of wire.	Total elongation.	Magnetic intensity of wire.	Square of magnetic intensity divided by total elongation.
+ 6 30 0	114 0	0.1 E. 0.1 S.	0.1 0	+0.6 +0.2	360
+11 30 0	203 0	0.4 E. 0.4 S.	0.4 0	+1.5 +0.3	562
+21 25 0	392 0	1.0 E. 1.0 S.	1.0 0	+3.0 +0.4	900
+35 55 0	724 0	3.2 E. 3.2 S.	3.2 0	+4.5 +0.4	632
+46 38 0	1058 0	4.0 E. 4.0 S.	4.0 0	+5.1 +0.4	650
+53 10 0	1333 0	4.6 E. 4.6 S.	4.6 0	+5.4 +0.4	634
+61 25 0	1835 0	4.5 E. 4.5 S.	4.5 0	+5.6 +0.4	697

Experiment 14. Same wire with  $-0.4$  permanent magnetism to begin with. Tension 408 lbs.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of wire.	Total elongation.	Magnetic intensity of wire.	Square of magnetic intensity divided by total elongation.
+ 6 30	114	0	0	+0.6	
0	0	0	0	+0.2	
+11 10	197	0	0	+1.4	
0	0	0	0	+0.3	
+20 55	382	0.2 E.	0.2	+3.1	4805
0	0	0.2 S.	0	+0.4	
+35 25	702	0.6 E.	0.6	+4.7	3682
0	0	0.6 S.	0	+0.4	
+45 20	1012	0.8 E.	0.8	+5.1	3251
0	0	0.8 S.	0	+0.4	

Experiment 15. Same wire. Tension 740 lbs.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of wire.	Total shortening.	Magnetic intensity of wire.	Current multiplied by magnetic intensity divided by shortening effect.
+ 6 20'	111	0	0	+0.9	
0	0	0	0	+0.3	
+20 0	364	0.1 S.	0.1	+3.4	1237
0	0	0.1 E.	0	+0.5	
+35 0	700	0.4 S.	0.4	+4.9	858
0	0	0.4 E.	0	+0.6	
+45 30	1017	0.6 S.	0.6	+5.4	915
0	0	0.6 E.	0	+0.6	
+58 0	1600	1.3 S.	1.3	+5.7	701
0	0	1.3 E.	0	+0.7	

In tables 13 and 14 we notice the rapid decline of the effect of elongation, until at last, in table 15, with a tension of 740 lbs. it ceases altogether, and the new condition of shortening commences. With a tension of about 600 lbs. the effects on the dimensions of the wire would cease altogether in the limits of the electrical currents employed in the above experiments. From the last column of table 15, which consists of the columns 2 and 5 multiplied together and divided by column 4, we gather that the shortening effect is very nearly proportional to the magnetism of the wire into the current traversing the coil. The law of the square of the magnetism will still indeed hold good where the iron is sufficiently below



the point of saturation, on account of the magnetism being in that case nearly proportional to the intensity of the current. For the same reason, on examination of the previous tables, it will be found that the elongation is, below the point of saturation, very nearly proportional to the magnetism multiplied by the current. The necessity of changing the law arises from the fact that the elongation ceases to increase after the iron is fully saturated; whereas the shortening effect still continues to be augmented with the increase of the intensity of the current.

Experiment 16. Same wire. +0.5 magnetism to begin with. Tension 1040 lbs.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of wire.	Total shortening.	Magnetic intensity of wire.	Current multiplied by magnetic intensity divided by shortening effect.
- 6 20 0	111 0	0 0	0 0	-0.4 0	
-11 5 0	196 0	0.1 S. 0.1 E.	0.1 0	-1.3 -0.2	255
-20 55 0	382 0	0.2 S. 0.2 E.	0.2 0	-2.7 -0.4	515
-34 45 0	694 0	0.8 S. 0.8 E.	0.8 0	-4.0 -0.4	347
-46 45 0	1063 0	1.5 S. 1.5 E.	1.5 0	-4.7 -0.5	333
-61 0 0	1804 0	2.8 S. 2.8 E.	2.8 0	-5.0 -0.5	322
+20 45 0	379 0	0.3 S. 0.3 E.	0.3 0	+3.0 +0.5	379
+35 10 0	704 0	0.8 S. 0.8 E.	0.8 0	+4.4 +0.5	387
+48 5 0	1114 0	1.8 S. 1.8 E.	1.8 0	+4.9 +0.5	303
+61 20 0	1829 0	2.7 S. 2.7 E.	2.7 0	+5.4 +0.5	366

Experiment 17. Same wire. Tension 1680 lbs.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of bar.	Total shortening.	Magnetic intensity of wire.	Current multiplied by magnetic intensity divided by shortening effect.
— 6 25	112	0	0	—0·8	
0	0	0	0	—0·2	
—11 10	197	0	0	—1·5	
0	0	0	0	—0·4	
—20 45	379	0·5 S.	0·5	—2·7	205
0	0	0·5 E.	0	—0·4	0
—34 50	696	1·5 S.	1·5	—3·8	176
0	0	1·5 E.	0	—0·4	0
—45 5	1003	2·4 S.	2·4	—4·3	180
0	0	2·4 E.	0	—0·4	0
—52 25	1299	3·3 S.	3·3	—4·4	173
0	0	3·3 E.	0	—0·4	0
—61 15	1823	4·5 S.	4·5	—4·7	190
0	0	4·5 E.	0	—0·4	0
+61 45	1861	4·4 S.	4·4	+5·0	211
0	0	4·4 E.	0	+0·5	0

The uniformity of the numbers contained in the last column of each of the two preceding tables affords conclusive evidence of the correctness of the law I have stated, viz. that *in the case of tension the shortening effect is proportional to the current traversing the coil multiplied by the magnetic intensity of the bar.*

In order to discover, if possible, what proportion the shortening effect bears to the force of tension, I have constructed the following table from the results observed with currents of about 700 and 1000 intensity in the preceding experiments.

Number of experiment.	Elongation or shortening.	Departure from elongation 4·4.	Tension.	Square of departure divided by tension.
9	4·4 E.	0	0	
13	3·6 E.	0·8	80	80
14	0·7 E.	3·7	408	335
15	0·5 S.	4·9	740	324
16	1·2 S.	5·6	1040	301
17	2·0 S.	6·4	1680	244

With the exception of the number 80, the results of the last column of the above table agree sufficiently well together

to render it extremely probable that the shortening effects are proportional *cæteris paribus* to the square root of the force of tension.

*Experiments with Cast Iron.*

The following experiments were made with a bar of cast iron one foot long and a quarter of an inch in diameter.

Experiment 18. Cast iron. Tension 80 lbs.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of bar.	Total elongation.	Magnetic intensity of bar.	Square of magnetic intensity divided by total elongation.
- 6 28	113	0	0	-0.2	
0	0	0	0	-0.1	
-11 5	196	0	0	-0.4	
0	0	0	0	-0.2	
-21 18	390	0.1 E.	0.1	-0.9	810
0	0	0.1 S.	0	-0.5	
-36 0	726	0.8 E.	0.8	-2.0	500
0	0	0.5 S.	0.3	-1.3	563
-46 10	1041	1.0 E.	1.3	-2.5	481
0	0	1.0 S.	0.3	-1.5	750
-58 35	1637	1.7 E.	2.0	-3.0	450
0	0	1.7 S.	0.3	-1.7	963
+19 45	359	0.5 S.	-0.2	0	
0	0	0	-0.2	-0.4	
+59 25	1692	2.3 E.	2.1	+3.2	488
0	0	1.7 S.	0.4	+1.7	722

Experiment 19. Same bar. Tension 654 lbs. Permanent magnetism to begin with -1.7.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of bar.	Total elongation.	Magnetic intensity of bar.	Square of magnetic intensity divided by total elongation.
+19 15	349	0.2 S.	0	-0.1	
0	0	0	0	-0.4	
+57 50	1590	1.4 E.	1.4	+3.0	643
0	0	1.0 S.	0.4	+1.7	722

On comparing Experiment 18 with Experiment 13, it will be observed that the elongation of the cast iron is equal, if not

superior to that of the soft iron, when magnetized to the same extent. It will also be remarked that the increase of tension does not produce half the diminution of elongation which it does in the case of the soft iron.

*Experiments with Soft Steel Wire.*

The following experiments with soft steel wire were made in precisely the same manner as those with soft iron wire, already described.

**Experiment 20.** Soft steel pillar, one foot long, a quarter of an inch in diameter. Pressure 82 lbs.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of pillar.	Total elongation.	Magnetic intensity of pillar.	Square of magnetic intensity divided by total elongation.
-34 0	674	0.8 E.	0.8	-2.1	551
0	0	0.4 S.	0.4	-1.1	302
-47 0	1072	1.0 E.	1.4	-3.6	926
0	0	0.6 S.	0.8	-1.7	361
-59 30	1697	1.2 E.	2.0	-4.5	1012
0	0	0.6 S.	1.4	-2.4	411

**Experiment 21.** Same soft steel pillar. Pressure 480 lbs.  
Permanent magnetism to begin with -2.0.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of pillar.	Total elongation.	Magnetic intensity of pillar.	Square of magnetic intensity divided by total elongation.
+18 15	330	0.2 S.	0	-0.2	
0	0	0	0	-0.4	
+30 30	589	0.1 E.	0.1	+1.3	1690
0	0	0	0.1	+0.6	360
+40 0	839	0.4 E.	0.5	+2.6	1352
0	0	0.2 S.	0.3	+1.3	566
+45 0	1000	0.6 E.	0.9	+3.1	1068
0	0	0.6 S.	0.3	+1.6	853
+61 10	1816	1.8 E.	2.1	+4.1	800
0	0	1.6 S.	0.5	+2.0	800



Experiment 22. Soft steel wire, one foot long, a quarter of an inch in diameter. Tension 80 lbs.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of wire.	Total elongation.	Magnetic intensity of wire.	Square of magnetic intensity divided by total elongation.
+19° 50'	360	0	0	+1.0	
0	0	0	0	+0.5	
+34 40	691	0.4 E.	0.4	+2.6	1690
0	0	0.1 S.	0.3	+1.5	750
+46 10	1041	0.5 E.	0.8	+3.5	1531
0	0	0.2 S.	0.6	+1.9	601
+56 30	1511	0.5 E.	1.1	+4.0	1455
0	0	0.3 S.	0.8	+2.1	551
-20 50	380	0.8 S.	0	-0.2	
0	0	0	0	+0.3	
-34 50	696	0.2 E.	0.2	-2.3	2645
0	0	0.1 S.	0.1	-1.1	1210
-47 45	1101	0.7 E.	0.8	-3.6	1620
0	0	0.3 S.	0.5	-2.0	800
-60 0	1732	0.7 E.	1.2	-4.4	1618
0	0	0.2 S.	1.0	-2.4	576

Experiment 23. Same soft steel wire. Tension 462 lbs.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of wire.	Total elongation.	Magnetic intensity of wire.	Square of magnetic intensity divided by total elongation.
+34° 55'	698	0	0	+2.6	
0	0	0.1 E.	0.1	+1.4	1960
+45 5	1008	0	0.1	+3.4	
0	0	0.2 E.	0.3	+2.0	1333
+60 15	1750	0.4 S.	-0.1	+4.2	
0	0	0.6 E.	0.5	+2.4	1152

Experiment 24. Same soft steel wire. Tension 1680 lbs.  
Permanent magnetism to begin with +1.8

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of wire.	Total shortening effect.	Magnetic intensity of wire.	Current multiplied by magnetic intensity divided by shortening effect.
-21° 13'	388	0.2 S.	0.2	-0.4	77
0	0	0.2 E.	0	+0.2	
-35 13	706	0.8 S.	0.8	-2.2	194
0	0	0.8 E.	0	-1.1	
-44 35	985	1.3 S.	1.3	-2.9	219
0	0	1.3 E.	0	-1.6	
-58 45	1648	2.5 S.	2.5	-3.7	244
0	0	2.5 E.	0	-2.2	

From the above experiments it will be seen that the same remarks which were made with regard to the soft iron, apply also to the soft steel. The superior retentive power of the latter metal enables us however to trace the elongating effects of the permanent magnetism, which, we see, diminish with the increase of tension, until at last, as in experiment 24, they cease altogether.

*Experiments with Hardened Steel Wire.*

The following experiments were made with steel wire of the same kind as that employed in the previous experiments. It was however perfectly hardened, so as to resist the action of the file in every part.

Experiment 25. Hardened steel wire, one foot long, a quarter of an inch diameter. Tension 80 lbs.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of wire.	Total shortening.	Magnetic intensity of wire.	Current multiplied by magnetic intensity divided by shortening effect.
+ 6 15	109	0	0	0	
0	0	0	0	0	
+11 0	194	0	0	+0.2	
0	0	0	0	0	
+20 50	380	0	0	+0.5	
0	0	0	0	+0.2	
+35 20	709	0	0	+1.1	
0	0	0	0	+0.5	
+45 40	1023	0.1 S.	0.1	+1.8	1841
0	0	0.1 E.	0	+0.9	
+52 0	1280	0.2 S.	0.2	+2.3	1472
0	0	0.2 E.	0	+1.2	
+62 20	1907	0.4 S.	0.4	+3.3	1573
0	0	0.4 E.	0	+1.9	

Experiment 26. Same hardened steel wire. Permanent magnetism to begin with  $-1.8$  Tension 408 lbs.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of wire.	Total shortening.	Magnetic intensity of wire.	Current multiplied by magnetic intensity divided by shortening effect.
+19 30	354	0	0	-1.1	
0	0	0	0	-1.4	
+35 40	717	0	0	-0.2	
0	0	0	0	-0.6	
+46 50	1066	0	0	+1.2	
0	0	0	0	+0.2	
+60 20	1755	0.4 S.	0.4	+2.8	1228
0	0	0.4 E.	0	+1.5	

Experiment 27. Same hardened steel wire. Permanent magnetism to begin with +1.3. Tension 1030 lbs.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of wire.	Total shortening.	Magnetic intensity of wire.	Current multiplied by magnetic intensity divided by shortening effect.
-21 15	389	0	0	+0.7	
0	0	0	0	+1.1	
-35 55	724	0	0	-0.1	
0	0	0	0	+0.4	
-48 33	1132	0.1 S.	0.1	-1.3	1470
0	0	0.1 E.	0	-0.5	
-52 10	1287	0.15 S.	0.15	-1.6	
0	0	0.15 E.	0	-0.6	
-61 10	1816	0.4 S.	0.4	-2.4	
0	0	0.4 E.	0	-1.2	1089
+35 45	720	0	0	+0.2	
0	0	0	0	-0.4	
+61 10	1816	0.4 S.	0.4	+2.7	
0	0	0.4 E.	0	+1.3	

From the above experiments we find that the induction of permanent magnetism produces no sensible effect on the length of a bar of perfectly hardened steel, and that the temporary shortening effect of the coil is proportional to the magnetism multiplied by the current traversing the coil. The shortening effect does not in this case sensibly increase with the increase of tension. I have made an experiment, in which a hard steel pillar was subjected to a pressure of 80 lbs., which I have not inserted, because the coil not being the same as I had hitherto employed, that experiment was not strictly comparable with the rest. Its result however showed that the hard steel pillar suffered a diminution of length equal to 0.1 of a division of the micrometer, with a current capable of giving a magnetic polarity of 1.7. This accords very well with the results obtained with a tension of 1030 lbs.

Copper is the only non-ferruginous metal which I have experimented on. In the trials made with wires of this metal, pressure and tension were successively applied, and very powerful currents transmitted through the coil; but I could in no case detect the slightest alteration in their dimensions.

I shall not prolong this paper by a discussion of the probable cause of the remarkable facts I have pointed out. The law of *elongation* naturally suggests the joint operation of the attractive and repulsive forces of the constituent particles of the magnet as the cause of that phænomenon. On the other

hand, the fact that the *shortening effect* is proportional to the magnetic intensity of the bar multiplied by the current traversing the coil, seems to indicate that, in this case, the effect is produced by the attraction of the magnetic particles by the coil. But then it will be asked, why so remarkable an augmentation of the effect is produced by the increase of tension in the case of the soft iron bars? When we are able to answer this question in a satisfactory manner, we shall probably have a much more complete acquaintance with the real nature of magnetism than we at present possess.

Fig. 2.



*Postscript.*

I have already, in the former part of this paper, described an experiment which indicated that no alteration in the *bulk* of a bar of soft iron could be produced by magnetizing it. I thought, however, that it would be interesting to confirm the fact by an observation of the alteration of the dimensions of the iron at right angles to the direction of its polarity. For this purpose I took a piece of drawn iron gas-piping one yard long,  $\frac{3}{16}$ ths of an inch in bore, and  $\frac{5}{16}$ ths of an inch in thickness. A piece of thick covered copper wire was inserted into this tube, and bent over the outside of it in the manner indicated by fig. 2. The lower extremity of the iron tube being fixed, and the upper end being attached to the micrometrical apparatus described in the first section of this paper, each division of which corresponded to  $\frac{1}{138328}$ th of an inch, I obtained the following results:—

Experiment 28. Drawn iron tube.

Deflection of galvanometer.	Tangent of deflection.	Shortening or elongation.	Total shortening effect
+62 30 0	1921 0	3.2 S. 1.2 E.	3.2 2.0
+68 30 0	2539 0	1.0 S. 1.0 E.	3.0 2.0
— 5 0 0	87 0	0 0	2.0 2.0
—15 30 0	277 0	0.2 E. 0	1.8 1.8
—30 0 0	577 0	1.0 E. 0	0.8 0.8
—61 30 0	1842 0	2.0 S. 1.0 E.	2.8 1.8



## Experiment 28 (continued).

Deflection of galvanometer.	Tangent of deflection.	Shortening or elongation.	Total shortening effect.
+15 30	277	0.1 E.	1.7
0	0	0	1.7
+30 20	585	0.7 E.	1.0
0	0	0	1.0
+45 30	1017	0.1 S.	1.1
0	0	0.1 E.	1.0
+60 0	1732	1.2 S.	2.2
0	0	0.6 E.	1.6
+72 30	3171	1.8 S.	3.4
0	0	0.8 E.	2.6

The results of the above table show that the length of the tube was diminished, in order to make up for the increase of its diameter, which, in this instance, was in the direction of the polarity. The quantity of the shortening effect, viz. 3.4, is however only one-third of that due to the maximum elongation of soft iron bars as observed in the first section. This is probably owing to the grain of the iron being in cross directions with respect to the polarity in the two cases; and partly perhaps to the iron tube not being fully saturated with magnetism. The experiment is worth repeating, especially as it affords a means of studying the magnetic condition of closed magnetic circuits.

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XXXIX. *Second Report of Proceedings in the Cambridge Observatory relating to the New Planet (Neptune).* By the Rev. J. CHALLIS, M.A., Plumian Professor of Astronomy in the University of Cambridge\*.

IN conformity with a wish expressed by the Vice-Chancellor and the Observatory Syndicate at their ordinary terminal meeting, held on March 15, I propose in this report to carry on, for the information of members of the senate, the account of proceedings in the observatory relative to the new planet, a first report of which was made on December 12 of last year. The theoretical grounds on which a search for the planet was instituted, the manner in which the search was conducted, and the degree of success that attended it, were stated in the former report, which brought the history of proceedings down to the date at which the planet was discovered. I have now to give an account of the subsequent observations both of its position in the heavens, and of its physical appearance, and to state the

\* Communicated by Professor Challis.

*Phil. Mag.* S. 3. Vol. 30. No. 201. April 1847.

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results respecting the orbit which have been deduced from the observations by calculation.

A regular series of observations of the planet was commenced on October 3, 1846, and continued at all available opportunities, partly with the meridian instruments, and partly with the Northumberland equatoreal, to December 4, soon after which the planet became too faint to observe on the meridian on account of day-light. The observations were subsequently carried on with the equatoreal to January 15. The series was much interrupted by cloudy weather, particularly in the months of December and January. On the whole I have obtained twenty-eight positions of the planet with the meridian instruments, and twenty-five positions with the Northumberland equatoreal by means of ninety-two differential observations of right ascension and as many of north polar distance. The equatoreal measures were all referred to the same star, No. 7648 of the British Association Catalogue, the exact place of which was determined by sixteen observations with the transit, and eight observations with the mural circle. I have reason to think that the positions obtained with the equatoreal are entitled to very nearly the same weight as those obtained on the meridian. All the above observations I have completely reduced, and have placed the results at the disposal of Mr. Adams for deducing elements of the planet's orbit.

On January 12, I had for the first time a distinct impression that the planet was surrounded by a ring. The appearance noticed was such as would be presented by a ring like that of Saturn, situated with its plane very oblique to the direction of vision. I felt convinced that the observed elongation could not be attributed to atmospheric refraction, or to any irregular action on the pencils of light, because when the object was seen most steadily I distinctly perceived a *symmetrical* form. My assistant, Mr. Morgan, being requested to pay particular attention to the appearance of the planet, gave the same direction of the axis of elongation as that in which it appeared to me. I saw the ring again on the evening of January 14. In my note-book I remark, "The ring is very apparent with a power of 215, in a field considerably illumined by lamp-light. Its brightness seems equal to that of the planet itself." On that evening, Mr. Morgan, at my request, made a drawing of the form, which on comparison coincided very closely with a drawing made independently by myself. The ratio of the diameter of the ring to that of the planet, as measured from the drawings, is about that of 3 to 2. The angle made by the axis of the ring with a parallel of declination, in the south-preceding or north-following quarter, I estimated at  $60^\circ$ .

By a measurement taken with the position circle on Jan. 15, under very unfavourable circumstances, this angle was found to be  $65^{\circ}$ . I am unable to account entirely for my not having noticed the ring at an earlier period of the observations. It may, however, be said that an appearance like this, which it is difficult to recognise except in a good state of the atmosphere, might for a long time escape detection, if not expressly and repeatedly looked for. To force itself on the attention, it would require to be seen under extremely favourable circumstances. Previous to the observations in January, the planet had been hid for more than three weeks by clouds. The evenings of January 12 and 14 were particularly good, and the planet was at first looked at in strong twilight. Under very similar circumstances I have twice seen with the Northumberland telescope the second division of Saturn's ring,

I communicated to Mr. Lassell of Liverpool, who was the first to suspect the existence of a ring, my observations upon it, accompanied with a drawing; and I have received from him in return a drawing of the appearance presented in his twenty-feet reflector, closely resembling mine both as to the form and the position of the ring. Mr. Lassell writes, "I cannot refuse to consider that your observation puts beyond reasonable doubt the reality of mine." In this conclusion I concur, and accordingly in communications to the Royal Astronomical Society and to Schumacher's *Astronomische Nachrichten*, containing my reduced observations, I have ventured to express my conviction of the existence of a ring.

By micrometer measures taken with the Northumberland telescope, I find the apparent diameter of the body of the planet to be very nearly  $3''$ .

The above account includes all the observations on the planet I could obtain before its disappearance in the solar rays. By the kindness of Mr. Adams I am able to add some particulars respecting its orbit, which he has derived by calculation from the reduced places with which I furnished him. As was stated in the former report, Mr. Adams calculated first approximations to the elements, by employing the places I obtained on August 4 and 12 in the course of searching for the planet, with observations since the discovery extending to October 13. For the sake of comparison with the second approximations, I now give the first results.

Heliocentric longitude . . . .	$326^{\circ} 39'$	Aug. 4, 1846.
Longitude of the descending node.	$309^{\circ} 43'$	
Inclination of the orbit . . . .	$1^{\circ} 45'$	
Distance of the planet from the sun	$30.05$	

In calculating the following second approximations, Mr. Adams used the mean of the two places of August as a single place, and of the others he selected nine which seemed to be the best determined, and which were separated by convenient intervals. All the results are calculated for the epoch of 1846, August, 8.0 mean time at Greenwich.

Heliocentric longitude of the planet referred	°	'	"
to the mean equinox of 1847.0 . . . . .	326	41	12.3
Heliocentric motion in longitude in 100 days	36	5.52	
Heliocentric latitude south . . . . .	30	34.4	
Change of heliocentric latitude in 100 days .	1	4.44	
Longitude of the descending node . . . . .	310	3	44.0
Inclination of the orbit . . . . .	1	46	49.1
Distance of the planet from the sun . . . .	30.008		
Half the latus rectum of the orbit . . . .	30.228		

The first position on which the above results depend, that of August 4, was obtained sixteen days before the planet was in opposition, and the last position, that of January 15, thirty-two days before it was in conjunction. The great variation of the planet's elongation from the sun in this interval, is favourable to the correctness of the above determinations, which, although they cannot pretend to extreme accuracy on account of the short period over which the observations extend, are yet entitled to considerable weight. Mr. Adams has in fact calculated the probable errors of the above results by supposing each observation of right ascension or of north polar distance to be liable to an error of 3"; and he finds that there is little probability of their receiving any great amount of correction by taking account of future observations. It may be remarked that the first and second approximations do not differ by any large quantities. Hence it may be inferred that the places of August are deserving of confidence, and that on account of the extension given to the period of observation by including those places, this second approximation to the elements is more accurate than it would have been if it depended solely on observations made since the discovery of the planet.

The calculations give 59' 8" for the planet's heliocentric motion from August 4 to January 15. This is so small an arc that it is not possible to deduce with any degree of certainty those elements the determination of which depends on change of the heliocentric distance. Mr. Adams has, however, discussed the observations with this object in view, and has obtained certain limiting results, which, as possessing considerable interest, I here subjoin.



The eccentricity of the orbit cannot exceed 0.18. The most probable value is 0.06, which differs but little from the eccentricities of the orbits of Jupiter, Saturn and Uranus.

The most probable longitude of perihelion is  $49^{\circ} 58'$ , and the probable true anomaly  $276^{\circ} 43'$ , according to which the planet is near the extremity of the latus rectum and is descending towards perihelion. These results are extremely uncertain.

The mean distance is 30.35, with a probable error of 0.25; and the corresponding sidereal period is 167 years, with a probable error of about two years. It is remarkable that the periodic time is very nearly double that of Uranus; so that these two bodies will offer an instance of mutual perturbations of large amount, differing in character from those of the other planets, but analogous to the mutual perturbations of the first and second, and second and third satellites of Jupiter.

According to Bode's law of the planetary distances, the mean distance of the new planet should be nearly 38. The actual mean distance differs so much from this, that we are compelled to conclude that this singular law, which holds with reference to the other planets, fails in this instance.

Since the apparent diameter of the new planet is to that of Uranus nearly in the ratio of 3 to 4, according to the foregoing determination of the distance its bulk is to that of Uranus in the ratio of 8 to 5.

The above is the sum of the results derivable from the first series of observations. For further and more exact information we must wait till the planet emerges from the solar rays. Before concluding this report, I am desirous of saying a few words respecting the *name* of the planet. I recently had the satisfaction of receiving from M. Struve the copy of a communication read by him at the general annual meeting of the Imperial Academy of Sciences of St. Petersburg, on December 29, in which he states the reasons that have induced himself and the other Poulkova astronomers to adhere to the name of *Neptune*, which name was first proposed by the French Board of Longitude, shortly after the discovery of the planet. These reasons are thus briefly expressed in a note addressed to me personally: "The Poulkova astronomers have resolved to maintain the name of Neptune, in the opinion that the name of Le Verrier would be against the accepted analogy, and against historical truth; as it cannot be denied that M. Adams has been the first theoretical discoverer of that body, though not so happy as to effect a direct result of his indications." M. Struve's communication has been published in this country by the Astronomer Royal, who has expressed his assent to

the reasons therein contained, and his determination to adopt the name of Neptune. Prof. Gauss and Prof. Encke have also, as I understand, adopted this name. I have only to add that it is my intention (and I am permitted to say, the intention of Mr. Adams also) to follow the example set by these eminent astronomers.

Cambridge Observatory,  
March 22, 1847.

*XL. On the Hourly Alterations of the Vapour Atmosphere at Bombay. By THOMAS HOPKINS, Esq.\**

[With a Plate.]

**I** HAVE already availed myself of the observations made at Bombay under the superintendence of Dr. Buist, through a paper on the meteorology of that place by Colonel Sabine, on which I made some remarks that were inserted in the *Philosophical Magazine* for December 1846. But since those remarks were written I have had transmitted to me, through the kindness of Colonel Sykes, a lithographed copy of the observations themselves, in which are to be found important facts connected with the hourly production of aqueous vapour at Bombay, and its apparent influence on the movements of the barometer.

In addition to the hourly registrations of other meteorological instruments, Dr. Buist has furnished columns of the heights of the wet- and dry-bulb thermometers, showing the hourly depression of the wet below the dry thermometer for each month of the year 1843. From these columns, it appears that the wet was depressed below the dry instrument, to a certain extent varying both with the time of the day and the season of the year. The depression was the least in the wet season and the greatest in the dry one; and, with reference to the diurnal changes, the depression was generally, but not uniformly, the least about sun-rise and the greatest near to mid-day. These depressions of the wet below the dry thermometer are known to be results of the cooling power of evaporation of water on the bulb of the wet instrument; and the cooling thus produced is proportioned to the extent of evaporation of water that takes place; being small when the evaporation is little, and great when it is much. Thus the registration of the wet-bulb thermometer not only furnishes us with means of determining how far evaporation kept down the temperature of the thermometer exposed to its influence, but

\* Communicated by the Author.



also of ascertaining what were the relative amounts of water evaporated during the different portions of the time,—the cooling being the effect of the conversion of certain quantities of water into aqueous vapour, and being in every separate part of that time proportioned to the quantity converted.

We have also in a separate column of Dr. Buist's returns, the difference between the dry and the wet thermometers; and this difference may be taken to express the relative quantities of water evaporated, and the force of evaporation. From about six in the morning, varying with the season, this difference generally increases until, say twelve or one o'clock in the day, when it declines until the following morning.

As already stated, evaporation is less in the wet than in the dry season; and there is also less difference between the morning and mid-day force of evaporation in the former than in the latter season. The cold part of the year is the period for the land and sea breezes, and also for the extensive daily fluctuations of the barometer; and Dr. Buist says that this season lasts five months, namely, during October, November, December, January and February, forming the winter of the part. But of these October is wavering and uncertain; we may therefore consider the other four months as the season when the sea and land breezes blow in the most decided manner, and in which the greatest daily fluctuations of the barometer take place; it is consequently desirable that we should carefully examine the returns for the whole of these months. To do this, we will take the mean hourly difference between the dry and wet thermometers for each month, and by adding them together and dividing by four, obtain the mean hourly force of evaporation for the winter season. This I have done, and the result is given in the following table, to which are added in other columns the mean hourly heights of the barometer, thermometer and wet-bulb thermometer, for the same period, that the whole may be seen at one view in juxtaposition. The same facts are shown in a diagram (Plate IV.), in which the curve of the wet-bulb thermometer is exhibited in accordance with the figures of the table; and also as a base line, from which the distance of the dry thermometer is shown, the intermediate space marking the relative force of evaporation in each hour of the day.



Table of the Mean Hourly state of the following instruments, and of the force of evaporation for the four winter months of 1843.

Hours.	Barometer.	Thermometer.	Wet-bulb thermometer.	Evaporation.
4 A.M.	29.838	74.9	68.4	6.5
5 A.M.	29.848	74.6	67.8	6.8
6 A.M.	29.867	74.1	67.4	6.7
7 A.M.	29.889	73.6	67.3	6.3
8 A.M.	29.911	74.9	68.1	6.8
9 A.M.	29.928	77.0	68.9	8.1
10 A.M.	29.928	78.4	69.1	9.3
11 A.M.	29.911	79.6	69.4	10.2
12 A.M.	29.884	80.8	69.5	11.3
1 P.M.	29.853	81.6	70.4	11.2
2 P.M.	29.827	82.2	71.9	10.3
3 P.M.	29.817	82.2	72.6	9.6
4 P.M.	29.815	81.9	72.8	9.1
5 P.M.	29.817	81.0	72.4	8.6
6 P.M.	29.835	79.6	72.2	7.4
7 P.M.	29.854	78.8	71.9	6.9
8 P.M.	29.874	78.4	71.6	6.8
9 P.M.	29.886	77.7	70.9	6.8
10 P.M.	29.887	76.9	69.8	7.1
11 P.M.	29.881	76.4	69.2	7.2
12 P.M.	29.873	76.1	69.0	7.1
1 A.M.	29.862	76.0	69.2	6.8
2 A.M.	29.848	75.7	68.9	6.8
3 A.M.	29.841	75.3	68.5	6.8

By examining this table we may see that evaporation at four in the morning is  $6^{\circ}5$ , from which it rises, until at ten o'clock it is  $9^{\circ}3$ , being an increase of  $2^{\circ}8$ ; and during this time the barometer is admitted to be raised to the height that it attains, 29.928 inches, through the pressure of the vapour that has been recently produced by evaporation and thrown into the atmosphere.

From ten to twelve o'clock, evaporation, as measured by the difference between the two thermometers, increases up to  $11^{\circ}3$ , being a further rise of  $2^{\circ}$ ; but at the same time the barometer, instead of rising higher, as might be expected, falls no less than .044, as by that hour it sinks to 29.884 inches.

Evaporation becomes rather less active after twelve o'clock; but it still shows great force, and continues to exhibit it till four o'clock, at which time it is  $9^{\circ}1$ , being only .2 less than it was at ten in the morning. Yet during the whole of the time from ten in the morning to four in the afternoon, when evaporation was so energetic, the barometer was falling, and sunk no less than .113. What then became of the vapour

that was produced and discharged into the atmosphere within this period? The quantity passed into the atmosphere in the six hours must have been large, it being the product of an average evaporation of  $10^{\circ}3$  for the whole time; whilst the average for the previous six hours was only  $7^{\circ}3$  when the barometer was rising. Now, can it be supposed that a comparatively small additional quantity of vapour raised the barometer considerably from four to ten in the morning, and a further large addition to that quantity, acting in the same way from ten to four in the afternoon, not only ceased to raise it, but was attended by a fall of that instrument to an extent greater than the previous rise,—without some other cause coming into operation? It cannot be maintained that the increase of surface thermometric temperature had but little counteracting effect before ten, and great counteracting effect after that time until four o'clock. For the rise of the thermometer from four to ten in the morning was little less than it was from ten in the morning to two in the afternoon,—the time of the highest temperature,—it having risen so much as  $3^{\circ}5$  in the former, and only  $3^{\circ}8$  in the latter period. And it should be remembered that evaporation from ten to four was constantly adding fresh vapour to that which was previously in the atmosphere, where the whole was accumulating, and pressing with its aggregate weight on the barometer. There is, therefore, in the facts presented to us in the tables, no countenance for the supposition that temperature, as measured by the thermometer near the surface of the earth, counteracted the increase of vapour pressure after ten o'clock, and caused the fall of the barometer.

It is however sufficiently evident, from the facts given, that the vapour produced in the morning constituted the material which supplied the heat that at this time rendered the atmosphere warm and light in the locality, and caused the barometer to fall.

When the sun advances above the horizon it warms the surface of the globe, and not only increases evaporation of water, but heats that portion of the atmospheric gases that is near the surface,—which portion rises probably in separate streams or columns,—cooler columns at the same time descending to the surface and taking the place of the warmer. This process goes on as the sun rises higher, until at some particular time, depending on the locality and season, the ascending columns reach a height sufficient to enable the expanding gases to cool and condense some of the vapour which is intermingled with them. This ordinarily takes place from nine to eleven o'clock, or say at ten in the morning, from

which hour condensation is warming the atmospheric mass in the locality. This warmed mass, a mixture of gases and vapour, as it ascends is acted on by the different laws of cooling, of condensation, and of expansion by heat, which the constituent parts of the atmosphere obey; and the result is the formation of a buoyant column of cloud, of greater or less thickness, according to the quantity of vapour that has been condensed. The whole local column being thus made lighter by the liberated heat, it presses with less force on the surface of the earth, and consequently on the barometer. This diminished pressure is however effected through the liberated heat driving a portion of the material of the atmosphere, the gases, from the heated part to other parts of the atmospheric space; and thus we find that the heat just liberated by the condensation of vapour counteracts the increased pressure of the aqueous matter, which is at the same time passing into the atmosphere in the form of vapour. For it is here contended that the vapour that had been produced from four to ten in the morning, is, soon after the last-named hour, not only raised but condensed,—deprived of a part of its heat, and converted into minute particles of water, which float in the gaseous atmosphere as a cloud; and as such, undoubtedly form a part of the whole atmosphere and contribute to its weight. It is not therefore through a reduction in the quantity of aqueous matter in the local atmosphere, at this period of the day, that the barometer falls; but that fall is caused by the expanding power of liberated heat driving from the heated vertical column a part of the ponderable gases which previously existed within it, and, in that way, by removing a part of the material of the atmosphere, causing the remainder to press with less weight on the barometer.

The quantity of vapour that passes daily into the atmosphere while the temperature is rising, and which does not fall as rain, is returned to the earth as dew on its surface; and thus an equilibrium is established between the production and the condensation of vapour; but this does not take place during the period of which we have been treating, that is to say, from four o'clock in the morning to four in the afternoon. Vapour is not daily abstracted from the atmosphere by the formation of dew on the surface of the earth, until the barometer ceases to fall at four o'clock in the afternoon. At present we have to consider the influence of vapour during two periods of six hours each,—that in which the barometer is rising from four to ten in the morning, and that in which it is sinking from ten to four in the afternoon: and it has been shown that aqueous matter during the whole of this time was

increasing in quantity and accumulating in the atmosphere; and as far as that matter influenced the atmosphere, it must have increased its weight. The aggregate pressure of aqueous matter did not cease to increase at ten o'clock in the morning, when the barometer began to fall; that fall was produced by a new cause which then came into operation in the locality, namely, the expanding and displacing power of liberated heat.

It will probably be asked whether cloud forms near to, or over Bombay, after ten o'clock, and increases till four, as assumed? and to this inquiry I cannot give an answer that is likely to be quite satisfactory, not having full information on the subject. Dr. Buist, in his introductory remarks, does not particularly notice the formation of clouds within that portion of the day. In the hourly-observation tables there is a column for describing the appearance of clouds; and the entries in that column show that the cumulus, the cirro-cumulus, and the cirro-stratus were often formed, but they are not described in such a way as would justify me in adducing them as proofs of the agency here ascribed to them, although they do furnish rather strong presumptive evidence on the subject.

Clouds form pretty freely from ten in the morning till four in the afternoon, whilst from ten at night to four in the morning the atmosphere is represented as being generally clear: and taking the accounts given of the clouds during the two periods, we are fully authorized to say that they formed and remained suspended in the air during the former period, and were dissolved before the arrival of the latter period; showing that there were daily formation of cloud by condensation of vapour, and daily dissipation of those clouds by evaporation.

But in addition to the daily formation of visible clouds, there are indications that condensation takes place, but only to such an extent as to produce a haziness or misty appearance in the sky; and if this process is carried on until a considerable height is attained, it must warm the atmospheric mass and lighten it. Indeed, at the commencement of the process of gradual condensation, the cloud that is thereby formed is not seen; as it is only when a sufficient stratum of floating globules of water is produced that the cloud becomes visible. The first slight falling of the barometer before rain is probably caused in this way.

Judging from analogous cases, it may be presumed that palpable daily clouds at Bombay were first formed near the high ground to the east of that place, as such clouds in other similar places generally form near to, or against the sides of hills. Our old navigators have described such formations in



many parts of the world; but the following account given by Hutchison of Glasgow, contains a description of that which ordinarily takes place in localities similar to that of Bombay, although the distance of the mountains from the places of observation in the two cases, and the influences of the trade-winds may modify the process. Mr. Hutchison says that "the formation of clouds is finely illustrated by the phænomena daily exhibited during the dry season over what are called the Liguana, or Port Royal Mountains in the island of Jamaica. These mountains are situated about four or five miles to the north-east by east of Kingston, the principal port in the island, and their height above the level of the sea is about 4000 to 5000 feet. During the dry season, from the beginning of November till the middle of April, the sea and land breezes alternately succeed each other with an intermediate interval of atmospheric stillness, in the following manner. From sun-rise till about ten o'clock in the forenoon it is usually perfectly calm. About ten o'clock, the sea breeze, blowing at Kingston from the east, or a little to the south of east, commences and continues till about half-past three in the afternoon, when it gradually and entirely subsides." Again, "About eleven o'clock every forenoon, or between that and mid-day, the summits of the Port Royal Mountains begin to be covered with clouds, which, though thin, fleecy and transparent at first, gradually increase in density till about one o'clock. By this time the upper portions of the mountain, when viewed from Kingston, seem to be wholly enveloped in dense clouds, rain is apparently falling in torrents, flashes of lightning are seen, and the sound of distant thunder is heard. About half-past two o'clock in the afternoon, the clouds, gradually diminishing in density, begin to quit the mountains; so that their summits again become visible, as in the morning, and so continue till about eleven o'clock the following day. The clouds after quitting the mountains rise gradually to a greater altitude and float very slowly westward, assuming as they proceed the appearance of large heaped-up cumuli." See Hutchison on Meteorological Phænomena, p. 64.

The general trade-wind about Jamaica was from the east, and it bore the ascending clouds to the west in the afternoon: the trade-wind at Bombay was ordinarily from north of west, and it would doubtless modify the influence of the mountains on the clouds formed in the part. Were meteorological instruments corresponding with those kept at Bombay placed to the east of that place at the rise of the hills, and registered in the same way as at Bombay, it would probably throw further light on the daily atmospheric disturbances in this part

of the world, and enable us to form a better judgement of the alterations which take place in other parts. The daily range of the barometer at Poonah, which is on the eastern side of the ridge of the Ghauts, and far from the sea, is about as great as it is at Bombay. There can be no sea breeze at Poonah, as the mountain range is between it and the sea; but are there not diurnal winds of similar character to those on the coast? Corresponding registrations at Poonah, Bombay, and some intervening place on the west of the Ghauts, might furnish valuable additions to our stock of meteorological information, and enable us to trace the operating causes as they pass from one meridian to another, and might thus furnish us with more conclusive evidence of the nature and causes of the hourly alterations which occur in the atmosphere than any that can be adduced at present.

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XLI. *Remarks on the Extractive Material of Urine, and on the Excretion of Sulphur and Phosphorus by the Kidneys in an unoxidized state.* By EDMUND RONALDS, Ph.D., Giessen\*.

SOME months back, at the instigation of Dr. Golding Bird, I undertook some experiments to ascertain whether, in cases of diseased and imperfect function of the lungs or liver, when the normal quantity of carbon could not be discharged from the system by those channels, the kidneys undertook an extra duty, and whether under such circumstances an excess of carbon could be shown in the urine above that usually secreted under healthy conditions.

Should this question be answered in the affirmative, and should it be found that a larger amount of carbon was excreted by the urine in persons affected with such diseases, a practical application might reasonably be made of the fact. For by stimulating the kidneys to still greater exertion, the amount of work required of the lungs or of the liver could be lessened, and thus a better chance offered them of being restored to a healthy state.

The method proposed for solving this problem was, to precipitate the urine of different patients suffering from diseases of the kinds mentioned, with basic acetate of lead, keeping it slightly alkaline by the addition of a few drops of ammonia, then to ascertain the amount of organic matter contained in the precipitate, and in particular the amount of carbon, and lastly to compare these quantities with those obtained in a similar manner from the urine of healthy individuals.

\* From the Philosophical Transactions for 1846, part iv.; having been received by the Royal Society April 25, and read June 18, 1846.

In endeavouring to determine the amount of organic matter in the lead precipitate by burning, as likewise in determining the quantity of carbon by an elementary analysis of the same precipitate with oxide of copper, results were obtained which did not agree, and many difficulties arose which it is not necessary to state now, but which rendered it absolutely necessary to separate by some means the organic matter from the oxide of lead before submitting it to analysis, and even that we might obtain accurately its quantity. Whilst employed in seeking an accurate mode of separation, Dr. Scherer's paper appeared on the extractive matters of urine\*, in which a successful mode of separation is described, and the question at issue answered. Dr. Scherer finds that the extractive or colouring matter of the urine contains a larger quantity of carbon and hydrogen when obtained from persons in whom the normal function of the lungs, of the liver or of the skin is deranged, than when taken from healthy subjects, and that the same excess of carbon passes off by the urine when the diet is more than usually rich in that element. From his paper however it does not appear that the quantity of this extractive or colouring matter passed during a certain space of time has been ascertained, and it strikes me that he assumes the quantity of extractive to be the same in all kinds of urine; this I think requires to be proved before it can be positively affirmed that more carbon and hydrogen do pass off by the urine in such diseased conditions, as a larger quantity of less highly carbonized extractive matter might compensate for the excess of carbon in the more highly carbonized, supposing the latter to be in less quantity. The relative quantity of these matters, and likewise the relative quantities of urea in a certain amount of urine, must be accurately determined before the conclusion can be considered as absolutely proved.

Whilst engaged with the foregoing researches, it occurred to me that it might not be devoid of interest to the physiologist to know the amount of sulphur which was secreted by the kidneys in an unoxidized state. That urine does contain sulphur, not in combination as sulphate, is evident from the smell of sulphuretted hydrogen which, mixed with that of ammonia, is evolved from it whilst undergoing spontaneous decomposition, also from the blackening which white lead paint suffers when exposed to the gases arising from putrid urine, and likewise from the fact, that urine allowed to putrefy in a glass vessel containing oxide of lead as one of its constituents, permanently blackens the glass. To set the fact beyond doubt, two portions of urine, previously deprived of mucus by acetic

\* *Annalen der Chem. und Pharm.*, lvii. 180.

acid and filtration, each consisting of four fluid ounces, were measured; the one simply evaporated and burnt, the other evaporated and afterwards deflagrated with nitre. Each portion was then dissolved in dilute nitric acid, leaving a minute insoluble residue, and the sulphuric acid precipitated by chloride of barium; the results were as follows:—

Four fluid oz., simply incinerated, gave      Four fluid oz., deflagrated with  
 $\text{BaO SO}_3 \overset{\text{grs.}}{2.656} = \overset{\text{gr.}}{\text{SO}_3} 0.902$       nitre, gave in grains  
 $\quad \quad \quad = \text{S} \quad 0.366.$        $\text{BaO SO}_3 \overset{\text{gr.}}{5.697} = \overset{\text{gr.}}{\text{SO}_3} 1.954$   
 $\quad \quad \quad \quad \quad \quad \quad \quad = \text{S} \quad 0.783$

The quantity of sulphur in four fluid ounces not excreted in the state of sulphate was therefore 0.417 grain. To ascertain the quantity of sulphur excreted by the urine in twenty-four hours in an unoxidized state, the whole quantity passed by three individuals during that time was collected on different days and measured; its specific gravity was taken, and after being filtered from mucus, two portions of each specimen were measured in a specific-gravity bottle containing 1000 grain measures; one portion was simply acidified with nitric acid, and the sulphuric acid precipitated by nitrate of barytes. The other portion was evaporated with nitre and deflagrated, and the fused mass treated with dilute nitric acid and nitrate of barytes. From the difference in the quantities of sulphate of barytes obtained, the amount of sulphur not in an oxidized state was ascertained. The results from five different experiments are shown by the following table.

Table, showing the relative proportions of sulphur free and combined with oxygen excreted by the kidneys in twenty-four hours.

Quantity of urine in 24 hours.	58 fluid-ounces.	41.3 fluid-ounces.	62.5 fluid-ounces.	56 fluid-ounces.	43.5 fluid-ounces.
Specific gravity.	1.014.	1.019.	1.017.	1.022.	1.016.
	In 100 grs.	In 100 grs.	In 100 grs.	In 100 grs.	In 100 grs.
Sulphate of barytes from the acid existing in the urine.	0.312	0.427	0.414	0.715	0.386
Sulphate of barytes precipitated after oxidation of the free sulphur .....	0.441	0.563	0.546	0.826	0.507
Quantity of unoxidized sulphur .....	0.017	0.018	0.018	0.0153	0.0165
Free sulphur excreted in 24 hours .....	4.639	3.715	4.998	3.866	3.247

It thus appears that from three to five grains of sulphur pass off daily by the urine in some other combination than as



sulphuric acid, and that these three to five grains amount to about one-fourth of the whole quantity of sulphur excreted by the kidneys. With a view to ascertain what compound contained this sulphur in the urine, I have examined the precipitates produced in urine by neutral and basic acetate of lead, after the separation of the sulphates by baryta, and find that they only contain traces of sulphur. The colouring matter described by Scherer, and obtained by the method he adopts, likewise contains only a trace; whereas the liquid containing the urea, generally considered free from all other organic matter, which remains on the separation of the precipitate by basic acetate of lead, contains nearly the whole of this excess of sulphur, but how combined I have not yet been able to ascertain. The determination of this question, which I propose to examine, may possibly lead to some interesting facts, and perhaps throw some light upon the question respecting the formation of cystic oxide.

From some preliminary experiments made for the purpose of ascertaining whether phosphorus was contained in urine in any other compound than as phosphate, the following results were obtained.

To the four ounces of urine employed for the preliminary determination of the sulphuric acid and sulphur in the former experiment, after the precipitation of the sulphuric acid by barytes, some more nitrate of barytes was added and then the whole carefully neutralized with ammonia. The following quantities were obtained:—

Phosphate of barytes obtained from the $\text{PO}_5$ naturally contained in urine was	Phosphate of barytes obtained from urine after deflagration with nitre was
$\text{BaO PO}_5$ 5.775 grains	$\text{BaO PO}_5$ 6.532 grains
$= \text{PO}_5$ 1.834	$= \text{PO}_5$ 2.074
$= \text{P}$ 0.805;	$= \text{P}$ 0.910;

showing an excess of phosphorus over that contained as phosphate in the four ounces of urine to be 0.105 grain.

In another experiment, in which the same portions of urine were employed as at the fifth column of the foregoing table, the following were the results:—

Phosphate of barytes obtained from the $\text{PO}_5$ naturally contained in 1016 grs. of urine.	Phosphate of barytes obtained from 1016 grs. of urine after deflagrating with nitre.
$\text{BaO PO}_5$ 3.135 grains	$\text{BaO PO}_5$ 5.313 grains
$= \text{PO}_5$ 0.993	$= \text{PO}_5$ 1.687
$= \text{P}$ 0.435.	$= \text{P}$ 0.740

Therefore 0.305 grain of phosphorus was contained in the 1016 grains of urine in an unoxidized state, which, calculated for the whole amount of urine passed in twenty-four hours,

would amount to 5·896 grains. In some other specimens of urine which I examined there was however no approach to this quantity of phosphorus in an unoxidized state, and as the mode of analysis employed in these preliminary experiments was not the most accurate, I intend to make the determination of the quantity of phosphorus the subject of further experiments.

The only opportunity I have had of examining urine in a diseased state, was a portion obtained from a patient suffering from diabetes mellitus. As might have been anticipated from the character of this disease, the amount of sulphur which the urine contained in an unoxidized state, was considerably greater (by one-fourth) than in healthy urine.

The diabetic urine had a specific gravity of 1046.

Of this urine 1046 grains, precipitated with nitrate of barytes, gave—

4·308 grains sulphate of barytes = 1·479 grain  $\text{SO}_3$  = 0·592 grain S.

After the precipitation of the sulphuric acid by baryta, the urine was evaporated down with nitric acid, mixed with nitre and deflagrated. An excess of nitrate of barytes having been used to precipitate the sulphuric acid, there remained on treating the fused mass with dilute nitric acid, an insoluble residue of sulphate of barytes, the sulphuric acid of which had been derived from the oxidation of the sulphur by the nitre; the sulphate of barytes amounted to

1·837 grain = 0·629 grain sulphuric acid = 0·251 grain sulphur, or 0·024 per cent.,

whilst in healthy urine the sulphur in this state never exceeded 0·018 per cent.

XLII. *Note on a System of Imaginaries*. By A. CAYLEY\*.

THE octuple system of imaginary quantities,  $i_1, i_2, i_3, i_4, i_5, i_6, i_7$ , which I mentioned in a former paper, (and the conditions for the combination of which are contained in the symbols

$$123, 246, 374, 145, 275, 365, 167,$$

*i. e.* in the formulæ

$$i_2 i_3 = i_1, i_3 i_1 = i_2, i_1 i_2 = i_3,$$

$$i_3 i_2 = -i_1, i_1 i_3 = -i_2, i_2 i_1 = -i_3,$$

with corresponding formulæ for the other triplets  $i_2, i_4, i_6$  &c.,) possesses the following property; namely, if  $i_\alpha, i_\beta, i_\gamma$  be any

\* Communicated by the Author.

three of the seven quantities which do *not* form a triplet, then

$$(i_{\alpha} i_{\beta}) \cdot i_{\gamma} = -i_{\alpha} \cdot (i_{\beta} i_{\gamma}).$$

Thus, for instance,

$$(i_3 i_4) \cdot i_5 = -i_7 \cdot i_5 = -i_2;$$

but

$$i_3 (i_4 i_5) = i_3 \cdot i_1 = i_2,$$

and similarly for any other such combination. When  $i_{\alpha}, i_{\beta}, i_{\gamma}$  form a triplet, the two products are equal, and reduce themselves each to  $-1$ , or each to  $+1$ , according to the order of the three quantities forming the triplet. Hence in the octuple system in question neither the commutative nor the distributive law holds, which is a still wider departure from the laws of ordinary algebra than that which is presented by Sir Wm. Hamilton's quaternions.

I may mention, that a system of coefficients, which I have obtained for the rectangular transformation of coordinates in  $n$  dimensions (Crelle, t. xxxii. *Sur quelques propriétés des Déterminans gauches*), does not appear to be at all connected with any system of imaginary quantities, though coinciding in the case of  $n=3$  with those mentioned in my paper "On Certain Results relating to Quaternions," Phil. Mag. Feb. 1845.

58 Chancery Lane, March 2, 1847.

### XLIII. On Gun-Cotton.

By E. F. TESCHEMACHER, Esq.\*

IN consequence of the discovery by Professor Schönbein of gun-cotton, and of the possibility of its substitution in many cases for gunpowder, I have been induced to enter into an examination of the mode of its formation and of its synthetical composition, principally with a view of ascertaining how far it would be likely to affect an important branch of trade—the production and value of saltpetre and nitrate of soda. The examination was not commenced with the view of publishing it, but some of the facts brought to light appeared to possess sufficient interest to lay before the Society. I must premise that the experiments relate only to the gun-cotton prepared by the process recommended by Mr. T. Taylor. Equal measures of nitric acid of sp. gr. 1.509, and of sulphuric acid sp. gr. 1.840, were mixed together, and constituted (No. 1) the acids used in these experiments. I dried 50 grs. of South American cotton (from La Guayra) over a water-bath, and found the cotton gave off 3.40 grs. of hygrometric water. The 46.60 grs. of cotton which remained after drying, I steeped in 487½ grs. of the mixed acids No. 1; the

\* Communicated by the Chemical Society; having been read Nov. 2, 1846.

strong acids were in contact with the cotton about two minutes, and after squeezing well, the cotton was washed in water to free it from the acid: the quantity of water used was 5000 grs.

The strong acid squeezed out of the cotton weighed  $113\frac{1}{2}$  grs. (No. 2), leaving 374 grs. of acid (No. 3) to be accounted for, to make up the original quantity used. The cotton thus prepared was perfectly dried in the air over a water-bath, and was found to weigh 79 grs., being an increase of 32.40 grs. on the cotton used, equal to an increase of  $69\frac{1}{2}$  grs. upon every 100 grs. of cotton in its original state.

To ascertain whether the cotton would absorb anything more, I steeped 25 grs. of the above cotton, after it had been washed and dried, in fresh acid (No. 1), but no increase of weight took place. I also steeped a fresh portion of cotton for three minutes in the same quantity of fresh acid (No. 1); it weighed after washing and drying 78 grs.; a third portion was steeped for four minutes in one-third more acid than in the previous experiments, it weighed after washing 79 grs.; so that it appeared that the cotton had taken up its maximum from the acid by the first immersion.

As these 32.40 grs. increase of weight were clearly derived from the  $487\frac{1}{2}$  grs. of the mixed acids, I made the following experiments to ascertain what alteration the acids had undergone.

I neutralized 100 grs. by weight of the original acid (No. 1) by carbonate of soda, and found that it required 52.80 grs. of soda for that purpose. I found likewise that to neutralize the  $113\frac{1}{2}$  grs. of strong acid (No. 2) separated from the cotton, required 58.52 grs. of soda, and that the remaining 374 grs. of acid to be accounted for, contained in the washings, required 170.30 grs. of soda for neutralization, making together 228.82 grs. of soda required to neutralize the  $487\frac{1}{2}$  grs. of acids No. 2 and 3, used for acting upon the cotton; therefore, if 100 grs. of the original mixed acids (No. 1) required 52.80 grs. of soda as above for neutralization,  $487\frac{1}{2}$  grs. would require (if not acted upon by the cotton) 257.36 grs. of soda; it however only required 228.82 grs. as above. The difference, 28.54 grs. of soda, is therefore equal to the acid which disappeared, or was taken up by the immersion of 46.60 grs. of cotton in the mixed acids. To ascertain whether a portion of each of the mixed acids, or only one of them, was taken up by the cotton, I added to the solution of the 100 grs. of mixed acids (No. 1) (neutralized by the soda) chloride of barium, and obtained a precipitate weighing 126 grs. of sulphate of barytes.

I also, in the same manner, precipitated from the solution



No. 2, 165·40 grs. of sulphate of barytes, and from No. 3, of 374 grs., 449·30 grs. of sulphate of barytes, making together 614·70 grs.; therefore, if 100 grs. of the original acids (No. 1) give 126 grs. of sulphate of barytes,  $487\frac{1}{2}$  grs. would give 614·25 grs.; they gave, as above, 614·70 grs., showing that no portion of the sulphuric acid was withdrawn, but that it was a portion of the ingredients of the nitric acid alone that combined with the cotton.

The quantity of soda neutralized by the acids (Nos. 2 and 3) was, as just stated, 28·54 grs.; therefore, if 32 grs. of soda neutralize 54 grs. of nitric acid, 28·54 grs. of soda will give 48 grs. of nitric acid, the quantity of acid which disappeared, or was taken up by the cotton.

From what took place, the following may be deduced as its synthetical composition:—48 grs. of dry nitric acid, containing 35·66 grs. of oxygen, were decomposed by the 46·60 grs. of cotton, and 32·40 grs. were added to the weight of the cotton. It is probable that the mixed acids combined with a portion of the constituent water of the cotton, which was replaced by oxygen and nitrogen, in the proportions of nitric acid, as there was no appearance of the evolution of either nitrogen or oxygen in the formation of the gun-cotton.

Its synthetical composition would stand thus:—

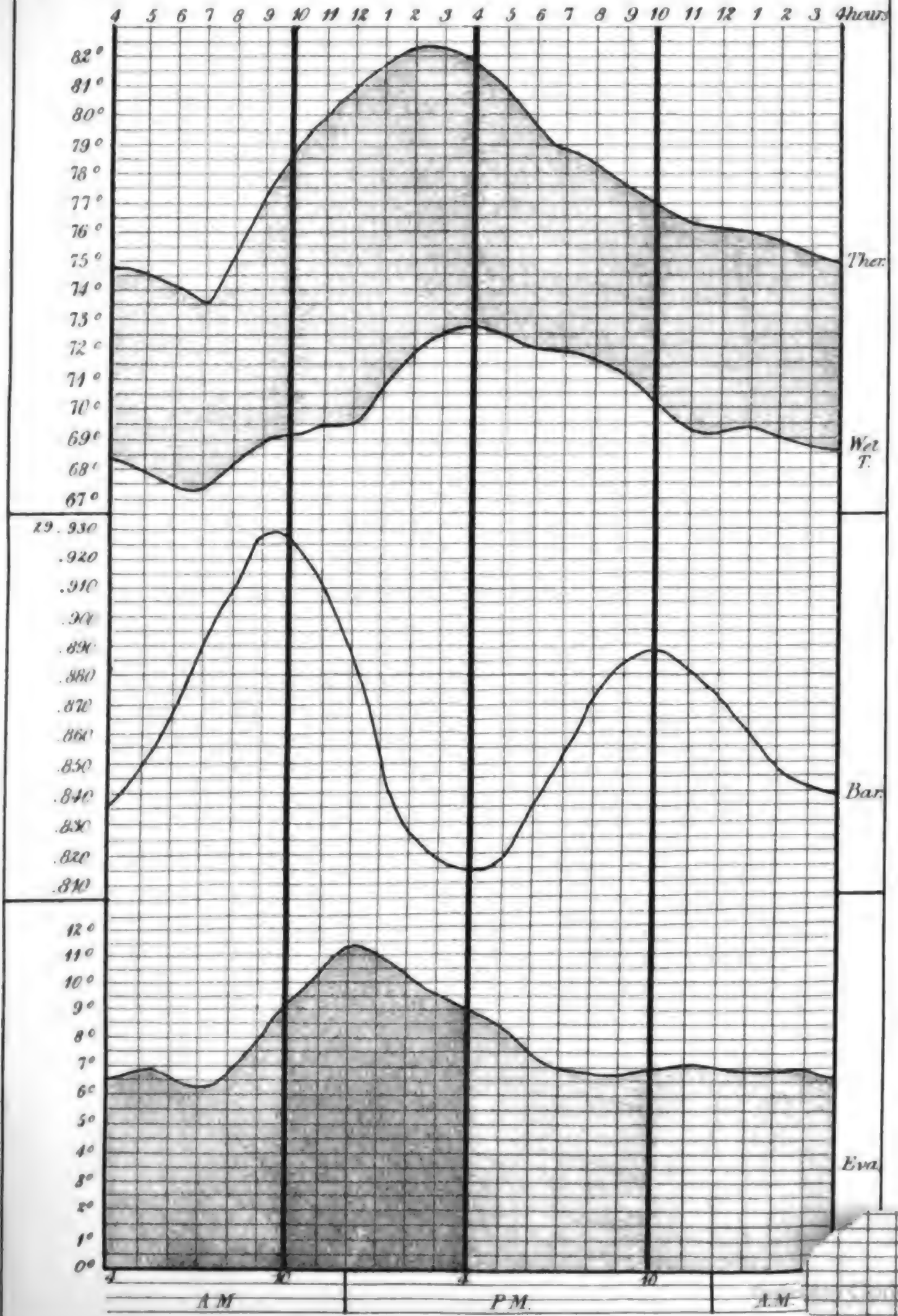
46·60	grs. of cotton deprived of
15·60	water, give
31·00	cotton without constitutional water, combined with
48·00	oxygen and nitrogen derived from the mixed acids,
79·00	the weight of gun-cotton produced, equal to
39·25	parts cotton,
60·75	... oxygen and nitrogen (nitric acid)
100·00	parts.

From these experiments we arrive at the following conclusions regarding the formation of this substance:—That as it requires 48 grs. of nitric acid, or its equivalent 90·66 grs. of saltpetre, to form 79 grs. of gun-cotton, it will require 60·75 grs. of nitric acid, or its equivalent 114·76 grs. of saltpetre, to form 100 grs. of gun-cotton; and according to the same calculation, it will require 97·76 grs. of nitrate of soda for the same purpose.

Taking into consideration the original cost of the cotton, the expensive manipulation of the conversion of the nitre into nitric acid, and the additional weight of nitre required to produce the same weight of gun-cotton, it is evident that the latter substance must be more expensive than gunpowder, taking weight for weight of each. How far, however, gun-

# Diagram

Of the hourly heights of the Barometer, the dry Thermometer, the wet bulb Thermometer, and the relative amounts of the force of evaporation at Bombay.



cotton may exceed gunpowder in its explosive force still remains to be ascertained, and this of course forms an important element in the calculation. Other processes may be already known, or may be hereafter discovered, calculated to reduce the expenses of the formation of the so-called gun-cotton, but it must be still borne in mind that an enormous quantity of oxygen, amounting to 45 parts in every 100 parts of gun-cotton produced, must be obtained from some extraneous source for combination.

Many other vegetable fibres may be substituted for cotton; but as far as a few experiments which I have made, it appears they do not possess the explosive force of cotton. In a trial upon flax, I found that 50 grs. increased in weight to 72 grs.; the explosive force was feeble; this was the case with sawdust similarly prepared; but it is possible that this latter form of impure lignin may eventually be of more importance than it appears at present.

Fifty grains of *deal* sawdust dried at  $212^{\circ}$  were first washed with dilute muriatic acid and then with dilute caustic alkali; they were found to have lost 6 grs. after washing and again drying. The 44 grs. remaining were steeped in the mixed acids for about five minutes, then washed and dried; they increased to 58.7 grs. The acids were examined by carbonate of soda in the manner before detailed, the sawdust was found to have neutralized 22.90 grs. of soda, equal to 38.65 parts of nitric acid, or 66.66 of nitric acid for every 100 parts of gun-sawdust. This gun-sawdust flashed off readily, but with less rapidity than gun-cotton, leaving a small carbonaceous residue.

The difference in quantity of nitric acid taken up by the sawdust and the cotton is no doubt owing to the former being a much more impure form of lignin than the latter.

#### XLIV. *Third Memoir on Induction.*

By Prof. ELIE WARTMANN\*.

[With a Plate.]

§ VIII. *Does electro-magnetic induction affect luminous radiations otherwise than by causing their plane of polarization to rotate?*

80. **M**R. Faraday has recently stated that a rotation is impressed on the plane of polarization of luminous

\* Communicated by the Author, having been read before the Vaudois Society of Natural Sciences on the 20th of May 1846 (see *Bulletins*, tom. ii. pp. 58, 61, 70, 75 and 98). It is a sequel to those which have been published in the *Phil. Mag.* vol. xxv. p. 266, and vol. xxvii. p. 547, and in the *Archives de l'Electricité*, tome iv. p. 34, and tome v. p. 440.

rays transmitted by various media, when these are the seat of a sufficient electro-magnetic induction\*. I have shown that polarized calorific radiations are affected in a manner entirely similar†. Are these effects limited to the cases in which they have been detected? Would not the action of the magnet upon fluids, or rather on diaphanous and diathermanous bodies, be capable of impressing on light and caloric other modifications hitherto unperceived? The mere statement of these questions is enough to indicate an unlimited field of research. I shall content myself with describing the experiments which I have made upon the rays of the spectrum, which may be studied by processes capable of accurate admeasurement, and whose theory is intimately connected with that of light.

81. I long ago observed that the production of the rays, their direction, number and distribution, are not affected by the presence of a magnet in contact with the prism. I repeated these experiments, inducing magnetism in various media through which the luminous bundle had to pass.

82. My prism, of the finest flint glass, was made by Fraunhofer; its angle of refraction is  $45^{\circ} 4' 20''$ .

83. The diaphanous bodies submitted to induction were the following:—Among the gases, air and nitrous acid, dry and moist: these were inclosed in a glass tube  $0^m\cdot180$  long and  $0^m\cdot008$  in diameter. Lastly, among solids, a specimen of very pure flint glass was selected, in the form of a square prism  $0^m\cdot170$  long and  $0^m\cdot0195$  the side.

84. The rays were observed in a dark chamber with an opening in the shutter of six metres, with an excellent comet-seeker by Cauchoir. This instrument has a reduced opening of  $0^m\cdot069$  and  $0^m\cdot66$  focal distance: it was used with a magnifying power of seven times.

85. The light sent horizontally by a heliostate with a silvered or black mirror, or from a lamp, was polarized more or less completely by reflexion or by its passage through a Nicol's prism. A second similar prism served as analyser.

86. The electro-magnet is formed of a horseshoe of very soft iron, weighing  $12^{kil}\cdot5$ , around which is wound a well-annealed copper wire of  $0^m\cdot003$  diameter and  $70^m\cdot8$  long.

\* Philosophical Transactions, 1845, and Phil. Mag. vol. xxviii. p. 294, &c.

† *L'Institut* of May 6, 1846, No. 644. M. Ruhmkorff has recently repeated my experiments. He polarizes and analyses heat with Nicol's prisms of large size. The induction is obtained by means of one of those double helices, destined to reproduce the phenomena discovered by Mr. Faraday, and on which M. Biot has read a very favourable report to the Institute. According as the magnetism is engendered in this powerful apparatus, or the voltaic circuit is broken, the deviation of the rheometric needle varies several degrees. (October 1.)



This apparatus easily raises 912 kil. : it was set in action with a Bunsen's battery of from twenty to forty pairs. Moreover, its energy was augmented, according to circumstances, by laying on the poles the great helix (3), the three wires of which were connected by their ends, or a tube of soft iron containing the transparent media to be induced ; sometimes even the envelope of iron with the diaphanous body were placed in the hollow of the helix.

87. Whatever may have been the degree of polarization of the luminous bundles and the intensity of the magnetic forces developed, *the rays of the spectrum* produced by the rays of the sun or from an artificial source presented no appreciable change. Their number and their distance, varying with the nature of the light and with that of the prisms, were not modified by the new molecular arrangement produced by induction.

§ IX. *Has statical or dynamical induction any influence on chemical affinities ?*

88. Chemical affinities are intimately connected with the electric forces. The problem of their reciprocal dependence has not yet been solved, because this problem has not been regarded in all its bearings. Admitting, with most physicists, that the magnetic influence is felt in imponderable fluids in a mediate manner, by a disturbance or by a new and forced equilibrium in the constitution of the medium traversed by these fluids, it becomes highly interesting to examine whether this disturbance interferes with the forces of affinity, whether it can increase or diminish them. The following experiments were directed to this object.

89. Between the arms of the electro-magnet (86) was arranged a voltameter with lamina of platina, in which some acidulated water was electrolysed. The voltaic current could, at will and by a very simple arrangement, thus bring the magnet into action and develop a north or south pole at either of its extremities. The product of decomposition in a given time was estimated by the hydrogen always collected on the same electrode. Now, whatever the direction and intensity of the magnetism engendered, as well as the position of the voltameter within or without the polar arms, the volume of the gas remained the same\*.

90. The same was the case also when the electro-magnet was replaced by the large helix (3), in the hollow of which the voltameter was placed.

\* I have been informed by Prof. Grove that similar experiments, still unpublished, had led him to the same result. (October 1, 1846.)

91. To meet the objection that the influence of the magnet is not sufficiently felt on the lamina of platina, I constructed two new voltmeters (Plate V. figs. 1 and 2) in which the electrodes are prisms of soft iron, gilded by the galvanic process, and which I magnetize by placing them upon the polar surfaces of the large magnet (86), covered with a very thin isolating leaf of mica. In one of these apparatus, the prisms are 0<sup>m</sup>·030 in breadth. Repeated experiments, made with batteries of variable force, sometimes collecting the two gases, sometimes the hydrogen alone, and decomposing acidulated or alkaline water, have invariably led to the same results.

92. But, it will be said, the electric current which imparts to the magnet its power, is an uninterrupted current and incapable of exerting any action upon the electrolyte which is not in its circuit. In order to remove this difficulty, I substituted electricity of tension for that of the current. A fourth voltmeter (fig. 3) was constructed so as to ensure the most perfect isolation between its poles. Behind these, against the sides of the glass vessel, I fixed two platina laminæ, one-half of which was immersed vertically 0<sup>m</sup>·03 in the liquid to be decomposed, whilst the other was continued outside in a horizontal band terminated by a ball. A thick coating of wax on the exterior of the voltmeter prevented all electric communication between these laminæ, except through the liquid. The hydrogen produced in the unit of time by the electrolysis of the acidulated water was measured carefully. The quantity of gas did not vary when one of the laminæ of platina was placed in relation with the conductor of a powerful electrical machine (with a plate three metres in circumference), the other lamina communicating with the ground; or when these laminæ were employed to connect the armatures of a battery of three large Leyden jars, kept constantly charged. The direction of the analysing current was changed without the effect being modified.

93. We may conclude from the preceding experiments, that *statical or dynamical induction has no influence upon the chemical actions engendered by electricity.*

#### § X. *Are magnets capable of producing chemical action?*

94. The subject which I have alluded to in the preceding paragraph is so intimately connected with the controverted question of the chemical actions produced by magnets and by terrestrial magnetism, that I deemed it necessary to devote to it some special experiments. Most observers who have studied it have used only magnets of small power. I have employed electro-magnets of great energy, which were at my disposal, to

repeat their researches, being persuaded that a very intense force must produce corresponding decisive and considerable effects. Other circumstances have thrown doubt on the conclusions mentioned by those authors; account has not been taken of the relations between the surfaces which were the seat of the action to be measured, nor of the differences of temperature, of density, and of composition of the liquids employed. I have endeavoured to guard against these causes of error, and to obtain conclusive results by means of numerous trials of sufficient duration.

95. I procured cylinders of soft iron, taken from the same bar, and about 0<sup>m</sup>·027 in height and 0<sup>m</sup>·02 in breadth. Four tumblers were so arranged that the plane of their axes coincided with that of the magnetic meridian and four others arranged in a perpendicular direction. After having poured into each of them a solution of sulphate of copper in layers of equal depth, an iron cylinder was placed in the centre; and the pole of an iron horseshoe magnet was then set on the upper unsubmerged face. The four magnets, the strongest of which carried about forty kilogrms, were distributed, one N.S., the second S.N., the third E.W., and the fourth W.E., with relation to the north pole of the terrestrial magnetism; they were separated, so as not to exert any reciprocal influence. At the end of fifteen hours the deposits of copper on each cylinder presented everywhere the same appearance and the same consistence. The balance proved that they were all nearly of an equal weight; the slight differences found, which scarcely attained to one- or two-thousandths of the total quantity of copper reduced, are explained by the inequality of development and cleanness of the surfaces of the eight cylinders. The experiment was repeated a great number of times with solutions of copper more or less pure, and more or less concentrated, without the general result varying.

96. I will further cite the following experiment, which led to the same conclusions. Twelve iron cylinders were distributed in pairs, in six distinct vessels containing a solution of iron-alum as neutral as possible. The pairs of cylinders of the second, third, fourth and fifth vessels, were placed in contact with the poles of four electro-magnets, the first of which has been already described (86); the second is capable of carrying sixty kilogrms; the third at least forty, and the last more than 280. They were in other respects arranged as in the cases before described (95), and their poles were covered with a plate of mica. Lastly, the current of a battery of forty Bunsen's pairs was passed, so as to decompose the liquid by passing across the interval between each couple of cylinders, at the

same time setting the electro-magnet in action. The current went from north to south between the non-magnetized cylinders of the first vessel; then from north to south between those of the second vessel and the axis of the electro-magnet; then from west to east and from east to west, through the liquid and the axes of the magnets of the third and fourth vessels; then from south to north between the cylinders and in the axis of the magnet of the fifth; lastly, from south to north in the last vessel between its cylinders removed from all magnetic atmosphere. Weighings made with very delicate balances showed that the deposits of oxide of iron on the negative cylinders were not at all affected by the magnetism. The mean weight of these deposits, estimated, after having heated them to above  $212^{\circ}$  F., was 1.3 gramme.

97. Those authors who have treated of the question of the influence of magnets, have often confounded the part which they may play in chemical actions with that which they exercise in phenomena of molecular arrangements and of crystallization. I think I have proved that their part is null in the first case, but I do not deny its existence in the second. Every new arrangement in the particles of a body must be influenced by the magnet if it is accompanied by a disengagement of dynamic electricity, a more frequent circumstance perhaps than is generally supposed\*.

98. My experiments, which are new in their arrangement and the power of the magnets employed, invalidate therefore the opinion of Von Arnim †, J. W. Ritter ‡, Ludicke §, Masch-

\* A reservation must however be made for the modifications of molecular equilibrium which the changes of condition occasion. Thus the fusion of solids and the solidification of liquids do not liberate more electricity than the evaporation of pure water or the liquefaction of vapours chemically neutral. Any one may convince himself of this by placing in a horizontal glass tube a leaden cylinder, the extremities of which are connected with those of a good rheometer, and by effecting the fusion of the metal or its solidification by a requisite elevation or lowering of temperature. Another experiment consists in filling a platina crucible with melted wax, in which is suspended a great quantity of plumbago. In the midst of this conducting mass is immersed a platinum rod, isolated in its centre by a covering of glass, and the lower extremity of which is naked only for an extremely small length. The rheometric wire communicating on the one part with the crucible, and on the other with the projecting head of the rod, the wax may be melted or be allowed to congeal without any current resulting.

† *Ideen zu einer Theorie des Magnetismus*. Gilb. *Ann.*, tome iii. p. 59; tome v. p. 394, and tome viii. p. 279.

‡ *Beiträge zu näherer Kenntniss des Galvanismus*, tome ii. p. 55.

§ Gilb. *Ann.*, tome ix. p. 375. This observer for the rest has since maintained an opposite opinion. See Gilb. *Ann.*, tome xi. p. 117.



mann\*, Hansteen†, Schweigger‡, Döbereiner§, Müller||, Kastner¶, Fresnel\*\*, Murray††, Archbishop Rendu‡‡, the Abbé Zantedeschi§§, Ampère|||, and of Mr. Hunt¶¶, who have all asserted that magnets possess a chemical power. They agree, on the contrary, with the results published by Steinhauser\*\*\*, Erman†††, Dulk‡‡‡, Wetzlar§§§, Otto-Linné Erdmann||||, Berzelius¶¶¶, the Marquis Ridolfi\*\*\*\*, and the Chevalier Nobili††††.

\* *Ann. de Ch. et de Phys.*, tome xxxviii. p. 201.

† *Ibid.* p. 206.

‡ *Jahrbücher*, tome xiv. p. 84.

§ *Ibid.*

¶ Kastner's *Archiv*, tome vi. p. 448.

¶¶ *Ibid.*

¶¶¶ *Ann. de Ch. et de Phys.*, tome xv. p. 219.

†† *Phil. Mag.* Nov. 1821. His conclusions are opposed to those of Maschmann and Hansteen.

‡‡ *Ann. Ch. et Phys.*, tome xxxviii. p. 196.

§§ *Bibliotheca Italiana*, April 1829. The author appears to have abandoned his first views; he does not cite his own experiments, and does not even mention the chemical power of magnets in his *Trattato del Magnetismo e della Eletticità*. Venice, 1845.

||| Becquerel, *Traité de l'Electricité*, tome i. p. 384. It is to be remarked that several special treatises pass wholly in silence this supposed property of magnetic bodies. Such are the Treatises on Electricity, Magnetism, &c. by Dr. Roget, in the Library of Useful Knowledge, the Manual of Electricity and Magnetism in Lardner's Cabinet Cyclopædia, &c.

¶¶ *Phil. Mag.* Jan. 1846. For more details see Researches on the Influence of Magnetism and Voltaic Electricity on Crystallization and other conditions of Matter, in the Memoirs of the Geological Survey of Great Britain, vol. i. London, 1846. Mr. Hunt himself instances a contradiction between the various results he has obtained with iron wires supported by the poles of a magnet and submitted to a chemical action. He finds that in a solution of crystals of sulphate of copper, the copper is reduced in greater abundance around the north pole, whilst in the acidulated water the volume of hydrogen liberated is always more considerable at the south pole. Von Arnim, at the end of the last century, asserted that the north pole of a magnet is oxidized more in water than the south pole. Ritter subsequently maintained the opposite opinion; but notwithstanding his promise, he never published the sequel of his researches, from which we may infer that he discovered his error, and the equal indifference of the two extremities of a magnet.

\*\*\* Steinhauser of Halle employed a very powerful magnetic apparatus. *Gilb. Ann.*, tome xiv. p. 125.

††† *Gilb. Ann.*, tome xxvi. p. 139.

‡‡‡ Dulk experimented at Königsberg with a magnet which supported twenty-five pounds. *Kastn. Archiv*, tome vi. p. 457.

§§§ Schweigg. *Jahrb.*, tome lvi. p. 118.

|||| Erdmann of Leipsig has submitted to a scrupulous examination all the researches made by his predecessors. His work may be cited as a model of patience and accuracy. Schweigg. *Jahrb.*, tome lvi. p. 24.

¶¶¶ *Jahresbericht*, No. x. p. 43.

\*\*\*\* *Antologia di Firenze*, No. xix, 1822.

†††† *Memorie ed Istrumenti*, tome i. p. 301.

§ XI. *On a new instance of electro-magnetic rotation.*

99. I have had occasion to observe, in the preceding experiments, a phænomenon of rotation which merits to be noticed, not only as a new confirmation of the theory of Ampère, but also on account of the relations which it may have with the facts which will be stated in the following paragraph.

100. If, after having caused two soft iron cylinders to adhere to the poles of a horseshoe magnet, these cylinders are immersed in a solution of sulphate of copper contained in a vessel of any form and protected from vibration, we perceive, at the end of a few minutes, when the deposit of reduced copper is already quite visible, a double current established in the mass of the liquid. This current often affects much more the lower than the upper molecules; sometimes, according to the density of the fluid and the state of its surface, it is only established in its interior. In order to be able to examine it well, the liquid mass must be illuminated, either directly or by means of mirrors or refractors. A convenient arrangement consists in placing this mass in a glass vessel with smooth sides, and illuminating it in the dark chamber by a bundle of solar light. The tint of this bundle may be varied from red to violet, by interposing in its course coloured media, or by refracting it by a prism, without the phænomenon being modified. The movement is rendered perceptible from the displacement of the particles suspended in the liquid; if the latter is perfectly limpid, it is not observed. It is easily produced by employing a concentrated solution of sulphate of copper, from which some small particles of oxide have been precipitated by a few drops of potassa; these particles remain in suspension. This current ceases at the end of a longer or shorter time, with the chemical action which gives rise to it, when the quantities of copper deposited and of iron dissolved have reached a certain relation. It does not occur in solutions of subacetate of lead, of iron-alum, of triple sulphate of copper, zinc and iron, whatever pains are taken to establish in their interior precipitates of oxide, or to add to them light powders. Sometimes it lasts for more than two hours. Pure water, rendered slightly opaline by particles of oxide of copper, presents no movement.

101. The current is never produced except under the influence of the magnet. Soft iron cylinders immersed in sulphate of copper are of themselves incapable of producing it; they only acquire this power when magnetism is induced in them. If an electro-magnet be employed, the rotation varies with the direction of the electric current. It takes place circularly or elliptically around each magnetic pole, and is directed like the

current of Ampère. It goes therefore from north to south by the west around the north pole, or that by which the current of the battery enters, and from north to south by the east around the south pole, or that which communicates with the zinc of the Bunsen's apparatus (fig. 9). In general it does not appear to be more rapid near the cylinders than at a certain distance. It is especially apparent between them, but it also exists at a tolerable distance from the centres. I have more than once seen the velocity vary in an intermittent manner, as if it had to overcome passing obstacles.

102. The theory of these rotations is easily suggested. When we immerse a bar of iron in a solution of sulphate of copper, the electro-chemical action determines in the liquid an electric current, which proceeds from the peripheric parts to those immediately around the cylinder, in the direction of the prolonged radii of the latter. This may be convincingly shown with a good rheometer, the extremities of which, of thick well-polished platinum wire, are placed alternately, the one near the iron, the other toward the margin of the vessel which contains the solution. This current is analogous to one passed into a metallic ring full of mercury, which would tend, radiating towards the centre, to issue by a conductor placed perpendicularly to the surface. An external horizontal current, and near the vessel, would, according to its own direction, cause the mercury to rotate to the right or to the left. In our experiment the magnet is substituted for the horizontal current.

103. A chemical action is therefore necessary, and proceeding from currents of a certain intensity, for the double polary rotation to take place. The liquids cited as not producing it in a certain manner, evidently did not fulfill this condition: they did not possess a requisite relation of energy between the electric currents and the magnetization. I attribute the observed variations in the force of the electric current, and consequently of the velocity of rotation, to variations in the play of the affinities between the iron, all the molecules of which are not equally acted upon, and the liquid, the composition of which changes incessantly. The reduced copper, even upon polished cylinders, is always striated; it is not deposited therefore in a uniform manner and without intermittence. The temperature, by influencing either the chemical action or the viscosity of the liquid, is perhaps not wholly without influence upon the rapidity of the rotation. Moreover, it will be conceived that this latter becomes visible only when it is communicated to solid matter on which the light is reflected\*.

\* It is evident, that in employing very intense chemical actions, this

§ XII. *On the Lines of Chemical Affinity.*

104. The phænomenon which I now propose to describe is not the result of an inductive action, I however mention it here on account of the connection which it appears to me to have with the preceding facts.

105. The sulphates of copper of commerce vary much in purity. Amongst others, there is one which, when dissolved in common water, gives a greenish opaline liquid, which becomes blue and limpid by filtration. If a soft iron cylinder is placed in this solution, we perceive, as soon as its contour is reddened by the deposit of copper, very minute filaments, of a pale blue, produced radiating all around its immersed surface. These filaments increase rapidly in number and dimensions. They soon attain forty to sixty millimetres in length, and then present the appearance of stamens with corrugated filaments, terminated by heads or elongated anthers, distributed over a very regular circumference (fig. 10). The development of this curious figure depends on the concentration of the liquid, the capacity of the vessel which contains it, and perhaps on other causes, such as the differences of density between the upper and lower layers produced by the substitution of the sulphate of iron for the sulphate of copper, the increase in density of the surface layers owing to evaporation, &c. Be this as it may, the relative opacity of the heads contrasts with the limpidity of the liquid around the iron. When the decrease in the amount of copper has reached a certain limit, indicated by the grass-green tint which the solution takes, the deposit agglomerates gradually and subsides to the bottom of the vessel, which it had not before done, and the reaction is terminated.

106. The phænomenon becomes more instructive when manifested under the action of two centres (fig. 11). Then the rays which diverge arrive perpendicularly one against another, following the line of the shortest distance. Beyond that, they meet following directions more and more oblique. They never invade each other's domain: they are devoid of any power of penetrating into the interstices of each other. These domains are separated by a perfectly straight line, which intersects rectangulary the middle of the line of the smallest interval. The rays which, from one part and another, stop at this right line, there undergo an inflexion, the more

simple or double rotation is easily obtained. We may thus, as Mr. Grove has told me he had repeated it, cause little cork boats to circulate around a bar of iron suspended to a powerful magnet, and immersed in dilute sulphuric acid, &c. I am told that Mr. Christie had also noticed this rotation, but I believe his observation is unpublished. (October 1.)



sensible as it takes place at a less distance from the others, and resemble more or less decided hyperbolic arcs. The figure is of a truly geometric symmetry.

107. With three centres placed on the summits of an equilateral triangle (fig. 12), the dividing right lines proceed from an internal point situated at an equal distance from the summits, and are prolonged perpendicularly to the three sides of the triangle which are here the lines of the least interval. The divergent rays in two directions curve in a very marked manner. For the rest, the figure which they produce is perfectly symmetrical.

108. The radiations which appear to render visible the lines according to which the affinities are developed, are not sensibly altered by magnetism; this is at least the result of an experiment unfortunately unique. But availing ourselves of the attraction which causes the cylinders to adhere to the arms of a magnet to give them a moderate motion of translation, we see the whole figure, and especially the dividing line which constitutes the most marked part of it, transferred also without any alteration of form. A vibration, a sudden transfer, detaches, on the contrary, the solid particles which were geometrically grouped: they are precipitated to the bottom, and all is spoiled.

109. I suppose these particles to consist of a sub-sulphate of copper\*; but I have not been able to collect a sufficient quantity of it to subject it to analysis. I propose however to publish soon a new series of researches on this interesting subject.

### § XIII. *Is there any reaction between currents of induction and continuous electric currents†?*

110. The apparatus which has served for the examination of this question is an electric machine constructed by M. Bonijol. On its reel are coiled two copper wires; one which makes 200 turns is traversed by the inductor current; the other, which is finer, forming 3400 revolutions, is the seat of the currents induced by the rapid alternatives of opening and breaking of the circuit of its neighbour. This

\* Mr. H. Rose has told me that he is of the same opinion. (October 1.)

† This paragraph was communicated to the Société de Physique et d'Hist. Naturelle of Geneva, April 9, 1842. See also the *Bulletins des Séances* of the Société Vaudoise des Sciences Naturelles, tome i. p. 68. M. de la Rive has arrived at similar conclusions in his *Mémoire sur l'action combinée des courants d'induction et des courants hydro-electriques*.—*Archives des Sciences Phys. et Nat.*, tome i. p. 393 (1846).

double helix is pierced in the centre by an aperture of  $0^{\text{m}}\cdot 04$  in diameter, in which may be fixed a bundle of soft iron wires, or a hollow cylinder of iron plate furnished externally with similar wires.

111. It is especially in valuing the heating effects of which the induced currents are capable, that we seek to resolve the proposed problem. After having placed in the circuit of the long wire Breguet's thermometer (4. c.), and having determined the number of alternate induced currents, which in the unit of time give the maximum of heating, an hydro-electric current was passed into the wire and into the thermometer. The inductor current was that of five large pairs of Daniell (6).

112. The following results were obtained:—

a. The calorific effect of the currents of induction is more considerable when the bundle of wires is placed in the helix than when the hollow cylinder is employed: the latter being removed, the temperature again decreases.

b. The result does not vary whether the induced wire is or is not traversed by a constant current.

c. The elevation of temperature of the thermometer produced by a continuous and constant current, which traverses a wire formed into a helix, is independent of the presence or absence of bundles of soft iron wires, solid or hollow, placed within it.

113. I have had constructed a large pair, of bismuth and soft iron,  $0^{\text{m}}\cdot 2$  long, and the square section of which is  $0^{\text{m}}\cdot 022$  at the side. The free extremities of these metals were bound to a thermo-electric rheometer (46) by a wire long enough for the instrument to be protected from all magnetic induction.

114. *No appreciable current was produced* on placing the pair in the large helix (3), and developing in it an intense electro-magnetic state with a Bunsen's battery of twenty elements.

115. The soft iron of the couple was enveloped in an isolated copper wire. Then, after having heated its solder to a constant temperature and determined the force of the thermo-electric current produced, a hydro-electric current was passed into the wire. *The deviation of the rheometer did not vary, notwithstanding the electro-magnetic state induced in the iron.*

XLV. *On the Chemical Composition of Gun-Cotton.*

By Messrs. ROBERT PORRETT and E. F. TESCHEMACHER\*.

**I**N the paper from Mr. Teschemacher read before the Society on the 2nd of November, the composition of gun-cotton, as deduced from careful synthetical experiments, was as follows :—

39·25 of cotton deprived of its constitutional water.

60·75 of dry nitric acid.

100·00

We have now to show that this composition agrees very closely with the following formula, which we have obtained from the analytical experiments that will form an important part of the present communication.

The formula in question is as follows :— $C_{12}H_8O_8 + 4NO_5$  = nitrated lignin ; or in centesimal proportions,

Carbon	. . . . .	20·00
Oxygen	$\left\{ \begin{array}{l} 17·78 \\ 44·44 \end{array} \right\}$ . .	62·22
Hydrogen	. . . . .	2·22
Nitrogen	. . . . .	15·56—100·00

Or it may be thus described :

Lignin dried at 350° :	Carbon	. . .	20
	Water	. . .	20 = 40
Nitric acid (15·56 nitrogen, 44·44 oxygen)			60
			<hr/> 100

This, it will be seen, very nearly agrees with Mr. Teschemacher's synthetical result. We have therefore taken the composition of nitrated lignin in all our experiments as identical with that of gun-cotton.

Having by preliminary trials ascertained that with certain precautions those products might be evolved without danger and collected in close vessels, we proceed to state that our method consisted in introducing the charge of gun-cotton into a thin glass tube of about the diameter of a goose-quill, 7 inches in length, closed at one end, and bent at the other so as to be conveniently introduced under a glass receiver filled with and inverted over mercury, whilst the principal part of the tube was held in a horizontal position ; the gun-cotton was exploded by heat externally applied by means of a spirit-lamp with a very small wick *to the part of the charge in the fore part of the tube*, so that its combustion proceeded backwards until the whole was consumed. The portion of the glass tube which is introduced into the mercury should

\* Communicated by the Chemical Society ; having been read Nov. 16, 1846.

be bent at such an angle as to prevent any of the mercury entering the tube previous to combustion, and the combustion must be continuous, which is readily effected by carefully applying the heat backwards as the cotton is ignited. With these precautions, the evolution of the gas was not attended with any such violence as to interfere with its collection, nor was any portion of the charge thrown out unconsumed, which would have been the case had the heat been applied to the hinder portion first. Having collected a sufficient quantity of the gas, we passed up into it, first a little caustic potash, which absorbed the carbonic acid mixed with cyanogen gas, and then green sulphate of iron, which took up the nitric oxide; the remaining gas was mixed with half its bulk of oxygen gas, and exploded in a detonating tube, showing the quantity of carbonic oxide present; the remaining gas after the separation of the carbonic acid thus formed was considered as nitrogen.

Some of these constituents were unexpected by us, especially the cyanogen; its presence however was fully proved, as we succeeded in forming prussian blue with it. The quantity of mixed gases obtained from 52.33 grs. of gun-cotton measured 100 cubic inches; these mixed gases have the following composition:—

	Relative volumes.	Cubic inches.	grs.	
Carbonic acid	2	14.286	6.759 =	{ 1.815 carbon 4.944 oxygen
Cyanogen . .	1	7.143	3.965 =	{ 1.815 carbon 2.150 nitrogen
Nitric oxide .	5	35.715	11.478 =	{ 6.091 oxygen 5.387 nitrogen
Carbonic oxide	5	35.715	10.714 =	{ 4.536 carbon 6.178 oxygen
Nitrogen . .	1	7.143	2.154 =	2.154 nitrogen
		100.000	35.070	

100 grs. of the same would therefore give 64.550 grs. of the mixed gases, and as the ultimate elements, 15.030 grs. of carbon, 31.680 grs. of oxygen, and 17.840 grs. of nitrogen.

By passing the gases from the combustion-tube through a tube containing fused chloride of calcium, we have separated 20 grs. of water, to be added to the gaseous products; and we observed a crystalline deposit, both in the combustion-tube and in the one leading from it, together with 5 grs. of carbon lining the combustion-tube, and resulting from the decomposition of a part of the crystalline deposits after heat-



ing the tube to redness. The weight of these together were 13·125 grs., of which 5 were carbon and 8·125 we ascertained to be anhydrous oxalic acid.

If we now collect these products, we find—

Carbon	{	in the gases . . . . .	15·030	}	22·738
		in the anhydrous oxalic acid	2·708		
		in the residue . . . . .	5·000		
Oxygen	{	in the gases . . . . .	31·680	}	54·876
		in the water . . . . .	17·780		
		in the anhydrous oxalic acid	5·416		
Hydrogen in the water . . . . .			2·220		
Nitrogen in the gases . . . . .			17·840		
			97·674		
Leaving a deficiency, from unavoidable errors in the experiment, of . . . . .					2·326
			100·000		

If these ultimate elements be compared with the composition we have assigned to nitrated lignin or gun-cotton, we shall observe the following near approximation :—

	From the analysis.	From the composition of nitrated lignin.
Carbon . . .	22·738	20·000
Oxygen . . .	54·876	62·220
Hydrogen . .	2·220	2·220
Nitrogen . .	17·840	15·560
	97·674	100·000

Showing in the analytical result a loss of about 7 per cent. of oxygen and a gain of about 5 per cent. upon the carbon and nitrogen taken together. We have not been able to make these discrepancies disappear, but have no doubt that they result from the difficulties of the experiments, necessarily performed on very small quantities, and multiplied when brought out by calculation as 100 grains.

It occurred to us as being very desirable to add to the gun-cotton some oxidizing agent in sufficient quantity to furnish the necessary oxygen for the complete combustion of all the elements of the gun-cotton ; we tried chlorate of potash with this view ; we could not however get the cotton to combine with as much as we desired from an aqueous solution of the chlorate, so as to adhere to the gun-cotton uniformly ; but a combustion effected with 1 gr. of the cotton with 0·4 gr. of chlorate, and by treating the residue in the tube with a further portion of chlorate, gave us the following result :—

Carbonic acid	0·420	grs.	} containing 0·200 carbon.
Carbonic oxide	0·198	...	
Nitrogen	. . .	0·178	...
Water	. . .	0·200	...
		<u>0·996</u>	...

which differs but little from the result obtained by the imperfect combustion, and agrees completely as far as the nitrogen is concerned.

Since reading this paper an article has appeared in the *Comptes Rendus* of the Academy of Sciences at Paris, dated 23rd November 1846, that Messrs. Fordos and Gelis had obtained from gun-cotton a *cyanic compound*; this tends to confirm our discovery of the presence of cyanogen among the gaseous products.

A view may be taken of the composition of gun-cotton agreeing with the ultimate results, but arranging the elements differently, by subtracting 1 atom of oxygen from the nitric acid and adding it to the lignin; gun-cotton would then be formed of nitrous acid + oxide of lignin; this new oxide may possess alkaline properties, and thus account for the singular fact of the non-acidity of gun-cotton: we intend to try if we can establish this composition by experiment.

We have tried the action of a small galvanic battery in decomposing the gun-cotton, but without any decided results; on the first vigorous action of the battery, reddened litmus paper was made blue in the negative cell, but as the power of the battery subsided this effect disappeared. As the result of several trials, it does not appear that gun-cotton is susceptible of explosion by the discharge from a Leyden phial.

In conclusion, we think proper to express our dissent from the mechanical view taken by Mr. Crum of the nature of gun-cotton. He considers that the tubular structure which the microscope develops in cotton accounts for the admission of nitric acid into its vessels, but does not explain why it should not get out again on washing; and he thinks that the preservation of its fibrous organization is inconsistent with its chemical combination with nitric acid. Now the fact is, that when cotton is immersed in a mixture of equal measures of sulphuric and nitric acids it combines with the latter only, to the exclusion of the former, which shows a peculiar affinity inconsistent with any mechanical view of the action.







XLVI. *Microscopic Examination of the Papillæ and Nerves of the Tongue of the Frog, with Observations on the Mechanism of Taste.* By AUGUSTUS WALLER, M.D.\*

[With a Plate.]

I HAVE described in some former observations the appearances presented by the papillæ, the vascular and muscular parts of the frog's tongue. At present I propose to examine it in reference to its functions as an organ of taste, and to study those portions of it in particular which enable it to perform its sensorial offices. As these are mainly carried on by means of nerves distributed within it, which collect and convey to the brain the gustatory impressions which are produced upon its surface, our attention will be principally directed to the ultimate ramifications of the nerves, and to the structure of those parts whence the sensations arise. This investigation is the more interesting from the circumstance of the minute anatomy of the ultimate ramifications of the optic, auditory, olfactory and the common sensitive nerves having been described with great care in man as well as the lower animals, whereas the structure of the gustatory nerves has been as yet unascertained. It is not from the unimportance of the subject in the estimation of physiologists that such is the case, as it presents some peculiarities with regard to its nervous structure of great interest, but that the microscopic inspection of a soft, fleshy and opaque body, like the tongue in most animals, offered greater obstacles than any of the other organs.

Valentin states that he has been unable to obtain a distinct view of the terminal plexus and loops of the nerves of the tongue. Burdach, who has so carefully examined the nerves of the frog's tongue, makes nearly the same observation; for he expressly mentions that he was unable to examine the mucous membrane of the upper surface of the tongue, on account of its thickness and opacity. I hope to succeed in demonstrating that the organ of taste, far from being the most difficult to examine, is the most accessible, and that the beautiful and simple mechanism of taste may be followed in the depth of the tissues during the continuance of life.

In briefly recapitulating the principal points that have already been ascertained respecting the structure and functions of the tongue, we find—

1. That anatomists distinguish three kinds of elevations upon the human tongue, viz. the conical, the fungiform, and the lenticular papillæ. The conical papillæ are the most numerous, the smallest, and the most liable to change under the influence of

\* Communicated by the Author.

disease. The fungiform are larger, more red, nearly globular in form, and supported by a stem which gives them the appearance of a mushroom or berry with its stalk. They are much less numerous than the former, amongst which they are interspersed. They exist in greater numbers at the tip and borders of the tongue, where the sensation of taste is most acute. The lenticular papillæ are the largest, and are confined to the base of the tongue, and are only about fifteen in number.

2. In Berré's plates of microscopic anatomy, an injected fungiform papilla is represented, which appears entirely composed of vascular coils arranged in nearly a globular shape. Cruveilhier says, "Les papilles qui hérissent la langue représentent le corps papillaire de la peau, à son summum de développement. Elles reçoivent des nerfs. Haller les a poursuivis jusque dans les papilles. Je les ai également suivis, mais sans pouvoir déterminer comment elles se terminent."

3. Comparative anatomy shows the existence in the tongue of most animals of the same kind of papillæ as in man. Cuvier (*Anatomie Comparée*) mentions, "Ce sont les papilles fongiformes qui reçoivent tous les filets nerveux qui sont assez gros pour être suivis à l'œil nu, et cette circonstance jointe à celle de la dureté des papilles coniques dans certains animaux nous porte à croire que les fongiformes sont le siège principal du goût." Messrs. Todd and Bowman adopted the same opinion from observing the extreme thinness of the epithelium over the fungiform papillæ, as compared with that which invests the conical papillæ\*.

4. The tongue in man is supplied by three pairs of nerves, the hypoglossus, the glossopharyngeal and the gustatory, a branch from the inferior maxillary. Numerous experiments on the lower animals and pathological facts in man have shown, that the first is the motory nerve of the tongue: much difference of opinion exists with respect to the functions of the latter. The experiments of Panizza would lead us to the conclusion that the glossopharyngeal is the sole nerve of taste, but the simple deductions of anatomy and the experiments of Longet are in favour of a conjoint action of the two, the glossopharyngeal being the nerve of taste for the base of the tongue, and the gustatory that of the tip and anterior third of the same.

If we now come to the tongue of the frog, we find in it, after it has been distended for the purpose of examination, the same elements as we meet with in the human subject. The frame-

\* I must refer the reader to their excellent account of the tongue in their work on Physiological Anatomy, which I regret having consulted too late to avail myself of in this paper.

work of it consists of two muscles, the thin and membranous hypoglossus duplex muscle, arising from the lower border of the hyoid bone and advancing upwards and outwards, its fibres expanding in a fan-like shape; and the genioglossus, an impair muscle of a triangular form, whose base is inserted to the apex of the lower jaw-bone, from whence it ascends and joins the inferior surface of the hypoglossus. This last is short and thick, and does not extend beyond the posterior half of the tongue. Numerous transverse fibres of a muscular nature form several layers, and tend to increase the strength of the tongue. As in man, these vessels and nerves are principally situate at the lower surface of the organ, which becomes uppermost in this animal when spread out of the mouth. The upper surface is covered with a very thin membrane, nearly transparent like glass, with all the appearances and properties of cellular tissue. The lowermost, which is the papillary or gustatory surface, is much more thick and opake.

The surface of the frog's tongue appears to the naked eye nearly smooth and constantly covered with mucus secreted upon it. When forcibly distended, it becomes sufficiently attenuated to allow of the passage of light, and we may then detect very minute elevations over the upper surface. By means of a simple microscope, we find that the apparent evenness arises merely from the minuteness of these bodies, and that it contains all the elements of the human tongue, with the exception of the lenticular papillæ. Small bodies of a conical, filiform, or cylindrical shape, are seen over the greater part of it, corresponding to the papillæ conicæ in man. Among these are others, fewer in number, larger, mostly of a globular form, and most abundant towards the extremity of the tongue: these are the fungiform papillæ. As in man, they are supported by a kind of neck or pedicel, which is narrower than their free extremity. Sometimes this pedicel disappears, owing frequently to the condition of forcible distension of the membrane; the papilla then appears simply globular\*. These papillæ are those which to the naked eye resemble minute granulations slightly prominent above the rest of the membrane. They differ from the first-mentioned in another important feature, for they are found to contain a coil of blood-vessels, within which there is a very active circulation. The conical papillæ offer nothing of the sort; numerous vessels ramify among them at their base, without any appearance of permeating within them. The simple lens is the best to employ when we desire to trace the analogies between the organ in this animal and that of man; but when we wish to examine the papillæ

\* See Phil. Mag. vol. xxix. plate 1.

more closely, we must use the higher powers of the compound microscope, which the transparency of the membrane permits.

The papillæ conicæ of the frog consist of small elevations of an irregular form, dispersed over the whole surface of the tongue. They may generally be regarded as consisting of small cones or cylinders, two or more of which are joined by a common stem to the mucous membrane. They are completely covered with scales of epithelium, and frequently appear to have small apertures upon their surface, as if leading to some cavity within them. In some, this opening is a small elongated depression at the apex of the papilla; in others it is sharp and well-defined, and below it some scales of epithelium appear lining the commencement of the papillary duct. When the tongue is much distended, these papillæ almost entirely disappear, as if they were simply formed of rugæ or folds of the mucous membrane. They present no appearance of circulation. At different ages of the animal they vary considerably. In the young they are smaller and more transparent than in those full-grown, where they are generally like truncated cones, distinct from one another. At their summit the elongated aperture already mentioned is generally seen. In the *Philosophical Magazine* one of the most common appearances of these papillæ is represented.

The fungiform papillæ, which are so easily distinguished from the former by their active circulation, deserve principally to arrest our attention. The blood, which when seen with the lower power appeared to move within a small cavity, without being contained in any capillary, is found, on being subjected to higher power, to circulate within the usual canals, which are coiled up into a very small space, so as to cause the blood to trace a very circuitous route before it is enabled to make its exit. The exact course which it takes may often be traced where the circulation is languid, by the gradual progress of the blood-discs. The coil is connected with two and sometimes with three small vessels, which supply it with blood. No difference can be detected between these supplying vessels, either with regard to calibre or thickness. They are to be considered as part of the capillary network, and they are often seen to convey the blood in opposite directions, when watched for a few minutes. In most instances the supplying vessels arrive from opposite points, and the circular papilla appears nearly on a level with the rest of the membrane; but in others the papilla is like a gourd or wide-necked flask with the larger end pendent and free. The vessels then run close together, traversing the neck, and after ascending upwards unite with the coil.



The nerves are derived from two pairs, the first of which traverse a foramen together with the vagus at the base of the cranium, between the occipital and sphenoid bones. The second arise from the first cervical nerves, and traverse the foramen between the first and second vertebræ of the neck. Both pairs make a curve which is directed forwards and downwards, and descend nearly in a parallel direction to the hyoid bone, from whence they ascend to the concavity of the lower maxillary and enter the tongue, where their main trunks, much diminished in size, may be traced to the two tubercles at its extremity. The cervical nerves, which at their commencement are nearly on the median line, and are internal to the cranial pair at their origin, retain the same relative position until they enter the tongue, where they begin to ramify, the two cranial nerves then becoming external. The cranial nerve in its passage to the inferior maxillary bone traverses the posterior belly of the digastricus, descends until it reaches the hyoid bone, when passing under the middle and over its great cornua, situate either beneath or by the side of the hypoglossus muscle, continues in that position until it enters the tongue, without giving off any branches to the muscles of the neck. The cervical nerve follows nearly the same direction until it reaches the hyoid bone under which it passes, and likewise places itself under the hypoglossus muscle, penetrating with the latter into the tongue, accompanied closely by the lingual arteries and veins. It supplies numerous branches to the muscles of the throat and neck. This nerve is much more considerable than the former, and is much more curved. The fifth pair or trifacial sends branches to the mucous membrane of the mouth, which appear to reach the posterior part of the tongue, but it gives off no nerves to that organ, corresponding to the lingual branch of the inferior maxillary.

Notwithstanding the transparency of the tongue when in a state of distension, we find considerable difficulty in tracing the ramification of its nervous trunks within the parenchyma. At the upper surface, where they are most superficial, we may under favourable circumstances, with the simple microscope, be enabled to follow the various ramifications of a large branch to a considerable extent, but we are unable either to command a general view of the distribution of a single trunk, or of distinguishing with any degree of certainty the ramifications of one pair from another.

The best mode to examine the nerves in the dead animal is to disarticulate the lower jaw-bone on one side, and to divide the throat on that side sufficiently to allow the jaw to be turned completely backwards. The passage of these nerves may be

followed from their exit from the cranium and spine, until their immersion into the tongue. After removing the mucous membrane of the surface, we may easily trace, by means of the scalpel, with the naked eye the distribution of the principal trunks nearly on the median line, giving off numerous branches, and finally proceeding much diminished in size nearly to the two tubercles on each side. By immersion in alcohol or naphtha, the nerves become whiter and still more distinct. The cervical and cranial pairs then appear unconnected with each other during their course; and Burdach is of opinion that no communication is ever found to exist between these two pairs of nerves, the first of which he terms the hypoglossus, and regards as a pure motor nerve, and the other the glossopharyngeal, which he looks upon as destined entirely to the mucous membrane.

My observations do not allow me to adopt this opinion entirely, as I have seen with the naked eye and with the microscope numerous communications, almost of a plexiform nature, between these trunks at their entrance into the tongue. I have therefore preferred to adopt the terms of cranial and cervical pairs, which are independent of all theoretical ideas. I consider the cervical as destined principally to the muscles, and the cranial to the mucous membrane, where I shall presently describe them. When the muscles have been divested of their papillary membrane, we are able to trace without difficulty, the various ramifications and nervous loops among the muscular fibres, where we see the nervous fibres nearly reduced to a single tubule, forming numerous loops or meshes, running either across or parallel to the striated muscular fibres. These nervous trunks and fibres appear all to be derived from the cervical nerve. Instead of completely removing the mucous membrane, which often renders the examination difficult, from the blood which issues from the divided vessel, we may remove a small portion of the membrane, which when done by tearing it away, will frequently be unattended by loss of blood. The surface thus exposed is usually found covered with a dense plexus of nervous ramifications, which appears to belong to the muscular system. Sometimes in spreading out the tongue we separate from it a kind of investing sac of cellular tissue, nearly as transparent as glass. The microscope detects in this numerous muscular fibres, generally accompanied with nerves and capillary vessels. The nervous fibres are usually accompanied with a satellite capillary vessel closely joined to them. To complete this brief account of the microscopic muscular appearances, I will mention that the circulation may be easily observed in these muscular capillaries, and that a

muscular contraction produces a constant acceleration of the blood, if previously stagnating in them. I have not detected any valves in these capillaries, such as are seen in some of the larger veins of the frog's tongue. Muscular striæ may be seen at the upper and lower surfaces of the tongue, through the layer of epithelium and cellular tissue. The contraction of the muscular fibres may be frequently observed, either uncovered or through the epithelium. I have examined them sometimes in an animal under the influence of strychnia; in none of these cases have I found either the zigzags described by Dumas and Prevost as characteristic of muscular contractions, or the approximation of the transverse striæ, as described by more recent observers.

To return to the nervous fibres. Burdach has employed another means of unveiling the distribution of the nerves, which consists in immersing the tongue for a few minutes in a solution of potassa, in the proportion of three or four drops of alkali to an ounce of water. By this method the membrane of the back of the tongue becomes rapidly converted into a transparent viscous substance like the white of egg, which is easily removed, leaving the subjacent parts nearly unaffected. By compressing the tongue between two pieces of glass, we may trace the distribution of all the principal nervous branches of the organ. The four nervous trunks advance forwards in a parallel direction to the tubercular extremity, giving off numerous branches on their external sides, which branches subdivide and ramify towards the borders of the organ. On their internal sides they give off no branches, and appear to have no anastomoses one with another. The same unilateral ramification is observed with regard to the vascular canals, and may in a great measure be accounted for by considering, that as they and the nerves are all near the median line of the tongue, the parts to be supplied are placed to the right and left sides of this line, and the nervous and vascular trunks are only in proportion to the parts to be supplied.

The cervical nerves are seen to form numerous plexiform anastomoses between their various branches, and to distribute frequent loops composed of nearly single fibres among the muscular parts. The cranial pair ramify in a unilateral manner until they attain the tubercular extremity, where branches are given off on each side of the main trunks: Burdach states that this nerve presents no plexiform arrangement like the former, that a few of its fibres at the extremities form curves or arches, but that generally they terminate abruptly in a bush-like manner, hence his term "*terminaison en buisson.*" In describing his process, Burdach has not concealed its defects in

destroying the papillary surface of the organ; and also he commits the mistake of regarding the papillæ as glandular bodies, as we may see by the following passages. "There are considerable difficulties, even in the frog, in recognizing the course of the nerves in the interior of the tongue; for independently of the thick layer of mucus secreted on its surface, this organ is obscured by a multitude of granules or opake knots, globular in form, which are doubtless mucous glands; and likewise by numerous canals, variously twisted, of a thickness nearly equal to  $\frac{1}{300}$ th of a line, which appear to be filled with corpuscles resembling the blood-discs, and which are either canals to convey the mucus, or lymphatic vessels; and lastly by numerous blood-vessels. Therefore we soon become convinced of the impossibility of distinguishing anything without the employment of artificial means . . . . . An impossibility already experienced by Valentin and which my own researches have confirmed . . . . . All chemical substances, whatever they may be, when applied during the life of the animal on the surface of the tongue, have always produced an increase in the afflux of the blood to the organ, and have still more injured its transparency\*."

In order to detect the real extremities of these nervous filaments, we have but to examine with care the papillary surface of the frog's tongue. If this is well extended and not over-injected with stagnant blood, we shall have no difficulty in discerning, at the base of each fungiform papilla, a dark gray spot formed by a nerve nearly the size of a capillary vessel. In fig. 1 we have a very correct representation of a papilla as it appears in a favourable subject. The nerve, of which we see a portion, pursues a very winding course. Near the base of the pedicel of the papilla it is usually twisted, as is represented, into a kind of loose knot composed of several loops. This spot is the darkest portion of the nerve, and is to be seen in most of the fungiform papillæ. From this point the fibres ascend apparently less numerous, and accompany the blood-vessels in their numerous gyrations within the papilla. By reason of the darkness and opacity of the blood-vessels the numerous convolutions are generally concealed, and it is only at the intervals between the coils that we are enabled to detect the nervous tubes. The principal varieties which I have had occasion to remark with respect to these papillary nerves, are that sometimes the efferent and afferent capillaries run close together joined to the nerves, which in that case are in a great measure concealed. Sometimes the nerve, like the blood-vessels,

\* Burdach, *Structure des Nerfs*.—*Annales des Sciences Naturelles*, vol. ix, 2ième series.



arises from two branches which converge near the papilla as if they were afferent and efferent. It will be preferable that the blood should be found circulating in the papilla, in order to detect more readily the papillary nerve. In cases where the animal has been killed by strychnia, I have always found these nerves more easily distinguishable, as if this substance exerted some effect on the constitution of the nervous system.

I considered it important to determine whether any vesicular matter existed in the papilla, such as has been described to exist at the extremities of some of the nerves of sensation. In one or two instances I have distinguished some granular matter, but in most cases I have not been able, whether from its non-existence or from its being hidden by the engorged vascular coil, to detect the slightest trace of it.

Besides these nerves of the fungiform papillæ, there exist others which are spread over the lower surface of the tongue. They consist of nearly simple nervous fibres, forming a kind of network under the mucous membrane. This nervous network corresponds to the capillary network distributed to the mucous membrane, in the same manner as the nervous papillary coil corresponds to the vascular one. During winter the epithelium of the frog's tongue becomes thinner and more transparent, and the animal is also in an anæmic condition, from which causes the vessels of the tongue are but very slowly engorged; these cases are therefore the most suitable for the inspection of the nerves.

*Tongue of the Toad.*—The toad's tongue presents very close analogies to that of the frog. I have already described in this journal the slight differences which distinguish them from each other. I shall now merely state what a further investigation has shown me with respect to their nervous structure and papillæ. The difficulty of procuring these animals in the winter has prevented my thorough examination of the distribution of the nervous trunks of their central parts, but as far as I have examined I find a similar distribution to those in the frog. The toad's tongue is less extensible and more thick than that of the frog, and for that reason it is necessary to select a very small animal for microscopic examination. The nervous trunks which ramify in the muscles and accompany the vessels appear to be more numerous than in the frog. The muscular striæ are more distinct, and the fibres are arranged more like separate muscles. The papillæ conicæ, or rather those that correspond to them, may be compared in general to irregular folds or plaits of the intestines; others are like small cylindrical villi; while some fewer are convex elevations, with a very small dark circular foramen in the centre and a passage leading perpendicularly within them.

The fungiform papillæ do not generally project upwards, but consist of a circular area containing a vascular coil and surrounded by an elevated rim of the mucous membrane. Scales of epithelium cover the papillæ conicæ, but the mucous membrane is generally too opaque to admit of the circulation of the blood being clearly distinguished, except at the neuro-vascular papillæ, where it is seen in the coil and at numerous spots about the size of  $\frac{1}{100}$  mm, where the mucous membrane appears to be removed, so as to expose fully the capillary circulation beneath. On these spots the forms of the blood-globules are so distinct as to give the appearance of not being confined in a vascular tube. The blood frequently stagnates at these points, or bursts through and is effused in small quantities over the papillary surface, where the separate globules may be seen to oscillate, evidently under the influence of the cilia spread over the surface. In favourable cases nervous filaments may be traced into the vascular area, similar to those of the frog, forming a knot, which afterwards gives off filaments accompanying the vascular coil.

These papillæ and their nerves were most clearly displayed in a toad not half-grown, which had been kept prisoner the whole of the winter. From some accidental cause it had been injured, which had produced great emaciation, and finally occasioned its death. When examined life was extinct, but the muscles were flaccid and circulation of blood existed in the tongue. This organ was beautifully transparent; the papillæ conicæ were as before described; some of the neuro-vascular papillæ had a circular area with coils; others, mostly confined to the extremity of the tongue, consisted of nearly globular transparent vesicles, projecting above the membrane and connected with it by a very narrow neck containing two capillary vessels and a nerve. Near the point of insertion of the vesicle to the membrane a nervous knot was seen, from whence numerous nervous fibres proceeded up the pedicel into the vesicular cavity. Numerous similar vesicles existed at the tubercles, where they formed nearly a continuous row, the transparency of the membrane allowing the nervous filaments which supplied them to be traced backward to the main trunk, which they joined at an acute angle. The vascular coil could likewise be followed to similar venous and arterial trunks, closely accompanying the nerve. This beautiful arrangement may be aptly compared to a bunch of currants, each currant representing a vesicle with its internal coil, and its pedicel to the vasculo-nervous neck of the papilla. As all the papillary nerves joined the main trunk at acute angles, we may easily figure to ourselves the appearance of the termination of the filaments of the cranial nerve, as mentioned by Burdach, by stripping off

the currants and leaving the principal stalk with their projecting pedicels.

*Physiological action of the Tongue.*—The fungi form or neuro-vascular papillæ are the parts which more especially deserve our attention, as their structural arrangement points them out as being evidently the principal if not the sole organs of taste in the tongue. Considered under the simplest point of view, they may be regarded as hollow vesicles, containing a coil of nerves and blood-vessels within them, and when a liquid is brought in contact with its external membrane, its thinness causes it to be rapidly permeated by the sapid substance which then comes in immediate contact with the nerve where the sensation of taste is created. The capillary coil then by its internal current and its extensive surface, rapidly carries it away and leaves the nerve free to receive a fresh impression. In analysing this mechanism, we may for convenience distinguish three separate periods or stages. In the first the body permeates the vesicular membrane; in the second it comes in contact with the nerve and produces an impression which is conveyed to the brain; in the third this substance is eliminated from the cavity and from the nerve. The first is a well-known property of all animal membranes, of allowing themselves to be traversed by the various liquid and gaseous substances with which they are in contact. We have an instance of this when venous blood contained in a bladder and exposed to the air or immersed in water containing air in solution, becomes crimson from the passage of the oxygen through the membrane into the blood. A similar action takes place when various other liquids and gases are separated from each other by an organic membrane. Acetic acid applied to the tongue of the frog over a blood-vessel filled with stagnating blood, destroys its red colour and dissolves the tunic of the vesicles in the same way as when it is applied to blood taken from the vessels. A still more notable example is presented by the branchiæ of the tadpole, which may be observed during life. There the blood is perceived circulating with great rapidity in the capillary vessels, and separated from the surrounding water by a very thin membrane. Nevertheless in virtue of the permeability of this membrane, a constant current exists which causes the oxygen in the water to be absorbed by the blood, and the carbonic acid of the blood to pass into the water. In this manner the respiration of the animal is effected. The rate at which a membrane is traversed by a liquid is *ceteris paribus* in proportion to the thinness of the membrane. At the branchial surface it is extremely thin, on account of the distinctness with which the globules of the blood appear through it. For the same reason, it is evident that the



membrane of the papilla, being also very thin, is easily permeated. On the contrary, the thickness of the epithelial scales over the rest of the surface indicates that there the same action is comparatively very feeble and slow. It is necessary for a substance to be in a state of solution before it is capable of traversing a membrane, for if merely suspended or floating in a liquid its progress is completely arrested by the membrane. The same remark also applies to the organ of taste, which is only capable of receiving an impression from a body which can be dissolved in the saliva, or is already in a state of solution.

To further accelerate the passage through the membrane, the body when placed in contact with the vesicle is subjected to a certain amount of pressure by means of the application of the tongue to the roof of the mouth. The effect of pressure in increasing the action of absorption is well-known from numerous facts; its influence in the action of taste is shown by the feebleness of the sensation generally caused by a body when merely applied on the surface of the tongue, compared to the acuteness of perception which ensues on its being compressed between the tongue and the palate. If the surface of the palate had been entirely smooth, the amount of pressure thus sustained by a substance in a liquid or a pulpy state would be very small, but by the existence of small folds and transverse depressions on its surface the body to be tasted becomes lodged and fixed between them, and thus exposed to a much greater amount of pressure. The importance of this compression of sapid substances receives additional evidence from the fact that the gustatory membrane is invariably disposed in a manner to ensure their compression; and that the soft palate, which likewise has the power of taste, may be regarded as squeezing the sapid substance when passing from the throat into the stomach.

When the substance is perfectly insoluble, the only sensation of its presence is of a tactile nature, like that of the hand, which enables us to appreciate the size, form, degree of resistance and temperature of the body. The resemblance of the papillæ conicæ to the papillæ of the organs of touch, render it very probable that they are destined for the same purposes and fulfill the same functions. The areolar network of the nervous fibres at the base of these papillæ is very similar to the terminations of the nerves in the skin, and tends to confirm the idea of the tactile nature of the papillæ conicæ. Supposing all impressions to arise from a molecular perturbation of the ultimate nervous fibres, the agent in this case, which causes the perturbation, is either simple mechanical contact or caloric. Neither of these agents is liable to accu-



multate around the peripheric extremities of the nerve, like a chemical substance; it is therefore a matter of no surprise that these nerves are not surrounded by so rich a vascular expansion. Nay, further, with respect to the action of caloric, we are aware that in virtue of its conductivity it tends quickly to attain a state of equilibrium, which in extreme cases may be in a degree hastened by the vascular circulation of a fluid of an equal temperature like the blood, which if developed to the same extent as in the fungiform papillæ, would, it is rational to suppose, so much equalize the distribution of caloric as to render the nerve incapable of distinguishing minute differences of temperature. But without entering any further into these speculations out of the pale of experimental science, it is proper to state that numerous facts derived from physiology and pathology are in favour of what I have deduced from the examination of the anatomical structure of the two species of papillæ, namely, the existence of two distinct species or sets of organs in the tongue, those for appreciating the chemical properties of bodies, and those for taking cognizance of their physical and mechanical attributes. In numerous instances the faculty of taste is entirely destroyed, leaving the tactile properties unaffected.

With regard to the second period or stage, when the blood comes in contact with the nerve and produces the impression which is conveyed to the brain, we can form no idea of the various actions which are produced within the nerve; we can only surmise that these impressions may either resemble electric currents, such as arise from the contact of two heterogeneous substances, or that they are caused by a kind of molecular perturbation, which is transmitted upwards to the brain, where it produces a kind of image of a more permanent nature. We know that different liquids vary greatly in their power of traversing the same membrane; alcohol and water, for instance, in a bladder become separated from each other by the water traversing the membrane and the spirit remaining behind. To this cause may perhaps in some small degree be attributed the difference in the sensations which different substances give rise to; but by far the most efficient cause is probably the peculiar manner in which the nerve is affected, as different rays of light excite different sensations independently of the intensity of their action. My experiments have not enabled me to ascertain whether the nervous fibrils supplying the two kinds of papillæ are the same or of different origin, or whether they are rendered distinct in their actions merely by means of the structure of the parts around them.

The third stage may be considered as simply a kind of per-

*Phil. Mag. S. 3. Vol. 30. No. 201. April 1847.* X

meation or imbibition, very similar to what we have described in the first. It is evident that if a substance were allowed to remain permanently in contact with the nerve, the impressions would be greatly defective, as the substances applied to it would be mixed. To remedy this evil, it is requisite that a means should be found of eliminating a substance from the nervous extremities nearly as quickly as it is allowed to reach them. The nerve when freed is capable of receiving a second impression as clear and distinct as the first. We may easily understand how this may be effected by means of the vascular coil; for if we suppose for a moment that this coil is filled with stationary blood, we see no reason to prevent the diffusion of the substance in the capillary area from taking place in the same manner as in the cavity of the vesicle. If the capillary is now emptied and filled with fresh blood, the same action of imbibition and of solution will take place a second time, and so on as fast as the blood is changed and renewed. In this way we obtain a constant current from the exterior to the interior of the vesicle; and if the sapid substance has quitted the surface of the tongue, the absorption will continue until by an action of endosmosis and exosmosis it becomes entirely removed from the vesicle. The existence of a constant current in the coil undoubtedly much accelerates the removal of the sapid substance from the vesicle, as we observe that whenever an active imbibition takes place, as in the branchiæ of the tadpole, there exists a corresponding increase in the vascular system.

#### EXPLANATION OF PLATE III.

*Fig. 1.* Fungiform papilla with nerve and blood-vessel.

*a.* Nerve at the base of the papilla, where it forms an intricate coil. Its tubules separate as they reach the internal convex border of the papilla, where they form numerous convolutions in the surface of the capillary.

*b.* Afferent capillary.

*c.* Efferent capillary.

*Fig. 2.* Upper surface of the tongue of a young frog, with conical and fungiform papillæ.

The papilla with the large dark area is a fungiform papilla in its early stage of development. Above is another papilla of the same kind, but more developed, with the central nucleus much diminished. Around these are seen conical or tactile papillæ.

#### XLVII. *Intelligence and Miscellaneous Articles.*

ON THE BEHAVIOUR OF METALLIC ZINC TOWARDS SOLUTIONS OF MERCURY. BY H. ROSE.

**T**HE behaviour of metallic zinc towards solutions of the peroxide and perchloride of mercury is highly remarkable. The zinc

throws down the whole of the mercury from solutions of the pernitrate and persulphate, when the separation of basic salts has been prevented by the addition of nitric or sulphuric acid. The zinc does not combine with the mercury, but the latter separates in the form of gray globules, which collect at the bottom of the vessel; the zinc retains its appearance, becoming only somewhat more gray than before the experiment.

Zinc behaves in the same manner towards a solution of the perchloride of mercury; its appearance is not changed, and the mercury separates in gray globules; but when hydrochloric acid has been added to the solution of the perchloride, the immersed bar of zinc soon becomes quite bright and shining. In this reaction no evolution of gas occurs; only a few bubbles adhere constantly to the bright surface of the amalgamated metal; the mercury is only imperfectly precipitated, for even after several days its presence may be detected in the solution. It is true that, when the zinc is left for a longer time in contact with the liquid, the mercury is at last entirely separated, but not however as a precipitate of gray globules; but the whole of it combines with the zinc and amalgamates it. At all events, when free hydrochloric acid is present, the mercury is separated far more slowly, and under totally different phenomena, than from the solution of the pure perchloride.

When the experiment is somewhat modified, the result is still more surprising. For instance, if a bar of zinc be immersed in hydrochloric acid, and when the evolution of hydrogen is very rapid, a solution of perchloride of mercury be added, the disengagement of gas ceases instantly; the zinc becomes bright, resulting from its amalgamation; and after several days the mercury is only imperfectly separated from the solution. If a solution of perchloride of platinum be now added, evolution of hydrogen immediately recommences.

When the mercury has been entirely separated from a solution of the perchloride by zinc in the form of gray globules, and hydrochloric acid added, the zinc soon becomes bright and amalgamated, without however the mercury, which has been separated in the form of globules, wholly combining with the zinc.

The behaviour of amalgamated zinc is very remarkable. Döbereiner observed that no evolution of gas resulted on the contact of amalgamated zinc with acids; and recently Millon has drawn attention to the fact, that certain metallic solutions hasten the evolution of hydrogen by means of zinc and dilute sulphuric acid (a phenomenon which has been satisfactorily accounted for by Barreswil), while a solution of perchloride of mercury retards this disengagement considerably.

Zinc, as is well known, is most readily amalgamated by immersing it in hydrochloric acid, and then rubbing it over with mercury. Zinc thus amalgamated remains perfectly bright when placed in hydrochloric acid, in which it scarcely dissolves at all, or so little, that only a few bubbles of gas adhere to the shining surface. As is well known, amalgamated zinc behaves, in an electrical point of

view, almost like a distinct metal; and, notwithstanding it is but little acted upon by acids, is more positive than zinc alone. If, therefore, another metal be placed by the side of amalgamated zinc, which is in contact with a dilute acid, a powerful evolution of gas immediately results on the former when it touches the amalgamated zinc. It is requisite, however, that, in order to retain its remarkable behaviour, especially towards perchloride of mercury, there must always be some free acid present, without which it acts like ordinary zinc. When an amalgamated bar of zinc is immersed in a solution of perchloride of mercury, the mercury is separated exactly as with pure zinc in the form of gray globules, and the amalgamated zinc loses its bright coating. In the same way amalgamated zinc reduces solutions of the sulphate of copper, nitrate of silver and acetate of lead, like ordinary zinc, with this difference, that an addition of hydrochloric or nitric acid does not prevent or retard the reduction of the metals by the amalgamated zinc, as is the case with the solution of perchloride of mercury.

When only a few drops of hydrochloric acid are added to a solution of the perchloride of mercury, the zinc is amalgamated; but after some time mercury separates in the form of gray globules, which does not occur when a larger quantity of hydrochloric acid has been employed.

Dilute sulphuric acid acts like hydrochloric acid in preventing or retarding the precipitation of the mercury from the solution of the perchloride; frequently, in this case, a fine crystalline salt separates, which however is only perchloride of mercury, which is far less soluble in dilute sulphuric acid than in water; subsequently a crystalline powder separates in minute scales, coating the zinc, and frequently floating upon the surface of the liquid; this is the protochloride of mercury, which is not further altered by the zinc. It is remarkable that the protochloride is only separated by zinc from the solution of the perchloride when sulphuric acid has been added, and not from a solution of the pure chloride, nor on the addition of hydrochloric acid. Moreover, when a solution of the perchloride of mercury is poured upon zinc immersed in dilute sulphuric acid, the evolution of gas suddenly ceases, just as with the addition of hydrochloric acid. The zinc likewise becomes amalgamated, and a few bubbles of gas adhere for a long time to its surface. Nitric acid also prevents the precipitation of the mercury from the solution of the perchloride by zinc, in the same manner as hydrochloric and sulphuric acids; in this case the protochloride is also formed. When a violent disengagement of nitric oxide has resulted from immersing zinc in nitric acid, it is instantly stopped by the addition of a solution of the perchloride of mercury. It is well known that neither sulphuric nor nitric acids in the dilute, and also in the concentrated state, are capable of decomposing the perchloride of mercury.

Iron completely precipitates the oxysalts of mercury in the same manner as zinc; the metal is likewise wholly separated by iron from a solution of the perchloride, and an addition of hydrochloric acid neither prevents the evolution of gas nor the elimination of the mer-



cury; as the iron is not amalgamated, the mercury separates in the form of globules.

While zinc entirely precipitates the mercury from a solution of the protonitrate, it cannot decompose the protochloride even when recently precipitated and in the presence of water. The insolubility of the salt is by no means the cause of this phænomenon, as the chloride of silver is most quickly and rapidly decomposed by zinc in the presence of water. Even on the addition of hydrochloric acid, the protochloride is not decomposed by zinc, or only to a very small extent when it has been left for several days in contact with it and the acid. Scarcely any evolution of gas is perceptible in this instance; nevertheless the liquid, after a short time, contains oxide of zinc in solution.

The insoluble protosulphate of mercury is likewise not reduced by zinc in contact with water; however, after several days some oxide of zinc has dissolved, and the zinc is slightly amalgamated; more is reduced when dilute sulphuric acid is added, but still the quantity is very inconsiderable; there is no evolution of gas, but nevertheless the zinc is amalgamated.—Poggendorff's *Annalen*.

#### ON THE COMPOSITION OF THE ORGANIC ALKALIES.

BY M. AUG. LAURENT.

The author observes that the elementary analysis of the organic alkalies is attended with great difficulty, for a difference of two- or three-thousandths of hydrogen is sufficient to change their atomic formula. The greater number of analyses show an excess of hydrogen over the calculated result, which usually amounts to the quantities above-mentioned, or about one equivalent. The author contrived an apparatus to ascertain whether he could not determine the hydrogen to about one-thousandth; and he remarks that two formulæ are given for morphia; that by Liebig being  $C^{34} H^{36} N^2 O^6$ , and that by Regnault  $C^{35} H^{40} N^2 O^6$ . The result of the author's analysis is stated by him to be—

$C^{34}$ .....	2550·0
$H^{36}$ .....	237·5
$N^2$ .....	175·0
$O^6$ .....	600·0
	<hr/>
	3562·5

And he further observes, that the analyses of Liebig and Regnault were calculated according to the ancient atomic weight of carbon; whereas with the number 75 the results of Regnault's analysis would be the same as his, or 71·7 and 71·85.

*Quina*.—M. Liebig's formula for this alkali is  $C^{20} H^{24} N^2 O^3$ , which, according to M. Regnault, ought to be doubled. M. Laurent repeatedly crystallized sulphate of quina, then extracted the alkali, and dissolved it in æther; the solution was evaporated, and the quina kept for a long time in fusion and then analysed. The results were—

C <sup>38</sup> .....	2850
H <sup>44</sup> .....	275
N <sup>4</sup> .....	350
O <sup>4</sup> .....	400
	<hr/>
	3875

*Cinchonia*.—M. Liebig represents cinchonia by C<sup>30</sup> H<sup>22</sup> N<sup>2</sup> O, and M. Regnault by C<sup>20</sup> H<sup>14</sup> N<sup>2</sup> O, or rather by double. M. Laurent's analysis gives—

C <sup>38</sup> .....	2850
H <sup>44</sup> .....	275
N <sup>4</sup> .....	350
O <sup>2</sup> .....	200

It appears therefore that cinchonia differs from quina in containing only half as much oxygen.

The author concludes by giving the following formulæ as the results of his analysis:—

Quina .....	C <sup>19</sup> H <sup>22</sup> N <sup>2</sup> O <sup>2</sup>
Cinchonia .....	C <sup>19</sup> H <sup>22</sup> N <sup>2</sup> O
Morphia .....	C <sup>17</sup> H <sup>19</sup> NO <sup>3</sup>
Quinoleina .....	C <sup>9</sup> H <sup>7</sup> N
Lophia .....	C <sup>23</sup> H <sup>16</sup> N <sup>2</sup>
Picryle .....	C <sup>21</sup> H <sup>15</sup> NO <sup>2</sup>
Narcotina .....	C <sup>23</sup> H <sup>25</sup> NO <sup>7</sup>
Cotarnina .....	C <sup>12</sup> H <sup>13</sup> NO <sup>3</sup>
Narcogenina .....	C <sup>18</sup> H <sup>19</sup> NO <sup>3</sup>
Opianic acid .....	C <sup>10</sup> H <sup>10</sup> O <sup>5</sup>
Opianate of ammonia .....	C <sup>10</sup> H <sup>13</sup> NO <sup>5</sup>
Hemipinic acid .....	C <sup>10</sup> H <sup>10</sup> O <sup>6</sup>
Hemipinate of ammonia .....	C <sup>10</sup> H <sup>16</sup> N <sup>2</sup> O <sup>6</sup>
Starch .....	C <sup>12</sup> H <sup>20</sup> O <sup>10</sup>
Pyroxyline .....	C <sup>12</sup> H <sup>17</sup> N <sup>5</sup> O <sup>27</sup>
Conina .....	C <sup>8</sup> NH <sup>15</sup>

*Ann. de Ch. et de Phys.*, Mars 1847.

#### ON THE CHANGE EFFECTED IN HYDRATED PEROXIDE OF IRON WHEN KEPT IN WATER. BY M. G. C. WITTSTEIN.

It is well known that a solution of persulphate or perchloride of iron gives a reddish-brown precipitate with ammonia, and it is flocculent and very bulky: its formula is  $\text{Fe}^2\text{O}^3 + 3\text{HO}$ , and it dissolves perfectly in cold acetic acid. It does not however appear to be known, that this precipitate, when kept under water, and without having been previously dried, almost entirely loses its property of dissolving in acetic acid, as if it had been dried. Some other organic acids, which readily and completely dissolve recently-precipitated peroxide of iron, such as tartaric and citric acids, &c., also dissolve a much smaller quantity of the oxide which has been long kept under water. M. Wittstein states that some researches which he has made on this subject have afforded him very satisfactory results.

If the recently-precipitated and washed oxide be examined by the microscope, it will be seen to be composed of amorphous globules, among which no crystals are perceptible; on the contrary, the precipitate which has been long kept under water appears to be entirely crystalline: the small fragments of crystals are of a deep yellow colour and slightly translucent. The author states that he is not aware of the time required to convert the amorphous into the crystalline peroxide; but the precipitates on which he made his experiments had been prepared more than two years previously. He thinks, however, that the transformation had taken place for a considerable time; for he remembers to have remarked that in six months the precipitate had altered in appearance and become more compact. The hydrate, moreover, in assuming the crystalline form, loses half the water which it contained; the formula of the precipitate which has undergone this change is  $2\text{Fe}^2\text{O}^3 + 3\text{HO}$ .

The difficulty then which attends the solution of hydrated peroxide of iron that has been long kept under water, depends upon two causes, the crystalline form and partial dehydration. It results from what has been above stated, that the peroxide of iron, in order that it may be dissolved by the acids named, and weak acids in general, ought to be employed soon after precipitation. It is probably not indifferent that the peroxide of iron employed as an antidote to arsenic should be recently-precipitated; at all events, preference ought to be given to recently-precipitated oxide, and it will be proper to renew it every six months, or annually. It is not requisite entirely to reject the hydrate which has been kept; it may be dissolved in hydrochloric acid and again precipitated by ammonia.—*Journ. de Pharm. et de Ch.*, Fevrier 1847.

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ON VILLEMITE. BY MM. DELESSE AND DESCLOIZEAUX.

The mineralogical collection of M. De Drée contains a *brownish silicate of zinc* from Franklin, New Jersey, United States. It appears to have been already analysed by MM. Vanuxem and Keating, who have referred it to the villemite of Levy; but as it differs much in appearance from the villemite found in Europe, and as its analysis does not agree with that of a mineral analysed by Dr. Thomson, and identical as to its physical properties, and from the same locality, MM. Delesse and Descloizeaux thought it would be interesting to make a comparative examination of the two minerals.

The villemite which MM. Delesse and Descloizeaux analysed was from the zinc mines of Vieille-Montagne near Aix-la-Chapelle: it is in small crystals, of a light rose colour, and has the form of a regular hexagonal prism, terminated by an obtuse rhomb of an angle of  $128^{\circ} 30'$ , as described by Levy and Phillips. These crystals were contained in the druses of a ferruginous calamine.

The silicate of zinc from New Jersey was also crystallized, but confusedly so; sometimes however the crystals are very well defined, and they are referable, both as to form and cleavage, to the villemite of Levy. The angles are the same; the cleavages perpen-

dicular to the axis of the prism are also evident, as well as those which are parallel to the faces of the regular hexagonal prism. The specific gravity of the mineral from New Jersey was 4.154.

By calcination, the silicate of zinc from New Jersey and the villemite lose only 0.005: this small loss is probably owing to a little hygrometric moisture, and to the oxides of iron and manganese which accompany both minerals; the villemite becomes of a slight brick-red colour. As to the New Jersey silicate, it has a brown colour, a resinous lustre, and yields a powder of a light dirty green colour, which becomes deeper by calcination: like villemite it is infusible by the blowpipe; with carbonate of soda on platina foil it becomes green, indicating the presence of manganese; in borax it dissolves perfectly, with a slight colour indicating iron; with phosphoric salt, a skeleton of silica remains in the globule.

The silicate of zinc from New Jersey, like villemite, is very easily acted upon by hydrochloric acid, and the silica separates in the gelatinous state; it then produces an incrustation around the central part, and it is necessary to continue the ebullition for some time, that the action may be complete.

For a quantitative analysis about fifteen grains were dissolved in hydrochloric acid; after having separated the silica in the usual way, the liquor was evaporated a second time, by which a little more was obtained; the zinc was separated by solution in potash, by repeatedly treating the residue insoluble in acid: the iron was separated from the manganese by succinate of ammonia; no cadmium was detected.

There were by these means obtained, by the analysis of the villemite of Vieille-Montagne and the silicate of zinc from New Jersey the following results:—

*Vieille-Montagne.*

Silica .....	27.28
Oxide of zinc .....	72.37
Protoxide of iron .....	0.35
	<hr/>
	100.00

*New Jersey.*

Silica .....	27.40
Oxide of zinc .....	68.83
Protoxide of manganese .....	2.90
Protoxide of iron .....	0.87
	<hr/>
	100.00

The results of these analyses agree very well with those of villemite and silicate by MM. Thomson, Levy, Vanuxem and Keating. It follows that manganeseous silicate of zinc of New Jersey, and the villemite, though at first presenting different appearances, belong to the same mineral species, which is a tribasic silicate of zinc  $\text{Si Zn}^3$ ; or supposing that silica contains only two atoms of oxygen, they will be bibasic silicates of zinc  $\text{Si Zn}^2$ .—*Ann. des Mines*, tome x. 1846.



**SOLUBILITY OF CARBONATE OF LIME IN WATER CONTAINING CARBONIC ACID. BY M. LASSAIGNE.**

The solubility of carbonate of lime in water containing carbonic acid has been long known to geologists, mineralogists and chemists ; it is in this state that all waters which contain this calcareous salt convey it for the assimilation of plants and animals.

The spontaneous precipitation of this salt, by contact of the air, fully explains the cause of the deposits formed by the water, and of the incrustations, frequently so abundant, which occur on the surface of bodies exposed to the water for a certain time.

There are other facts, however, connected with the subject which have not been examined into : such are the degree of solubility of carbonate of lime at common temperatures in water saturated with carbonic acid, and the state of saturation in which the salt exists in this condition.

The plan on which M. Lassaigue operated was to prepare a solution of pure carbonic acid by agitating at a temperature of 50° F. and a pressure of 29·7 inches : in this solution chalk reduced to a fine powder was digested for twelve hours.

The solution obtained was filtered to separate the excess of carbonate of lime, and was perfectly limpid ; it reddened tincture of litmus slightly, but had no immediate effect on syrup of violets ; but after several hours' contact the solution became gradually green. The solution when treated with a little potash became immediately turbid, and deposited white flocculi of hydrated carbonate of lime, which soon diminished in volume by losing water, and were converted into a white powder, which partly adhered to the sides of the vessel ; the same effects were obtained with ammonia. When the solution was heated it became turbid, as the gas was expelled by the action of the heat, and carbonate of lime was precipitated.

It was found by experiment that the carbonic acid, which effected the solution of the carbonate of lime, was five times greater than that which existed in the carbonate of lime dissolved at 50°.

This result indicates that the calcareous salt to be dissolved in water by carbonic acid, is directly converted into sexticarbonate of lime, that is to say, into a compound of one equivalent of lime and six equivalents of carbonic acid, which would give  $\text{CaO}, 6\text{CO}_2$  as the formula of the soluble salt. M. Lassaigue thinks it probable that this is the state in which the combination exists in certain spring waters which contain it, unless it be regarded as a bicarbonate of lime, rendered soluble by four equivalents of free carbonic acid. The author is, however, disposed to consider the first hypothesis as more conformable to the theory of the saturation of metallic oxides with oxacids.

In operating at the temperatures of 50° F. and 32° F., a difference was observed in the solubility of neutral carbonate of lime in the same solution of carbonic acid. At the former temperature, a volume of the saturated water dissolved 0·00088 of its weight of

carbonate of lime, while an equal volume at 32° dissolved only 0·00070.

This decided difference proves then that the solubility of carbonate of lime in water impregnated with carbonic acid, varies with the temperature, at least within the limits of 32° and 50° F., which were those at which the experiments were made.—*Journ. de Ch. Méd.*, Février 1847.

**SOLUBILITY OF PHOSPHATE OF LIME IN WATER SATURATED  
WITH CARBONIC ACID. BY M. LASSAIGNE.**

The solution of carbonic acid employed in these experiments was saturated at the temperature of 50° F., and at a pressure of 29·9 inches; the phosphate employed was left in the solution in each case during twelve hours; and the quantity dissolved was ascertained by evaporating the same volume of each solution to dryness and weighing the residue.

1. *Pure phosphate of lime*, obtained by the double decomposition of a calcareous salt and alkaline phosphate; the solubility was 0·000750.

2. *Fresh bone*, a piece of nearly two inches long,  $\frac{16}{100}$  dths of an inch wide, and  $\frac{2}{100}$  dths thick; solubility 0·000166.

3. *Bone disinterred* after about twenty years' repose in a cemetery, the subsoil of which was sandy; solubility 0·000300.

M. Lassaigne remarks that these facts confirm an opinion advanced by M. Dumas, that bones which have suffered incipient decomposition in the earth, and which have consequently become less coherent, yield a larger quantity of their calcareous salts to water saturated with carbonic acid, than fresh bones containing all their organic matter.—*Journ de Ch. Méd.*, Janvier 1847.

**CRYSTALLIZED BILE.**

M. Verdeil effects the crystallization of bile by the following process:—Fresh ox-bile is to be dried by the water-bath, and the residue is to be treated with about twenty parts of absolute alcohol. By this the mucus remains insoluble, whilst the bilate of soda readily dissolves. The solution is to be filtered and treated with animal charcoal to decolorize it; æther is then to be cautiously added to it until it begins to turn milky, and is then to remain in a closed vessel. Crystallization occurs after some time: the bilate is then deposited on the sides of the vessel in the form of small acicular crystals in concentric groups. In twenty-four hours the liquor is to be poured off; and the crystals, after being washed with æther free from alcohol, are to be dried over sulphuric acid.

The crystals thus obtained always contain a little common salt; from this they are freed by placing them with absolute alcohol in a freezing mixture: this salt does not dissolve under these circumstances.

Pure bilate of soda dried at 212° F. yielded by analysis—

Carbon . . . . .	59·84	59·77	60·07
Hydrogen . . . . .	8·73	8·80	9·20
Azote . . . . .	4·11	4·33	
Sulphur . . . . .	8·78	3·89	
Oxygen . . . . .	16·45	16·32	
Soda . . . . .	7·09	6·89	
	<hr/> 100·00	<hr/> 100·00	

*Journ. de Pharm. et de Ch.*, Février 1847.

#### SINGULAR PROPERTY OF GUN-COTTON MIXTURE.

Dr. Draper has made the following observations:—Lecturers on chemistry have known for a long time, that one of the best methods of illustrating the properties of carbonic acid gas, is to evolve it from carbonate of ammonia by the action of monohydrated nitric acid. A dense white fume accompanies the gas, and marks all its movements in a striking manner.

Commercial nitric acid fails to produce the same effect. It sets the gas free in an invisible state. But if a mixture of commercial nitric acid and oil of vitriol be used, then the dense fume is at once produced. The explanation seems to be, that the oil of vitriol, by retaining water, allows some of the carbonate of ammonia to pass off with the carbonic acid in a dry state, and hence gives the gas a smoky aspect.

But it is singular, that though oil of vitriol will of course decompose carbonate of ammonia very rapidly, the gas which escapes is transparent.

Some months ago Dr. Ellet, of South Carolina College, published a process for preparing gun-cotton, which is unquestionably the greatest improvement yet made in the preparation of that explosive substance. His plan is to soak cotton in a mixture of oil of vitriol and saltpetre, and then wash it thoroughly from the adhering salt. Now if this mixture of oil of vitriol and saltpetre be made to act on carbonate of ammonia, like monohydrated nitric acid, or common nitric acid mixed with sulphuric, it evolves carbonic acid in the smoky state.

#### ON THE DEHYDRATION OF SULPHATE OF LIME UNDER VARIOUS CIRCUMSTANCES. BY M. M. E. MILLON.

The author observes that it is well-known that sulphate of lime which is slowly produced, retains its water of combination at a temperature at which precipitated sulphate loses it.

*Sulphate of lime.*—This salt contains two equivalents of water,  $\text{SO}_3, \text{HO} + \text{CaO}, \text{HO}$ ; according to Graham it suffers no loss at  $212^\circ \text{F.}$ , and becomes anhydrous at  $266^\circ$ . The general results obtained by M. Millon effect considerable changes in this statement.

Sulphate of lime, of whatever form, origin or mode of formation, always suffered a first loss, varying from 15 to 17 per cent. This quantity represents 1·5 equivalent of water: its elimination is per-

fectly decided, and cannot be confounded with the total loss, which varies from 20 to 22 per cent. and corresponds to two equivalents of water. The following are the results of experiments:—

*Artificial sulphate of lime*, prepared by precipitating cold solutions of sulphate of zinc and chloride of calcium; when dried over sulphuric acid the result was invariable. By exposure for six hours to a temperature of 176° to 185° F. it lost 17 per cent.; no further loss occurred by exposure for an additional hour to the same temperature; when rendered perfectly anhydrous the loss was 22 per cent.

*Artificial sulphate of lime*, precipitated from mixed boiling solutions of sulphate of zinc and chloride of calcium; dried as before, the loss at 176° to 185° F. remained invariably at 15·71 per cent.; when rendered anhydrous the loss was 20·87 per cent.

*Moistened plaster* lost at the above temperatures 16·22 per cent.; rendered anhydrous, the loss was 20·39 per cent.

*Precipitated sulphate of lime*, dissolved in hydrochloric acid, and crystallized from it.—By fifteen hours' exposure to 176° up to 185°, it lost no weight, and none occurred till the temperature reached 221° F., when it lost 15·38 per cent. The same heat was afterwards continued during four hours without further loss. Rendered anhydrous, the loss was 20·78 per cent.

*Arrow-headed gypsum from Montmartre*.—This suffered no loss at 176° to 185° F.; at 221° it amounted to 15·37 per cent., and the total loss was 20·78 per cent.

*Fibrous sulphate of lime*.—In fifteen hours lost no weight at 185° F.; at 221° the loss was 17·60 per cent.; it was afterwards exposed for twelve hours to the same temperature with scarcely any diminution; the total loss was 22·62 per cent.

*Alabaster from Volterra*.—Suffered no diminution of weight by exposure at 176° to 185° F.; at 230° F. the loss was 15·61 per cent.; several hours' continued application of the same heat occasioned no further diminution of weight; the total loss was 20·83 per cent.

*Snow-white gypsum (Gypse en neige)*.—Lost no weight at 185° F.; at 238° F. it was 15·57 per cent. No further diminution occurred by several hours' longer exposure to this heat. The total loss was 21·27 per cent.

*Fibrous gypsum from America*.—Lost no weight at 185° F.; at 230° F. lost 15·41 per cent.; total loss 20·59 per cent.

*Prismatic gypsum from Sicily*.—No diminution of weight at 185° F.; at 230° it was 15·58 per cent., and the total loss was 20·44 per cent.

It appears, therefore, that all native sulphates of lime retain their water at 185° F., and do not lose any below 221° to 236°. Artificial sulphate of lime crystallized in hydrochloric acid is similarly circumstanced. Artificial sulphate of lime, on the other hand, whether precipitated hot or cold, loses three-fourths of the water at 176° to 185°. Moistened plaster is also dehydrated at this lower temperature.

It will be observed that all the sulphates of lime undergo a fractional loss of water, and that the second state of hydration is expressed by  $(\text{SO}_3)_2, \text{HO} + 2\text{CaO}$ .



The last fourth of the water of hydration goes off very slowly, if the heat be not raised to nearly  $400^{\circ}$  or  $570^{\circ}$  F.; at  $257^{\circ}$  to  $393^{\circ}$  F. mere traces of water are separated in several hours. This resistance is unquestionably very favourable to the calcination of plaster intended for buildings; it prevents its complete dehydration, even at a somewhat higher temperature.

M. Millon has also observed that anhydrites which contain some hundredths of water, lose it in fractional quantities; one specimen lost 3.65 per cent. of water. The first loss of 2.89 per cent. occurred at  $221^{\circ}$  F.; the remainder, or 0.76 per cent. of water, required a higher temperature for its expulsion. On examining the anhydrate with a glass, opake portions were visible, which were small crystals of the trapezoidal variety of sulphate of lime, and were evidently formed by the anhydrate having absorbed moisture from the atmosphere.—*Ann. de Chim. et de Phys.*, Fevrier 1847.

#### COFFEE AS AN ANTIDOTE TO ACETATE OF MORPHIA.

An invalid took at one dose ten grains and nearly eight-tenths of acetate of morphia; thirty grains of emetic tartar were exhibited without occasioning vomiting; after a lapse of three hours, and not till then, and when the patient was perfectly comatose, a strong infusion of coffee with the grounds was given. In the course of twelve hours the invalid took about  $11\frac{1}{2}$  ounces of coffee; the coma ceased and he recovered.

This fact proves, among a hundred others, that even in the worst cases of poisoning, the medical man should never despair of the recovery of his patient. In the above-described case, in spite of a very strong dose of poison, and notwithstanding the absence of all assistance during three entire hours, and although it was impossible to evacuate any portion of the morphia, the patient recovered. If a similar accident should again occur, vomiting ought to be immediately attempted; if this fail the stomach-pump should be employed, and then concentrated coffee should be administered.—*Journ. de Pharm. et de Ch.*, Fevrier 1847.

#### ON THE FORMATION OF CYLINDRICAL MASSES OF SNOW IN ORKNEY.

*To Richard Taylor, Esq.*

Sandwick Manse by Stromness, Feb. 11, 1847.

MY DEAR SIR,—A curious phenomenon in this parish has astonished and perplexed all, and filled the superstitious with no small degree of consternation. Since the 6th inst. we have had hail- or snow-showers, on the 9th snow-drift, and yesterday a slight thaw with frost again in the evening.

During the night a heavy fall of snow took place which covered the plain to the depth of several inches. Upon this pure carpet there rest thousands of large masses of snow which contrast strangely with its smooth surface. A solitary mass may be seen in a field, but in general they occur in patches from one acre to a hundred in extent, while the clusters may be half a mile asunder, and not one mass to

be seen in the interval. These fields appear at a distance as if cart-loads of manure had been scattered over them and covered with snow; but on examination the masses are all found to be cylindrical, like hollow fluted rollers or ladies' swan-down muffs, of which the smaller ones remind me, from their lightness and purity, but most of them are of much greater dimensions and weight than any lady would choose to carry, the largest that I measured being  $3\frac{1}{2}$  feet long and 7 feet in circumference. The weight however is not so great as might be expected from the bulk; so loose is the texture, that one near this house which was brought in and weighed, was found to be only 64 lbs., though it measured 3 feet long and  $6\frac{1}{2}$  feet in circumference. The centre is not quite hollow, but in all there is a deep conical cavity at each end, and in many there is a small opening through which one can see, and by placing the head in this cavity in the bright sun, the concentric structure of the cylinder is quite apparent. So far as I am yet informed, they do not occur in any of the adjoining parishes, and they are limited to a space of about five miles long and one broad. They may occupy about 400 acres of this, and I counted 133 cylinders in one acre, but an average of a hundred would, at a rough computation, yield a total of about 40,000.

Now the question naturally arises, what is the origin of these bodies? I believe the first idea was that they had fallen from the clouds, and portended some direful calamity, and I hear an opinion that one had fallen on a corn-stack and been broken to pieces. It is a pity to bring down such lofty imaginations, and to deprive these cylinders of their high descent, but I prefer truth, when it can be discovered, to the loftiest theory. I must at once, then, set aside the idea that they fell from the atmosphere in their cylindrical form, as the first one I examined satisfied me that its symmetry and loose texture must have been immediately destroyed on coming in rude contact with this earth.

Farther observation has convinced me that they have been formed by the wind rolling up the snow, as boys form large snow-balls. This is proved by examination of the *bodies themselves*; their round form, concentric structure, and fluted surface all show this mode of formation. Again, it is proved by their *position*: none are found on the weather side of hills or steep eminences, where the wind could not drive them up, nor close to leeward of any wall or perpendicular bank from which they seem to have originated—the nearest well-formed small ones being 60 yards to leeward, and the large ones 100 yards. All nearer than this are fragments that have not gone on to completion, but broken down in their passage, and the different portions of the wreck form the nuclei of others. Many however are found blown to the windward side of walls or over the lee side of banks. Indeed, they are found almost exclusively on the leeward side of hills and eminences, where both the wind and declivity assisted in rolling them along, or on plains so exposed that the wind alone operated without the declivity.

I shall only add, that this mode of formation is proved by the *direction* in which these cylinders lie. The wind has been from the

north for four days, and I believe that it was so all night, when I am told it blew strong. Now they are all lying with their ends east and west, and their side to the wind; and farther, in some cases, their tracks are still visible in the snow for twenty or thirty yards on their north side, from which they have gathered up their concentric coats; and I understand these were still more evident at an early hour before a snow-shower obliterated them in many places.

I am, Sir, yours very truly,

CHARLES CLOUSTON.

# METEOROLOGICAL OBSERVATIONS FOR FEB. 1847.

*Chiswick.*—February 1. Overcast. 2. Slight snow. 3. Cloudy. 4. Cloudy: frosty. 5. Overcast. 6. Slight rain. 7. Overcast: snowing. 8. Sharp frost: snowing. 9. Clear and frosty: intense frost at night. 10. Severe frost: snowing. 11. Overcast: slight thaw: severe frost. 12. Intense frost: clear: severe frost. 13. Clear and frosty. 14. Rain. 15. Cloudy: boisterous. 16. Overcast: rain. 17. Fine. 18. Densely clouded: boisterous. 19. Boisterous: fine: clear and calm. 20, 21. Overcast: fine. 22. Hazy: overcast. 23. Hazy and cold. 24. Dry air: clear and frosty. 25. Slight haze. 26. Hazy. 27, 28. Cloudy and cold.

Mean temperature of the month .....	34°·79
Mean temperature of Feb. 1846 .....	45·32
Mean temperature of Feb. for the last twenty years .....	39·55
Average amount of rain in Feb. ....	1·61 inch.

*Boston.*—Feb. 1. Cloudy: snow p.m. 2. Cloudy: snow early a.m.: snow nearly all day. 3. Cloudy: snow p.m. 4. Fine. 5—7. Cloudy. 8—10. Fine. 11. Cloudy: snow on the ground. 12. Cloudy. 13. Fine. 14. Cloudy: rain early a.m. 15. Cloudy. 16. Cloudy: rain early a.m. 17. Fine: rain early a.m. 18. Fine: rain p.m. 19. Stormy. 20—23. Cloudy. 24. Fine. 25. Cloudy. 26. Cloudy: snow early a.m. 27. Fine: snow p.m. 28. Fine: melted snow.

*Sandwich Manse, Orkney.*—Feb. 1. Snow: clear. 2. Snow: clear: frost: clear. 3. Bright: thaw: drizzle. 4. Damp: drizzle. 5. Showers: lightning. 6. Hail-showers: aurora. 7. Hail-showers: snow-showers: aurora. 8. Snow-showers: aurora. 9. Snow-drift. 10. Sleet: thaw: snow: frost. 11. Deep snow\*: snow. 12. Deep snow: bright: showers: thaw. 13. Thaw: rain. 14. Sleet-showers. 15. Cloudy: showers. 16. Showers. 17. Showers: rain. 18. Showers. 19. Showers: clear. 20. Cloudy: rain. 21. Bright: showers. 22. Bright: clear: aurora: large halo. 23. Bright: clear. 24. Cloudy: clear: aurora. 25. Clear: frost: clear. 26. Bright: clear. 27. Clear: cloudy. 28. Cloudy.

*Applegarth Manse, Dumfries-shire.*—Feb. 1. Frost: snow lying half an inch deep. 2. Frost: slight shower: snow. 3. Frost. 4. Frost, but mild. 5. Thaw: slight rain. 6. Thaw: fair. 7. Frost: clear and fine. 8. Frost, hard. 9. Frost: threatening snow. 10. Frost: sprinkling snow. 11. Frost: fine: clear. 12. Frost: sprinkling snow. 13. Frost a.m.: rain p.m. 14. Thaw, soft and fine. 15. Frost, slight: thaw: rain. 16—18. Rain. 19. Rain and sleet: fierce wind. 20. Rain and wind. 21. Fair and fine: thrush singing. 22. Rain early a.m.: cleared. 23. Slight hoar-frost: clear. 24. Frost: clear and bright sun. 25. Hoar-frost. 26, 27. Frost. 28. Frost: clear and fine.

Mean temperature of the month .....	36°·25
Mean temperature of Feb. 1846 .....	43·4
Mean temperature of Feb. for twenty-five years .....	37·2
Mean rain in Feb. for twenty years .....	2 inches.

\* This morning the snow in many places is found rolled up in hollow fluted cylinders, the largest of which measures 3½ feet long and 7 feet in circumference; one which measures 3 feet by 6½ weighs 64 lbs.

*Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at CHISWICK, near London; by Mr. Veall, at BOSTON; by the Rev. W. Dunbar, at Applegarth Manse, DUMFRIES-SHIRE; and by the Rev. C. Clouston, at Sandwick Manse, ORKNEY.*

Days of Month.	Barometer.						Thermometer.						Wind.				Rain.			
	Chiswick.		Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Dumfries-shire.		Orkney, Sandwick.		Chiswick, 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.
	Max.	Min.	84 a.m.	9 a.m.	9 p.m.	84 p.m.	Max.	Min.	84 a.m.	Max.	Min.	84 p.m.								
1847. Feb.																				
1.	29.750	29.601	29.30	29.67	29.83	30.06	40	30	35	38½	30½	36	n.	calm	nne.	ne.	.....	.....	.....	.....
2.	29.813	29.785	29.58	29.90	29.90	30.10	36	31	35	39½	27	34	n.	calm	nne.	calm	.....	.....	.....	.....
3.	30.052	29.821	29.63	29.94	30.00	30.04	37	30	35	38	28	34	ne.	calm	nne.	calm	.....	.....	.....	.....
4.	30.151	30.119	29.82	30.03	30.08	30.02	39	22	33	39½	29	40	n.	calm	nne.	wnw.	.....	.....	.....	.....
5.	30.141	29.976	29.80	29.90	29.71	29.52	35	32	35.5	45½	27½	44	w.	calm	s-w.	wnw.	.....	.....	.....	.....
6.	29.683	29.565	29.26	29.40	29.44	29.53	51	30	41	45½	39	38½	sw.	w.	wnw.	n.	.....	.....	.....	.....
7.	29.574	29.448	29.20	29.40	29.43	29.45	35	17	32	35	28	27	sw.	calm	nnw.	n.	.....	.....	.....	.....
8.	29.653	29.326	29.30	29.44	29.25	29.25	30	05	25.5	34	19½	25½	sw.	w.	w.	nw.	.....	.....	.....	.....
9.	29.419	29.358	29.09	29.20	29.21	29.50	34	04	25.5	35½	21	30	w.	w.	nw.	n.	.....	.....	.....	.....
10.	29.566	29.460	29.25	29.44	29.52	29.69	36	22	25	...	26½	37	w.	w.	nnw.	n.	.....	.....	.....	.....
11.	29.862	29.673	29.43	29.60	29.70	29.78	40	06	31	41½	...	33	n.	calm	nw.	n.	.....	.....	.....	.....
12.	29.887	29.869	29.55	29.63	29.74	29.78	37	09	30	39	22	32	sw.	calm	nw.	w.	.....	.....	.....	.....
13.	30.026	29.882	29.73	29.80	29.50	29.16	40	16	24.5	39	19½	38	sw.	calm	nw,sw	s.	.....	.....	.....	.....
14.	29.660	29.604	29.24	29.30	29.37	29.15	45	39	40	48	36½	40	sw.	w.	w.	wsW.	.....	.....	.....	.....
15.	29.540	29.358	29.15	29.19	29.18	29.08	54	34	36	47½	35	40½	w.	s.	sse.	w.	.....	.....	.....	.....
16.	29.798	29.636	29.34	29.39	29.30	29.13	51	40	39	45½	35½	43½	sw.	w.	sse.	w.	.....	.....	.....	.....
17.	29.904	29.879	29.43	29.44	29.38	29.26	57	47	45	48	37½	44	sw.	w.	sse.	ssw.	.....	.....	.....	.....
18.	29.908	29.769	29.38	29.40	29.37	28.93	51	41	51	50	44	46	sw.	w.	ssw.	s.	.....	.....	.....	.....
19.	30.197	29.769	29.35	29.50	29.83	29.67	49	32	42	44	35	41	sw.	w.	nw.	wsW.	.....	.....	.....	.....
20.	30.235	30.213	29.75	29.92	29.80	29.62	52	41	42	46	36	42	sw.	w.	s.	ssw.	.....	.....	.....	.....
21.	30.279	30.235	29.79	29.93	30.08	30.00	51	41	47	57	43	45	sw.	calm	sw.	wsW.	.....	.....	.....	.....
22.	30.297	30.259	29.90	30.11	30.18	30.30	46	39	42.5	47	37½	44	sw.	calm	w.	se.	.....	.....	.....	.....
23.	30.255	30.209	29.91	30.13	30.10	30.21	42	27	40	48½	36	42	e.	se.	e.	se.	.....	.....	.....	.....
24.	30.197	30.098	29.95	30.10	30.08	30.23	41	27	35	44½	32½	40	e.	calm	e.	ese.	.....	.....	.....	.....
25.	30.136	30.108	29.86	30.05	30.05	30.21	40	21	32	41	25	39	ne.	calm	e.	e.	.....	.....	.....	.....
26.	30.124	30.063	29.78	30.08	30.10	30.28	39	25	36	40	30	40	ne.	ne.	e.	sse.	.....	.....	.....	.....
27.	30.100	30.076	29.85	30.12	30.12	30.34	35	28	33.5	40½	31½	35	e.	calm	e.	ne.	.....	.....	.....	.....
28.	30.229	30.197	29.98	30.20	30.28	30.45	38	32	34	43½	31	41½	e.	calm	e.	calm	.....	.....	.....	.....
Mean.	29.940	29.829	29.55	29.775	29.733	29.735	42.17	27.42	35.8	42.7	31.1	38.25					0.94	1.21	1.17	3.48



Fig. 3.

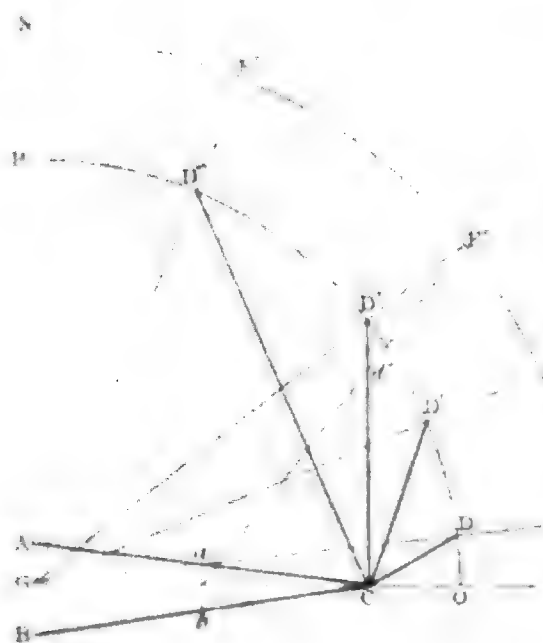


Fig. 2.

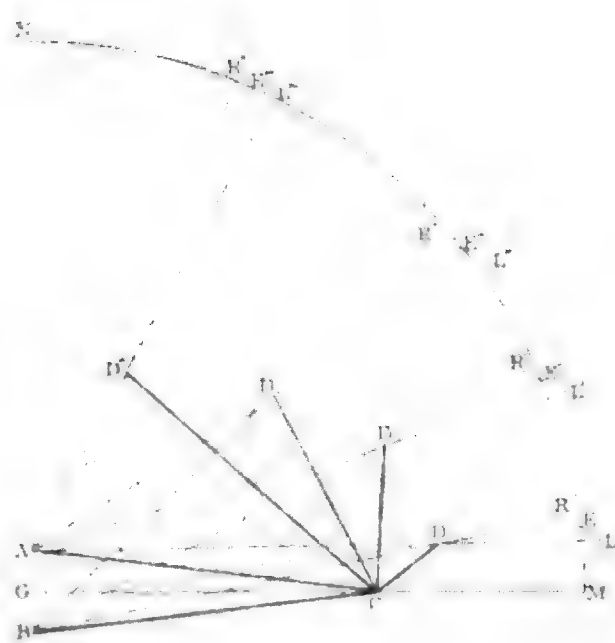
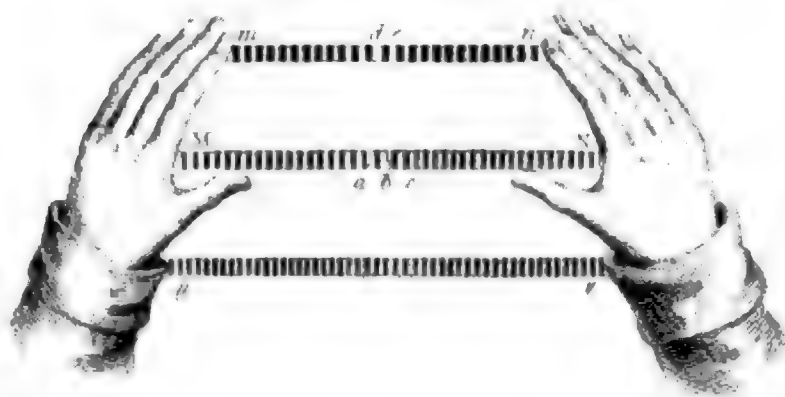


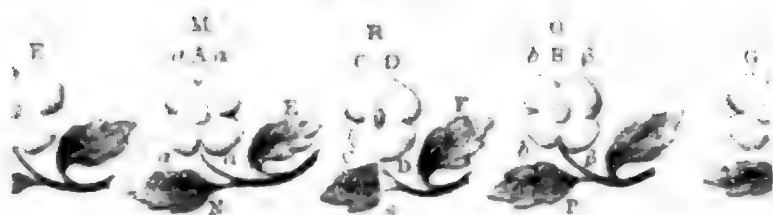
Fig. 5.



Fig. 4.



*Fig. 1.*



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AND  
JOURNAL OF SCIENCE.

[THIRD SERIES.]

MAY 1847.

XLVIII. *On the Knowledge of Distance given by Binocular Vision.* By SIR DAVID BREWSTER, K.H., D.C.L., F.R.S., and V.P.R.S. Edinb.\*

[With a Plate.]

IN analysing Mr. Wheatstone's beautiful discovery, that in binocular vision we see all objects of three dimensions by means of two dissimilar pictures on the retina, I trust I have proved that the dissimilarity of these two pictures is in no respect the cause of our vivid perception of such objects, but, on the contrary, an unavoidable accompaniment of binocular vision, which renders it less perfect than vision with one eye †. On the other hand, it is quite true that, in Mr. Wheatstone's experiment of producing the perception of objects of three dimensions by the apparent coalescence of two dissimilar representations of such objects *in plano*, the dissimilarity of the pictures is necessary in the exhibition of that beautiful phenomenon.

In performing with the eye alone, the various experiments detailed in a former paper ‡, I was very much struck with the fact, that the apparent solid figure, produced by the union of its dissimilar pictures, never took its right position in absolute space; that is, in place of appearing suspended between the eye and the plane upon which the dissimilar figures were drawn, the base of the solid seemed to rest on that plane, whether its apex was nearer the eye or more remote than its component plane figures.

With the view of finding the cause of this, I placed the component figures on a plate of glass suspended in the air, so as to have no vision of the surface on which they rested, and

\* Read at the Royal Society of Edinburgh, April 15, 1844. See their Transactions, vol. xv. p. 663.

† See this Journal for May and June 1844, vol. xxiv. pp. 356, 439.

‡ Ibid.

after uniting these figures by binocular vision, and concealing the two outstanding single figures, I obtained results which, though not entirely satisfactory, proved that there existed some disturbing cause which prevented the united image from placing itself in the *binocular centre*, or the intersection of the optical axes. This disturbing cause was simply the influence of other objects in the same field of view, whose distance was known to the observer.

In order to avoid all such influences, and to study the subject under a more general aspect, it occurred to me that these objects would be gained by using a numerous series of plane figures, such as those of flowers or geometrical patterns upon carpets or paper-hangings. These figures being always at equal distances from each other, and almost perfectly equal and similar, the coalescence of any pair of them, by directing the optic axes to a point between the paper-hangings and the eye, is accompanied with the coalescence of every other pair. When the observer, therefore, places himself in front of that side of a papered room in which there are neither doors nor windows, and conceals from his eye the floor, the roof, and the right and left-hand sides of the room, the whole of his retina will be covered with the images of the united plane figures, and there will be no interposing objects to prevent him from judging of the distance of the picture that may be presented to him.

Let the observer therefore now place himself *three* feet in front of the papered wall, and unite two of the figures, suppose two flowers, at the distance of *twelve* inches. The whole wall will now be presented to his view, consisting of flowers as before, but each flower will be composed of two flowers superimposed at the binocular centre, or the point of convergence of the optical axes. If we call  $D$  the distance of the eyes from the wall or *three* feet,  $C$  the distance between the eyes or two-and-half inches, and  $d$  the distance between the similar parts of the two flowers, we shall have  $x$  the distance of the binocular centre from the wall,  $x = \frac{Dd}{C+d} = 30$  inches nearly, and  $D-x=6$  inches, the distance of the binocular centre from the middle point between the two eyes.

Hence the whole papered wall, with all its flowers, in place of being seen, as in ordinary vision, at the distance of *three* feet, is now suspended in the air, at the distance of *six* inches from the observer. In maintaining this view of the wall, the eye will at first experience a disagreeable sensation; but after a few experiments the sensation will disappear, and the observer will contemplate the new picture with the same satisfaction

and absence of all strain as if he were looking directly at the wall itself; for there is a natural tendency in the eyes to unite two similar pictures, and to keep them united, provided they are not too distant.

When this picture is at first seized by the observer, he does not for a while decide upon its distance from himself. It sometimes appears to advance from the wall to its true position in the binocular centre, and when it has taken its place, it has a very extraordinary character:—the surface seems slightly convex towards the eye; it has a sort of silvery transparent aspect, and looks more beautiful than the real paper; it moves with the slightest motion of the head, either laterally or to or from the wall. If the observer, who is now *three* feet from the wall, retires from it, the suspended wall of flowers will follow him, moving further and further from the real wall, and also, but very slightly, further and further from the observer; that is, the distance of the observer from the real wall increases faster than the distance of the suspended wall from it, according to the law expressed by the preceding formula. The binocular centre, therefore, recedes from the eye as the observer retires, and the strain consequently diminishes.

In order to observe these phænomena in the most perfect manner, the paper should be pasted upon a large screen, previously unseen by the observer, unconnected with the roof or the floor, and placed in a large apartment. The deception will then be complete; and when the picture stands suspended before the observer, and within a few inches of himself, he may stretch out his hand and place it on the other side of the picture, and even hold a candle on the other side of it, so as to satisfy himself that in both cases the picture is between his hand and himself.

When we survey this picture with attention, several very curious phænomena present themselves. Some of the flowers, when narrowly examined, appear somewhat like real flowers. In some the stalk gradually retires from the general plane of the picture; in others it rises above it: one leaf will come further out than another, or the flower will appear thicker and more solid, deviating considerably from the plane representation of it seen by each eye separately. All this arises from slight and accidental irregularities in the two figures which are united, thus producing an approximation to three dimensions in the picture. If the distance, for example, of the ends of two stalks in two coalescing flowers is greater than the distance of corresponding points in other parts of the stalk, the end of the stalk will rise from the general surface of the figure, and *vice versa*. In like manner, if the distance between



two corresponding leaves is greater than the distance between other two corresponding leaves, then the two first, when united, will appear nearer the eye than the other two, and hence the appearance of a solid flower is partially given to the combination. These effects are better seen in old and imperfectly made paper-hangings, than in those which are more carefully executed.

In continuing our survey of the suspended image, another curious phænomenon presents itself: a part of one of the pieces of paper, and sometimes a whole stripe from the roof to the floor, will retire behind the general plane of the image, or rise above it; thus displaying, on a large scale, an imperfection in the workmanship which it would have required a very narrow inspection to discover. This defect arises from the paper-hanger having cut off too much of the white margin of one or more of the adjoining pieces, so that when the two halves of a flower are united, part of the middle of the flower is left out; and hence when this defective flower is united with the one on the right-hand of it, and the one on the left-hand united with the defective one, the united or corresponding portion, being at a less distance, will appear further from the eye than those parts of the suspended image composed of complete flowers. In like manner, if the two portions of the flowers are not brought together, but separated by a small space, the opposite effect will be produced. This will be understood from fig. 1, Plate VI., where M N, O P represent portions of two separate pieces of paper, each twenty-one inches wide. In this specimen, there are only two flowers in each piece, namely one white flower, A or B, and two halves. If the two halves C, D, are united as in the figure, it is obvious that the flower is incomplete, a part of the central circle of the corolla having been cut off from each half. If we now, by straining the eye, unite C D with B, and also with A, then at the same time E will be united with the second or left-hand image of A, and G with the second or right-hand image of B. But since a piece has been cut out of C D, the half  $\alpha\alpha$  of A is nearer the half D D than the other half  $a a$  is to the other half C C; and, in like manner, the half  $b b$  of B is nearer the half C C than the other half  $\beta\beta$  is to the other half D D. Hence, when the strained eyes unite  $\alpha\alpha$  to D D, the binocular centre is more remote than when  $a a$  is united to C, and the same is true of the other halves; consequently, the halves D D and  $b b$  must appear, as it were, sunk in the wall, or as further removed from the observer; and if the defective cutting exists along the line R S from the floor to the ceiling, the whole stripe of paper between R S and O P, from the

floor to the ceiling, will appear sunk in the papered wall. But if the defect is confined to a portion only of the flowers, then a rectangular space of the breadth  $RO$ , and of a height equal to the defective portion, will appear sunk in the paper. If every junction has the same defect as that at  $RS$ , then the whole will appear to consist of equal stripes, every alternate one being raised and the other depressed.

In the preceding example, there are only *two* flowers in a breadth, and their distance is  $10\frac{1}{2}$  inches, which is also the breadth of the sunk stripes. But if the flowers are three or four in number, and their distance  $\frac{21}{3}$ ,  $\frac{21}{4}$  inches, the sunk

stripes will vary according as we unite two flowers whose distances are in the one case 7 or 14 inches, and  $5\frac{1}{4}$  or  $10\frac{1}{2}$  or  $16\frac{3}{4}$  or 21 in the other. Calling  $B$  the breadth of the paper,  $n$  the number of flowers or figures in that breadth, and

$W$  the width of the sunk stripe, then we have  $W = \frac{B}{n}$  or  $\frac{2B}{n}$

or  $\frac{3B}{n}$  according as we unite the two nearest, or the first and second flower, the first and third, or the first and fourth. When  $W = B$ , the sunk stripes will cover the whole paper, and all the flowers will lie in the same plane.

These results afford an accurate method of examining and discovering defects in the workmanship of paper-hangers, carpet-makers, painters, and other artists whose profession it is to combine a series of similar patterns in order to form a uniform and ornamental surface. The smallest defect in the similarity and equality in the figures or lines which compose a pattern, and any difference in the distance of the single figures, is instantly detected; and what is remarkable, a small inequality of distance in a line perpendicular to the axis of vision, or in one dimension of space, is exhibited in a magnified form as a distance coincident with the axis of vision, and in an opposite dimension of space!

At the commencement of this class of experiments, it is difficult to realize, and very easy to dissolve, the singular binocular picture which we have been describing; but after the eyes have been drilled for a while to this species of exercise, the pictures become very persistent. Although the air-suspended image might be expected to disappear after closing one eye, and still more after having closed and re-opened both, yet I have found it in its original position in this latter case, and even after rubbing my eyes and shaking my head; and I have sometimes experienced a difficulty in ascertaining,

after these operations, whether it was the real or the air-suspended wall that was before me. On some occasions a singular effect was produced. When the flowers on the paper are distant six inches, we may either unite two six inches distant, or two twelve inches distant. In the latter case, when the eyes have been accustomed to survey the suspended picture, I have found that, after shutting and opening them, I neither saw the picture formed by the two flowers twelve inches distant, nor the papered wall itself, but a picture formed by uniting the flowers six inches distant! The binocular centre had shifted its place; and instead of advancing to the wall, as is generally the case, and giving us ordinary vision of it, it advanced exactly as much as to unite the nearest flowers, just as on a ratchet wheel the detent slips over one tooth at a time; or, to speak more correctly, the binocular centre advanced in order to relieve the eyes from their strain, and when the eyes were opened, it had just reached that point which corresponded with the union of the flowers six inches distant.

In the construction of complex geometrical diagrams consisting only of fine lines, and in which similar figures are repeated at equal distances, it is very difficult to attain minute accuracy. The points of the compasses sink to different depths in the paper, and the lines which join such points seldom pass through their centres. Hence arises a general inaccuracy which the eye cannot detect; but if we examine such diagrams by strained binocular vision, their imperfections will be instantly displayed. Some parts will rise higher than others above the general level, and the whole will appear like several cobwebs placed at the distance of a tenth or a twelfth of an inch behind each other\*.

In all the experiments made by Mr. Wheatstone by the stereoscope, and in those described in my former paper, the dissimilar figures are viewed in a direction perpendicular to the plane on which they are drawn. A series of very interesting results however are obtained by uniting the images of lines meeting at an angular point, when the eye is placed at different heights above the plane of the paper, and at different distances from the angular point.

Let  $AC$ ,  $BC$  be two lines meeting at  $C$ , the plane passing through them being the plane of the paper, and let them be viewed by the eyes at  $E'''$ ,  $E''$ ,  $E'$ ,  $E$  at different heights in a plane  $GMN$  perpendicular to the plane of the paper. Let  $R$  be the right eye and  $L$  the left eye, and when at  $E'''$  let them

\* This effect is finely seen in the diagram of the Homogeneous Curve, which forms plate 9. of Mr. Hay's work "On the Harmony of Form."

be strained so as to unite the points A, B. The united image of these points will be seen at the binocular centre  $D'''$ , and the united lines AC, BC will have the position  $D'''C$ . In like manner, when the eye descends to  $E'''$ ,  $E'$ , E, the united image  $D'''C$  will rise and diminish, taking the positions  $D''C$ ,  $D'C$ , DC till it disappears on the line CM, when the eyes reach M. If the eye deviates from the vertical plane GMN the united image will also deviate from it, and is always in a plane passing through the eye and the line GM.

If at any altitude EM the eye advances towards ACB in the line EG, the binocular centre D will also advance towards ACB in the line EG, and the image DC will rise and become shorter as its extremity D moves along DG, and after passing the perpendicular to GE it will increase in length. If the eye, on the other hand, recedes from ACB in the line GE, the binocular centre D will also recede, and the image DC will descend to the plane CM and increase in length.

The preceding diagram is, for the purpose of illustration, drawn in a sort of perspective, and therefore does not give the true positions and lengths of the united images. This defect however is remedied in fig. 3, where E,  $E'$ ,  $E''$ ,  $E'''$  is the middle point between the two eyes, the plane GMN being, as before, perpendicular to the plane passing through ACB. Now, as the distance of the eye from G is supposed to be the same, and as AB is invariable as well as the distance between the eyes, the distance of the binocular centres O, D,  $D'$ ,  $D''$ ,  $D'''$ , P, from G will also be invariable, and lie in a circle ODP whose centre is G, and whose radius is GO, the point O being determined by the formula  $GO = GD = \frac{GM \times AB}{AB + RL}$ . Hence,

in order to find the binocular centres D,  $D'$ ,  $D''$ ,  $D'''$ , &c. at any altitude E,  $E'$ , &c., we have only to join EG,  $E'G$ , &c. and the points of intersection D,  $D'$ , &c. will be the binocular centres, and the lines DC,  $D'C$ , &c. drawn to C, will be the real lengths and inclinations of the united images of the lines AC, BC.

When GO is greater than GC there is obviously some angle A, or  $E''GM$  at which  $D''C$  is perpendicular to GC.

This takes place when  $\cos A = \frac{GC}{GO}$ . When O coincides with

C, the images CD,  $CD'$ , &c. will have the same positions and magnitudes as the chords of the altitudes A of the eyes above the plane GC. In this case, the raised or united images will just reach the perpendicular when the eye is in the plane GCM, for since  $GC = GO$ ,  $\cos A = 1$ , and  $A = 0^\circ$ .

When the eye at any position,  $E''$  for example, sees the



points A and B united at D'', it sees also the whole lines AC, BC forming the image D''C. The binocular centre must therefore run rapidly along the line D''C; that is, the inclination of the optic axis must gradually diminish till the binocular centre reaches C, when all strain is removed. The vision of the image D''C, however, is carried on so rapidly, that the binocular centre returns to D'' without the eye being sensible of the removal and resumption of the strain which is required in maintaining a view of the united image D''C.

If we now suppose A B to diminish, the binocular centre will advance towards G, and the length and inclination of the united images D C, D' C, &c. will diminish also, and *vice versâ*. If the distance R L (fig. 2) between the eyes diminishes, the binocular centre will retire towards E, and the length and inclination of the images will increase. Hence persons with eyes more or less distant will see the united images in different places and of different sizes, though the quantities A and A B be invariable.

While the eyes at E'' are running along the lines A C, B C, let us suppose them to rest upon the points *a*, *b* equidistant from C. Join *a b*, and from the point *g*, where *a b* intersects G C, draw the line *g E''*, and find the point *d''* from the formula  $g d'' = \frac{g E'' \times ab}{ab + RL}$ . Hence the two points *a b* will be united at *d''*, and when the angle E'' G C is such that the line joining D and C is perpendicular to G C, the line joining *d''* C will also be perpendicular to G C, the loci of the points D'' *d''* *d'* *d* will be in that perpendicular, and the image D C, seen by successive movements of the binocular centre from D'' to C, will be a straight line.

In the preceding observations we have supposed that the binocular centre D'', &c. is between the eye and the lines A C, B C; but the points A, C, and all the other points of these lines, may be united by fixing the binocular centre beyond A B. Let the eyes, for example, be at E''; then if we unite A B when the eyes converge to a point, Δ'' (not seen in the figure), beyond G, we shall have  $G \Delta'' = \frac{GE \times AB}{RL - AB}$ ; and if we join the point Δ'' thus found and C, the line Δ' C will be the united image of A C and B C, the binocular centre ranging from Δ'' to C, in order to see it as one line. In like manner, we may find the position and length of the image Δ''' C, Δ' C, and Δ C corresponding to the position of the eyes at E''' E and E. Hence all the united images of A C, B C, viz. C Δ''', C Δ'', &c., will lie below the plane of A B C, and extend beyond a vertical line N B continued; and they will

grow larger and larger, and approximate in direction to C G as the eyes descend from E''' to M. When the eyes are near to M, and a little above the plane of A B C, the line, when not carefully observed, will have the appearance of coinciding with C G, but stretching a great way beyond G. This extreme case represents the celebrated experiment with the compasses, described by Dr. Smith, and referred to by Professor Wheatstone. He took a pair of compasses, which may be represented by A C B, A B being their points, A C B C their legs, and C their joint; and having placed his eyes about E above their plane, he made the following experiment:—"Having opened the points of a pair of compasses somewhat wider than the interval of your eyes, with your arm extended, hold the head or joint in the ball of your hand with the points outwards, and equidistant from your eyes, and somewhat higher than the joint. Then, *fixing your eyes upon any remote object* lying in the plane that bisects the interval of the points, you will first perceive two pair of compasses (each by being doubled with their inner legs crossing each other, not unlike the old shape of the letter W). But by compressing the legs with your hand, the two inner points will come nearer to each other; and when they unite (having stopped the compression), the two inner legs will also entirely coincide and bisect the angle under the outward ones, and will appear more vivid, thicker and larger, than they do, so as to reach from your hand to the remotest object in view even in the horizon itself, if the points be exactly coincident\*." Owing to his imperfect apprehension of the nature of this phænomenon, Dr. Smith has omitted to notice that the united legs of the compasses lie below the plane of A B C, and that they never can extend further than the binocular centre at which their points A and B are united.

There is another variation of these experiments which possesses some interest, in consequence of its extreme case having been made the basis of a new theory of visible direction by the late Dr. Wells†. Let us suppose the eyes of the observer to advance from E to N, and to descend along the opposite quadrant on the left-hand of N G, but not drawn in fig. 3, then the united image of A C, B C will gradually descend towards C G, and become larger and larger. When the eyes are a very little above the plane of A B C, and so far to the left-hand of A B that C A points nearly to the left eye and C B to the right eye, then we have the circumstances under which Dr. Wells made the following experiment:—"If we

\* Smith's Optics, vol. ii. p. 388, § 977.

† Essay on Single Vision, &c., p. 44.

hold two thin rules in such a manner that their sharp edges (A C, B C in fig. 3) shall be in the optic axes, one in each, or rather a little below them, *the two edges will be seen united in the common axis* (G C in fig. 3); and this apparent edge will seem of the same length with that of either of the real edges, when seen alone by the eye in the axis of which it is placed." This experiment, it will be seen, is the same with that of Dr. Smith, with this difference only, that the points of the compasses are directed towards the eyes. Like Dr. Smith, he has omitted to notice that the united image rises above G H, and he commits the opposite error of Dr. Smith, in making the length of the united image too short.

If in this form of the experiment we fix the binocular centre beyond C, then the united images of A C, B C descend below G C, and vary in their length and in their inclination to G C, according to the height of the eye above the plane of A B C, and its distance from A B.

It is a remarkable circumstance, that no examples have been recorded of false estimates of the distance of near objects, in consequence of the *accidental* binocular union of similar images. This has, no doubt, arisen from the rare occurrence of these circumstances or conditions, under which alone such illusions can be produced. In a room where the paper-hangings have a small pattern, or similar figures recurring at the distance of 1,  $1\frac{1}{2}$ , or 2 inches, a short-sighted person might very readily turn his eyes on the wall, when their axes converged to some point between him and the wall, which would unite one pair of the similar images; and in this case he would see the wall nearer him than the real wall, and moving with the motion of his head like something aërial. In like manner, a long-sighted person, with his optical axes converged to a point beyond the wall, might see an image of the wall more distant, and of an aërial character:—or a person who has taken too much wine, which often fixes the optical axes in opposition to the will, might, according to the nature of his sight, witness either of the illusions above-mentioned.

In the preceding observations, we have confined ourselves to the binocular union of figures upon an opaque ground. This limitation almost necessarily precluded us from observing the results when the binocular centre is beyond the plane where these figures are situated, because it is not easy to adjust the eyes to a distant object, unless we look through the surfaces containing the figures. Now this is by far the most interesting form of the experiment, and it has the advantage of putting scarcely any strain upon the eyes, not only because the binocular centre is more distant, but because we cannot, in this

way, unite figures whose distance exceeds  $2\frac{1}{2}$  inches, the interval between the eyes. Transparent patterns for these experiments may be cut out of stiff card-paper, or thin plates of metal, or they may be made of paper pasted upon large panes of glass. Experiments may be made with trellis-work, or with windows composed of small squares or lozenges; but the readiest pattern is the cane bottom of a chair, and I have performed my experiments by simply placing such a chair upon a high table, with its cane bottom in a vertical position. The distance of the centres of the eight-sided open figures in the direction of the width or depth of the chair, varies in different patterns from 0.54 to 0.76 of an inch. In order to simplify the calculations, we shall take the distance at 0.5, or half an inch. Then let

$D=12$  inches be the distance of the pattern from the eyes.

$d=0.5$  the distance of the centres of the similar figures.

$+\Delta$ =distance of suspended image from, and in front of, the pattern.

$-\Delta'$ =distance of suspended image from, and behind, the pattern.

$C=2.5$  the distance between the eyes.

Then we shall have

$$+\Delta = \frac{Dd}{C+d} \text{ and } -\Delta' = \frac{Dd}{C-d}. \text{ Hence}$$

$D-\Delta$ =distance of suspended image from the eye, and in front of the pattern, and

$D+\Delta'$ =its distance from the eye, and behind the pattern.

From these formulæ we have computed the following table, adapted to similar figures, whose centres are distant  $\frac{1}{2}$  an inch, 1,  $1\frac{1}{2}$ , 2 and  $2\frac{1}{2}$  inches; but in reference to the positive values of  $\Delta$  and  $D$ , we may consider them as feet, 0.5 being in that case = 6 inches.

D Inches.	$d=0.5$ .		$d=1.0$ .		$d=1.5$ .		$d=2.0$ .		$d=2.5$ .		$d=3.0$ .	
	$+\Delta$ .	$-\Delta$ .	$+\Delta$ .	$-\Delta$ .	$+\Delta$ .	$-\Delta$ .	$+\Delta$ .	$-\Delta$ .	$+\Delta$ .	$-\Delta$ .	$+\Delta$ .	$-\Delta$ .
6	1	1.5	1.72	4	2.25	9	2.66	24	2.94	144	3	Infin.
12	2	3	3.43	...	4.50	18	5.33	48	5.88	288	6	Infin.
24	4	6	6.86	16	9	36	10.66	96	11.76	576	12	Infin.
48	8	12	13.7	32	18	72	21.33	192	23.52	1152	24	Infin.

Taking the case where  $D$  is 12 inches, and uniting the two nearest openings where  $d$  is 0.5, let  $MN$  (fig. 4) be a section of the transparent pattern,  $L, R$  the left and right eyes,  $La, d$ ,  $Lb, e$  lines drawn through the centres of two of the open figures  $ab$ , and  $Rb, d$ ,  $Rc, e$  lines drawn through the centre of  $b$  and  $c$ ,



and meeting  $Lad$ ,  $Lbe$  at  $d$  and  $e$ ,  $d$  being the binocular centre when we look at it through  $a$  and  $b$ , and  $e$  the binocular centre when we look at it through  $b$  and  $c$ . Now, the right eye  $R$  sees the opening  $b$  at  $d$ , and the left eye  $L$  sees the opening  $a$  at  $d$ , hence the image at  $d$  consists of the similar images of  $a$  and  $b$  united. In like manner  $e$  consists of  $b$  and  $c$  united, and so on with all the rest, so that the observer at  $L R$  no longer sees the real pattern  $M N$ , but a suspended image of it at  $mn$ , three inches behind  $M N$ . If the observer now approaches  $M N$ , the image  $mn$  will approach to him, and if he recedes,  $mn$  will recede, being  $1\frac{1}{2}$  inch distant from  $M N$  when the observer is six inches from  $M N$ , and twelve inches from  $M N$  when he is forty-eight inches from  $M N$ , the image  $mn$  moving from  $M N$  with a velocity one-fourth of that with which the observer recedes. These two velocities are in the ratio of  $D$  to  $\frac{Dd}{C-d}$ .

Resuming the position in the figure where the observer is twelve inches distant from  $M N$ , let us consider the important results to which this experiment cannot fail to lead us. If the observer, with his eyes at  $L R$ , grasp the cane bottom or pattern at  $M N$ , as shown in fig. 4, his thumbs pressing upon  $M N$ , and his fingers trying to grasp  $mn$ , he will then *feel what he does not see, and see what he does not feel!* The real pattern is absolutely invisible at  $M N$ , and stands fixed at  $mn$ . The fingers may be passed through and through—now seen on this side of it, now in the middle of it, and now on the other side of it. If we next place the palms of each hand upon  $M N$ , feeling it all over, the result will be the same. No knowledge derived from touch—no measurement of real distances—no actual demonstration from previous or subsequent vision, that there is a real solid body at  $M N$ , and nothing at all at  $mn$ , will remove or shake the infallible conviction of the sense of sight that the object is at  $mn$ , and that  $d L$  or  $d R$  is its real distance from the observer. If the binocular centre be now drawn back to  $M N$ , the image *seen* will disappear, and the real object be seen at  $M N$ . If it be brought still further back to  $f$ , the object  $M N$  will again disappear, and will be seen at  $\mu \nu$ , as described in a former part of this paper.

In making these experiments, the observer cannot fail to be struck with the remarkable fact, that though the openings at  $M N$ ,  $mn$ , and  $\mu \nu$  have all the same angular magnitude, that is, subtend the same angle at the eye, viz.  $d L e$ ,  $d R e$ , yet those at  $mn$  appear larger than those at  $M N$ , and those at  $\mu \nu$  smaller. If we cause the image  $mn$  to recede and  $\mu \nu$  to approach, the figures in  $mn$  will invariably *increase* as they

*recede*, and those in  $\mu v$  will *diminish* as they *approach* the eye, and their *visual magnitudes*, as we shall call them, will depend on the respective distances at which the observer, whether right or wrong in his estimate, conceives them to be placed.

Now this is a universal fact, which the preceding experiments demonstrate; and though the estimate of magnitude thus formed is an erroneous one, yet it is one which neither reason nor experience is able to correct.

When we look at two equal lines, whose difference of distance is distinctly appreciable by the eye, either directly or by inference, but whose difference of angular magnitude is not appreciable, the most remote must necessarily appear the smallest. For the same reason, if the remoter of two lines is really smaller than the nearer, and therefore its angular magnitude also smaller from both these causes, yet, even in this case, if the eye does not perceive distinctly the difference, the smaller and more remote line will appear the larger\*.

The law of visual magnitude, which regulates this class of phænomena, may be thus expressed.

If we call  $A$  the angular magnitude of the *nearest* of two lines or magnitudes whose apparent distance is  $d$ ,  $a$  the angular magnitude of the remoter line, whose apparent distance is  $D$ , and  $V, v$  the visual magnitudes of the two lines, then

$$V : v = A \times D : a \times D.$$

Now let the two lines  $MO, NP$  be the two sides of a quadrilateral figure seen obliquely by an eye at  $E$ , then, if the apparent distances of  $MO, NP$  are such, that

$$A \times d > a \times D, \text{ then } V > v,$$

and the lines  $MN, OP$  will converge to a vanishing point beyond  $NP$ . But if

$$A \times d = a \times D, \text{ then } V = v,$$

and the line  $MN, OP$  will appear to be parallel. And if

$$A \times d < a \times D, \text{ then } V < v,$$

\* Malebranche seems to have been the first who introduced the *apparent* distance of objects as an element in our estimate of *apparent* magnitude. *De la Recherche de la Vérité*, tom. i. liv. i.; tom. iii. p. 354. See also Bouguer, *Mém. Acad. Par.* 1755, p. 99. These views however have been abandoned by several subsequent writers, and the *real* distance of objects has been substituted for their *apparent* distance. Varignon, *Mém. Acad. Par.* 1717, p. 88. M. Lehot, for example, says, "L'expression de la grandeur visuelle d'un corps est égale à la grandeur réelle, multipliée par le logarithme de la distance réelle divisée par cette distance."—*Nouvelle Théorie de la Vision*, 1er Mém. Suppl. p. 7, 8. Paris, 1823. This estimate of distance is incompatible with experiment and observation.

and the lines  $MN$ ,  $OP$  will converge to a vanishing point between  $MO$  and the observer.

These results may be considered as laying the foundation of a new art, to which we may give the name of Visual Perspective, in contradistinction to Geometrical Perspective. This art furnishes us with an immediate explanation of a great variety of optical illusions which have never yet been explained; and there is reason to believe that some of its principles were known to ancient architects, and even employed in modifying the nature and position of the lines and forms which enter into the construction of their finest edifices.

St. Leonard's College, St. Andrews,  
April 10, 1844.

### *Appendix.*

When I wrote the paragraph in page 314, I had no expectation of learning that any example of such an illusion had ever occurred. A friend, however, to whom I had occasion to show the experiments, and who is short-sighted, mentioned to me that he had been on two occasions greatly perplexed by the vision of these suspended images. Having taken too much wine, and being in a papered room, he saw the wall suspended near him in the air; and on another occasion, when kneeling and resting his arms on a cane-bottomed chair, he had fixed his eyes on the carpet, which accidentally united the two images of the open-work, and threw the suspended image of the chair-bottom to a distance, and beyond the plane on which his arms rested.

The following case, communicated to me by Professor Christison, is still more interesting. "Some years ago, when I resided in a house where several rooms are papered with rather formally recurring patterns, and one, in particular, with stars only, I used occasionally to be much plagued with the wall suddenly standing out upon me, and waving, as you describe, with the movements of the head. I was sensible that the cause was an error as to the point of union of the visual axes of the two eyes; but I remember it sometimes cost me a considerable effort to rectify the error; and I found that the best way was to increase still more the deviation in the first instance. As this accident occurred most frequently while I was recovering from a severe attack of fever, I thought my near-sighted eyes were threatened with some new mischief; and this opinion was justified in finding that, after removal to my present house—where, however, the papers have no very formal pattern—no such occurrence has ever taken place. The reason is now easily understood from your researches."

XLIX. *On the Universality of Magnetism.**By Dr. DE HALDAT\*.*

THE question of the universality of magnetism, on which M. de Haldat presented a memoir to the Academy of Sciences in 1841 †, having given rise to some objections, has been subjected to new researches, in which the author has confirmed the fact, that all bodies in small masses and of an elongated form are subject to the influence of the magnet when exposed to its action, both when they are made to oscillate between the opposite magnetic poles, and when they are suspended in the same situation by silk filaments whose torsion is employed to value the force which they have acquired. A desire to trace the proximate cause of these phænomena induced the author to ascertain whether these bodies possess, of themselves, the property of acquiring the magnetic state, or whether they owe this property to the presence of iron, as many philosophers have maintained.

If we consider magnetism in a general point of view, we shall find that, to attribute this property to iron alone, is to assign to that substance a special virtue, which later researches have shown to exist in two other metals, and which the attraction of the magnetic needle by the rotatory discs composed of various metals contradicts in the most direct manner; and we must acknowledge that, in short, to deny to the fluid, the magnetic agent, the generality of influence which we are forced to recognise in the other agents, or imponderable fluids, as heat, light and electricity, is to contravene the law of analogy, which is one of our surest guides in the study of nature. How, in fact, can we suppose that an agent which, like caloric, but with an infinitely greater velocity, penetrates all bodies, can be deprived of the property of exercising upon them an influence analogous to that exerted by the other agents with which it has such analogy? How can we admit, not only a great resemblance but even an identity recognised by a great number of philosophers between electricity and magnetism, and deny the existence in one of its agents of that which we attribute to the other? Finally, if all the agents, as is generally believed, are only different modifications of one universal agent, how can we deny to magnetism the generality of influence attributed to the fluid, the universal agent, of which it would be, so to speak, one of the factors?

These reasonings, conformable to the principles of natural philosophy, and sufficient perhaps for philosophers who restrict

\* From the *Annales de Chimie et de Physique*, Jan. 1847.

† *Comptes Rendus des Séances de l'Acad. des Sciences*, t. xii. p. 950.



themselves to general considerations, being unable alone to solve the objections proposed by the partisans of special magnetism, the author had recourse to experiments directed according to the following considerations. If the property of acquiring the magnetic state belongs only to one body, the partisans of this special virtue must prove,—1st, that this metal exists in all bodies presenting magnetic phænomena; 2nd, that all bodies contain it, and in sufficient quantity to satisfy the magnetic phænomena developed in the experiments of this class; 3rd, that, whatever be the state of the metal, pure or combined with different bodies, it always preserves its characteristic property. The answers to these questions are given in an article specially devoted to an investigation of iron, and an explanation of the processes adapted to detect it in the various substances in which it exists, either in a state of simple mixture or of combination. And as excessively weak quantities are able to communicate the magnetic power to the bodies which would contain it, it was indispensable to adopt a process capable of indicating the minutest quantities. The characteristic precipitate of its solutions by the cyanide of potassium furnishing a simple and efficacious means of detecting this metal, the author adopted it, after having convinced himself that in forming, with the precipitate diluted with water, a transparent column two centimetres (nearly 0·8 of an inch) in length, the experimenter can, on receiving a ray of white light, render perceptible at least a five-thousandth of the quantity of iron in the substances subjected to the magnetic experiment. At the same time all the agents adapted to render the test more sensitive were had recourse to, and each analysis checked by means of a test liquor composed of persulphate of iron, the amount of which being excessively small, proved that if this metal had existed in the composition submitted to examination, it would necessarily have been detected.

Sixty substances deprived of iron by the usual means, and found to be chemically pure, were thus prepared and subjected to magnetic action, after having received the elongated form which ensures the success of these experiments. Among these substances are the following, whose absolute purity must be theoretically admitted, and which has moreover been proved by experiment: distilled water in the state of ice, hyaline quartz of perfect transparency, the carbonate and muriate of ammonia obtained by the combination of their component principles in a state of vapour, the carbon of the smoke of oil-lamps or pure resins, sulphur washed with hydrochloric acid and repeatedly sublimed, camphor several times sublimed,

gum, bees wax, &c.; we may also add gold obtained from deposit, silver reduced from the chloride, and copper precipitated by galvanism. Each of these substances was placed upon a sheet of very thin paper, previously purified and suspended by a double filament of silk, and submitted to the influence of the two poles of a horse-shoe iron magnet carrying twenty-five kilogrammes. With respect to their behaviour under the influence of this double power, they are divided into two classes, conformably to the observation of Mr. Faraday, previously recognised by M. Becquerel\*. One class was placed in the direction of the magnetic current, the other transversely to this current. There is therefore no substance absolutely neutral with regard to the property of acquiring the magnetic power; but whatever be the direction which it takes, this can always be ascertained, and the acquired power determined, even measured, by means of the parts of the apparatus which measures the torsion.

Reverting to the question of the universality of magnetism and the speciality of the iron which it repels, the author has discussed the objections raised against the distinction of the two modes of magnetization, which, with the fact of the polarization of the luminous ray under the influence of the magnetic current, has strongly confirmed a theory already based on so many remarkable analogies and numerous experiments. He has refuted the fact of the variations in the direction of needles successively reduced to disproportionate lengths, which have been assigned to them from the consideration of the powerlessness of the most energetic magnets to act otherwise than upon the centre of the needles, and consequently to give them other than a transverse direction.

In rejecting the magnetic speciality of iron, the author has not denied the very remarkable pre-eminence of this metal with regard to the property of acquiring the magnetic power, which it always manifests with great energy, even when it only exists in an excessively small mass. An entire article is devoted to the valuation of the variations of which this arrangement is susceptible by the combination of oxygen, of several other substances, and above all by the passage of the metal to the saline state in which it is generally diamagnetic, even when the components of the salt are only united by a weak affinity. In this examination it has been found, that a quantity of iron inappreciable by the most delicate balance might be detected by the magnetic process, and even estimated approximately; that, however, this power diminishing with the quantity of the metal which puts it in play, it necessarily finds its limit in the

\* *Annales de Chimie et de Physique*, 2nd series, t. xxxvi. p. 327.  
*Phil. Mag. S. 3. Vol. 30. No. 202. May 1847.* Z

extreme attenuation of which it is susceptible; and that, whatever be the quantity of iron contained in a body, it is appreciable by the magnetic process as much as it is by chemical means.

From general considerations, and from very numerous facts stated in this memoir, and from numerous others for which there was no room, it results:—

1. That iron, although eminently magnetic, is not the only body which possesses this property.
2. That its tendency to acquire this state is dependent upon its purity, and varies with its combination.
3. Lastly, that the magnetic power acting upon all bodies, either giving them a direction parallel to the current or transverse to this current, the fluid, the magnetic agent, possesses, like all other imponderable fluids or agents, universality of influence in nature.

*L. On the Metaphosphates.*

By ROBERT MADDRELL, Esq.\*

HAVING had my attention drawn to a new method for preparing pure phosphoric acid from bone-ash, proposed by Professor Gregory, and a new and apparently anomalous phosphate of magnesia, accidentally discovered by him whilst trying to obtain the above-mentioned acid pure by heating at a temperature above 600° F.†, I was induced at the suggestion, and with the kind assistance of Professor von Liebig, to repeat the process, and also to prepare a quantity of the salt, and subject it to analysis. The general results I have found are as follows:—If phosphoric acid, freed from lime and sulphuric acid‡, be heated to a temperature above 600° F., it deposits a white substance, having all the general physical characters of the new salt, but in which I found on analysis 22·47 per cent. of magnesia, corresponding to the metaphosphate of magnesia ( $\text{MgO PO}_5$ ). After a considerable quantity of this substance had been deposited, I digested the mass in water, filtered, and evaporated the solution, and again heated the acid to upwards of 600° F., under the impression that it might still contain the new compound; by continuing this heat for half an hour, I again obtained a quantity of meta-

\* Communicated by the Chemical Society; having been read Dec. 7, 1846.

† See Transactions of the Royal Society of Edinburgh.

‡ See Geiger's *Handbuch der Pharmacie*.

phosphate of magnesia; and it was not until after a third repetition of this process that I obtained Professor Gregory's compound, in which I found on analysis 16·276 per cent. of magnesia, corresponding to the quantity found by that chemist in his salt; but on estimating the quantity of phosphoric acid present, I only obtained 75·25 per cent. of  $\text{PO}_5$ , leaving a loss of upwards of 8 per cent. Being convinced that another base must be present, from the enormous loss, I proceeded further with my analysis and found the presence of soda, which gave me 8·146 per cent. of soda.

This salt therefore is not an anomalous phosphate of magnesia, but a double metaphosphate of magnesia and soda, in the proportions are 3 equivs. of metaphosphate of magnesia and 1 equiv. of metaphosphate of soda,  $3(\text{MgO}, \text{PO}_5) + \text{NaO}, \text{PO}_5$ . It is nearly insoluble in hydrochloric acid and aqua regia, but soluble in concentrated sulphuric acid.

The analysis of the salt was made as follows:—I dissolved the salt in pure concentrated sulphuric acid, then diluted the solution with water, and precipitated the magnesia by an excess of ammonia; from the precipitate, after being heated to redness, was estimated the quantity of magnesia. To the filtered solution was added a solution of a salt of magnesia (with proper precautions) to precipitate the remainder of the phosphoric acid, and from this precipitate, as well as the former, was estimated the quantity of the  $\text{PO}_5$ . A second quantity was taken for the determination of the soda, which was dissolved in concentrated sulphuric acid, diluted with water, and precipitated by an excess of ammonia. The filtered solution, containing phosphoric acid, was then precipitated by acetate of lead, the excess of lead by sulphuretted hydrogen, and the soda in the solution evaporated and estimated as a sulphate, which gave me the following results:—

*Analysis, No. I.*

				Per cent.
·897	gram. substance	gave	·399 $2\text{MgO}, \text{PO}_5$	·146 $\text{MgO} = 16·276 \text{ MgO}$
·897	...	...	1·065	·675 $\text{PO}_5 = 75·250 \text{ PO}_5$
·712	...	...	·133 $\text{NaO SO}_3$	·058 $\text{NaO} = 8·146 \text{ NaO}$
				<hr/> 99·672

*No. II.*

				Per cent.
1·16	gram. substance	gave	·511 $2\text{MgO}, \text{PO}_5$	·187 $\text{MgO} = 16·12 \text{ MgO}$
1·16	...	...	1·386	·878 $\text{PO}_5 = 75·689 \text{ PO}_5$
1·23	...	...	·228 $\text{NaO}, \text{SO}_3$	·099 $\text{NaO} = 8·048 \text{ NaO}$
				<hr/> 99·857



Reckoned.	Atom.	Found.	
		I.	II.
16.368 =	3 MgO	16.276	16.12
75.376 =	4 PO <sub>5</sub>	75.250	75.689
8.256 =	NaO	8.146	8.048
<u>100.000</u>		<u>99.672</u>	<u>99.857</u>

Formula, 3(MgO, PO<sub>5</sub>) + NaO, PO<sub>5</sub>.

The different result obtained by Professor Gregory no doubt arose from the method upon which his examination of this salt was conducted, viz. by determining the amount of magnesia present, and considering the loss on the weight of substance used to represent phosphoric acid, and probably from not thinking at the time of the presence of a salt of soda in bones.

With regard to the phosphoric acid prepared as above, after the salt has ceased to form, I find that it is not yet quite pure, but contains traces of magnesia as well as soda, and therefore this process will always yield an acid containing more or less of these substances.

Under the impression that the phosphoric acid obtained by this process was pure, I attempted to prepare with it the metaphosphates of nickel and cobalt, by adding the sulphates of these bases to an excess of the acid, and heating as before to upwards of 600° F., with the idea that they might be deposited in a somewhat similar manner to the metaphosphate of magnesia. This I found on analysis not to be the case, as the salts which I obtained in this way were not the pure metaphosphates, but double salts in combination with soda, closely allied in their composition to Gregory's salt.

The nickel compound obtained in this way was deposited in the form of a greenish-yellow, anhydrous powder, insoluble in water and diluted acids, but soluble in concentrated sulphuric acid.

The analysis of this salt was made by fusing with carbonate of soda, and treating the fused mass with water to separate the metallic oxide from the alkaline phosphate. A second quantity was taken for the determination of soda, which I dissolved in concentrated sulphuric acid, then diluted with water, and precipitated the solution containing phosphoric acid by acetate of lead, and the excess of lead in the filtered solution by sulphuretted hydrogen. To the filtered solution was then added an excess of ammonia with sulphuret of ammonium to precipitate the nickel, and the solution containing soda was evaporated and determined as a sulphate.

	Found. Per cent.	Atom. Reckoned. Per cent.
·853 grm. substance gave ·254 NiO	= 29·789	6NiO = 29·805
2·24 ... .. ·231 NaO, SO <sub>3</sub> = ·091 NaO	= 4·062	NaO = 4·134
PO <sub>5</sub> estimated as loss	= 66·149	7PO <sub>5</sub> = 66·061
	<u>100·000</u>	<u>100·000</u>

Formula, 6(NiO, PO<sub>5</sub>) + NaO, PO<sub>5</sub>.

The cobalt salt is a beautiful rose-coloured, anhydrous compound, insoluble in water and dilute acids, but soluble in concentrated sulphuric acid.

The analysis was made by dissolving it in concentrated sulphuric acid, diluting with water, and precipitating by an excess of ammonia and sulphuret of ammonium. The sulphuret was then redissolved in nitric acid, and the solution diluted with water, filtered and precipitated by caustic potash. A second quantity was taken for the determination of the soda, and estimated as the preceding salt of nickel.

	Found. Per cent.	Atom. Reckoned. Per cent.
1·24 grm. substance gave ·403 Co <sub>3</sub> O <sub>4</sub>	= ·3755 CoO = 30·282	6NiO = 29·775
1·12 ... .. ·117 NaO, SO <sub>3</sub> = ·051 NaO	= 4·358	NaO = 4·136
PO <sub>5</sub> estimated as loss	= 65·460	7PO <sub>5</sub> = 66·089
	<u>100·000</u>	<u>100·000</u>

Formula, 6(CoO, PO<sub>5</sub>) + NaO, PO<sub>5</sub>.

Thinking I might still obtain the pure metaphosphates of nickel and cobalt in the way above mentioned, I prepared a quantity of pure phosphoric acid by the oxidation of phosphorus with nitric acid, and the process, when tried with the acid prepared in this way, perfectly succeeded.

By this method I obtained also the metaphosphates of MnO, Al<sub>2</sub> O<sub>3</sub>, Fe<sub>2</sub> O<sub>3</sub>, Cr<sub>2</sub> O<sub>3</sub>, CuO, BaO, SrO, CaO, KO, NaO, though not in every case using the sulphates of the bases, as I find that almost any other salt will do equally well.

*Metaphosphate of Oxide of Nickel.*—This salt I obtained on adding a solution of sulphate of nickel to an excess of pure phosphoric acid, and by evaporating the mixture in a platinum vessel until the sulphuric acid had been expelled, then by heating it to upwards of 600° F., when it deposited as a greenish-yellow, anhydrous compound. It is insoluble in water, hydrochloric acid and aqua regia, but soluble in concentrated sulphuric acid.

The determination of the nickel was made as the preceding salt of nickel, and the phosphoric acid estimated from the loss.

	Found. Per cent.	Atom. Reckoned. Per cent.
1·565 grm. substance gave ·537 NiO	= 34·401	NiO = 34·485
PO <sub>5</sub> as loss	= 65·599	PO <sub>5</sub> = 65·515
	<u>100·000</u>	<u>100·000</u>

Formula, NiO, PO<sub>5</sub>.

*Metaphosphate of Oxide of Cobalt* was obtained by the same process as the preceding salt of nickel. It is a beautiful rose-coloured, anhydrous compound, insoluble in water and diluted acids, but soluble in concentrated sulphuric acid.

The analysis was made by dissolving in concentrated sulphuric acid, diluting with water, and precipitating by an excess of ammonia and sulphuret of ammonium. The sulphuret was redissolved in nitric acid, the solution diluted with water, filtered, and precipitated by caustic potash. The  $\text{PO}_5$  was estimated from the loss.

	Found. Per cent.	Atom. Reckoned.
·983 grm. substance gave ·367 $\text{Co}_2\text{O}_3 = \cdot342 \text{ CoO}$	34·791	$\text{CoO} = 34·452$
$\text{PO}_5$ as loss =	65·209	$\text{PO}_5 = 65·548$
	<u>100·000</u>	<u>100·000</u>

Formula,  $\text{CoO}, \text{PO}_5$ .

*Metaphosphate of Protoxide of Manganese*.—This salt was obtained in a similar manner to the preceding salts of nickel and cobalt. It is a white, anhydrous compound, insoluble in water and dilute acids, but soluble in concentrated sulphuric acid.

The determination of the manganese was made as in the preceding salt of cobalt, and the  $\text{PO}_5$  estimated from the loss.

	Found. Per cent.	At. Reckoned. Per cent.
·857 grm. substance gave ·306 $\text{MnO}, \text{Mn}_2\text{O}_3 = \cdot2847 \text{ MnO}$	33·22	$\text{MnO} = 33·321$
$\text{PO}_5$ as loss =	66·78	$\text{PO}_5 = 66·679$
	<u>100·00</u>	<u>100·000</u>

Formula,  $\text{MnO}, \text{PO}_5$ .

*Metaphosphate of Alumina* was obtained by dissolving alumina in an excess of phosphoric acid, evaporating, and heating up to the same temperature as with the preceding salts. It is a white, anhydrous compound, insoluble in water and concentrated acids.

The analysis for the determination of alumina was made, according to Berzelius's method, by fusing the salt with  $1\frac{1}{2}$  part of silicic acid and 6 parts of carbonate of soda; and the  $\text{PO}_5$  estimated from the loss.

	Found. Per cent.	Atom. Reckoned.
1·253 grm. substance gave ·243 $\text{Al}_2\text{O}_3 =$	19·392	$\text{Al}_2\text{O}_3 = 19·352$
$\text{PO}_5$ as loss =	80·607	$3\text{PO}_5 = 80·648$
	<u>100·000</u>	<u>100·000</u>

Formula,  $\text{Al}_2\text{O}_3, 3\text{PO}_5$ .

*Metaphosphate of Peroxide of Iron*.—This salt was obtained on the addition of perchloride of iron to an excess of diluted phosphoric acid, and by evaporating and heating up to the same temperature as before; it is insoluble in water and diluted acid, but soluble in concentrated sulphuric acid.

The analysis was made by fusing the salt with carbonate of soda, then by treating the fused mass with water, to separate the peroxide of iron from the alkaline phosphate, and the phosphoric acid estimated from the loss.

	Found. Per cent.	Atom. Reckoned.
·850 grm. substance gave ·224 $\text{Fe}_2\text{O}_3$ =	26·353	$\text{Fe}_2\text{O}_3$ = 26·767
$\text{PO}_5$ as loss =	73·647	$3\text{PO}_5$ = 73·233
	<u>100·000</u>	<u>100·000</u>

Formula,  $\text{Fe}_2\text{O}_3, 3\text{PO}_5$ .

*Metaphosphate of Oxide of Chromium* was obtained by dissolving oxide of chromium in an excess of diluted phosphoric acid, evaporating, and heating up to the same temperature as before. It is a beautiful green, anhydrous compound, insoluble in water and concentrated acids.

The analysis was made by fusing the salt with caustic potash; the fused mass was then digested in water (to separate the oxide of chromium from the alkaline phosphate), filtered, and the filtered solution neutralized with hydrochloric acid. To the solution was added ammonia, chloride of ammonium, and a salt of magnesia, to precipitate the phosphoric acid, and from the precipitate, after being heated to redness, the quantity of the latter was estimated. The  $\text{Cr}_2\text{O}_3$  I was obliged to estimate as loss, as I found it exceedingly difficult to obtain a correct result, it being more or less oxidized by the action of the caustic potash.

	Found. Per cent.	Atom. Reckoned.
·67 grm. substance gave ·772 $\text{MgO}$ , $\text{PO}_5$ = ·488	$\text{PO}_5$ = 72·836	$3\text{PO}_5$ = 72·731
$\text{Cr}_2\text{O}_3$ as loss =	27·164	$\text{Cr}_2\text{O}_3$ = 27·269
	<u>100·000</u>	<u>100·000</u>

Formula,  $\text{Cr}_2\text{O}_3, 3\text{PO}_5$ .

*Metaphosphate of Oxide of Copper* was obtained on adding a solution of nitrate of copper to an excess of diluted phosphoric acid, and evaporating and heating up to the same temperature as before. It is a bluish-white, anhydrous compound, insoluble in water and diluted acids, but soluble in concentrated sulphuric acid. This salt I dissolved in concentrated sulphuric acid, diluted the solution with water, and precipitated by sulphuretted hydrogen; the sulphuret was then redissolved in hydrochloric acid, diluted with water, filtered, and precipitated by caustic potash, and the  $\text{PO}_5$  estimated from the loss.

	Found.	Atom. Reckoned.
1·518 grm. substance gave ·542 $\text{CuO}$ =	35·704	$\text{CuO}$ = 35·713
$\text{PO}_5$ as loss =	64·296	$\text{PO}_5$ = 64·287
	<u>100·000</u>	<u>100·000</u>

Formula,  $\text{CuO}, \text{PO}_5$ .

*Metaphosphate of Barytes* was obtained by decomposing



carbonate of barytes in diluted phosphoric acid, then evaporating and heating it up to 600° F.; it is insoluble in water, hydrochloric acid and aqua regia, but decomposed by concentrated sulphuric acid. This salt was heated with concentrated sulphuric acid, digested in water, and filtered, and the  $\text{PO}_5$  estimated from the loss.

	Found. Per cent.	Atom. Reckoned.
1.236 grm. substance gave .974 BaO, $\text{SO}_3 = .639$	BaO = 51.70	BaO = 51.746
	$\text{PO}_5$ as loss = 48.30	$\text{PO}_5 = 48.254$
	<u>100.00</u>	<u>100.000</u>

Formula,  $\text{BaO}, \text{PO}_5$ .

*Metaphosphate of Strontian*.—This compound was obtained by decomposing carbonate of strontian in diluted phosphoric acid, evaporating and heating as before; it is insoluble in water, hydrochloric acid and aqua regia, but decomposed by concentrated sulphuric acid.

This salt was heated with concentrated sulphuric acid, and then the mass digested in water, mixed with alcohol, and filtered. The  $\text{PO}_5$  was estimated from the loss.

	Found. Per cent.	Atom. Reckoned.
1.014 grm. substance gave .755 SrO, $\text{SO}_3 = .425$	SrO = 41.913	SrO = 42.043
	$\text{PO}_5$ as loss = 58.087	$\text{PO}_5 = 57.957$
	<u>100.000</u>	<u>100.000</u>

Formula,  $\text{SrO}, \text{PO}_5$ .

*Metaphosphate of Lime*.—This compound was obtained in a similar manner to the salts of barytes and strontian. It is a white, anhydrous powder, insoluble in water and diluted acids, but decomposed by concentrated sulphuric acid. The analysis of this salt was made the same as the salt of strontian.

	Found.	Atom. Reckoned.
.806 grm. substance gave .550 CaO, $\text{SO}_3 = .2284$	CaO = 28.337	CaO = 28.52
	$\text{PO}_5$ as loss = 71.663	$\text{PO}_5 = 71.48$
	<u>100.000</u>	<u>100.00</u>

Formula,  $\text{CaO}, \text{PO}_5$ .

*Metaphosphate of Magnesia* was obtained by dissolving carbonate of magnesia in an excess of diluted phosphoric acid, evaporating, and heating as before; it may also be obtained by preparing phosphoric acid from bone-ash, according to the above-mentioned process. It is a white, anhydrous compound, insoluble in water and diluted acids, but soluble in concentrated sulphuric acid. This compound was dissolved in pure concentrated sulphuric acid, diluted with water, and precipitated by an excess of ammonia. The filtered solution containing the remainder of the phosphoric acid was preci-

pitated (with the proper precautions) by a salt of magnesia and estimated accordingly.

gram.			Found.	Atom.	Reckoned.
1.887	substance gave	1.157	2MgO, PO <sub>5</sub> = .424	MgO = 22.47	MgO = 22.453
...	...	...	2.307	...	= 1.46 PO <sub>5</sub> = 77.424
					PO <sub>5</sub> = 77.547
					<u>99.894</u>
					<u>100.000</u>

Formula, MgO, PO<sub>5</sub>.

*Metaphosphate of Potash*.—This salt was obtained by igniting strongly together equal parts of chlorate of potash and phosphoric acid (of a syrupy consistence), and by digesting the mass in water to separate the soluble part from the insoluble. It is an anhydrous compound, nearly insoluble in water, but soluble in diluted as well as concentrated acids. The acetic solution gives a white precipitate with nitrate of silver. This insoluble compound is decomposed on being treated with solutions of nitrate of silver, acetate of lead, and chloride of barium, into the metaphosphates of these metallic oxides.

This compound was dissolved in acetic acid, diluted with water, and the PO<sub>5</sub> in the solution precipitated by acetate of lead, and the excess of lead in the filtered solution by sulphuretted hydrogen. The solution containing potash was then evaporated, and the potash determined as nitrate.

			Found.	Atom.	Reckoned.
1.468	gram. substance gave	1.25	KO, NO <sub>3</sub> = .582	KO = 39.645	KO = 39.8
			PO <sub>5</sub> estimated as loss =	60.355	PO <sub>5</sub> = 60.2
				<u>100.000</u>	<u>100.0</u>

Formula, KO, PO<sub>5</sub>.

*Metaphosphate of Soda* was obtained by igniting strongly together equal parts of nitrate of potash and phosphoric acid (of a syrupy consistence), and by digesting in water to separate the soluble from the insoluble part. It is an anhydrous compound, nearly insoluble in water, but soluble in diluted and concentrated acids. The solution in acetic acid gives a white precipitate with nitrate of silver; it is also decomposed on being treated with the same salts as the salt of potash. This salt was dissolved in acetic acid, diluted with water, and the PO<sub>5</sub> precipitated in the solution (with proper precautions) by a salt of magnesia.

			Found.	Atom.	Reckoned.
.943	gram. substance gave	1.042	MgO, PO <sub>5</sub> = .659	PO <sub>5</sub> = 69.883	PO <sub>5</sub> = 69.537
			NaO estimated as loss =	30.117	NaO = 30.463
				<u>100.000</u>	<u>100.000</u>

Formula, NaO, PO<sub>5</sub>.

LI. *On the Amount of Sulphur and Phosphorus in various Agricultural Crops.* By HENRY CLIFTON SORBY, Esq.\*

HAVING thought it highly probable that in the ordinary method of estimating the amount of sulphur and phosphorus in plants, viz. by burning them and analysing their ashes, there might be in many cases a loss from portions being rendered volatile by contact with combustible matter at a high temperature, I was induced to commence a series of analyses, employing a method against which there was no such objection, and, as will be seen, the quantities really present in various plants are much greater than has been hitherto anticipated. Having obtained a proper specimen, one portion of known weight was dried at  $212^{\circ}$  F. and weighed, and another in precisely the same condition was employed for the analysis, the amount which I generally used varying with the dryness of the substance from 200 grs. to 500 grs., which was cut into pieces if necessary, and introduced into a flask, and carefully heated with pure nitric acid, a little water being added if the substance was a dry one. No large quantity of the acid is required, and a gentle heat is kept up until the whole is digested down into a yellowish pulpy mass; water is then added, the whole boiled, and when cold filtered and washed, there being invariably a quantity of white fibrous substance left undissolved. To the yellowish solution thus obtained nitrate of baryta was next added, and though at first perhaps no precipitate was formed, sulphate of baryta was gradually deposited, and after standing for a day or so, the precipitate was collected on a filter, washed, dried, ignited and weighed, from which the amount of sulphur was calculated. It is well to ascertain whether it is pure sulphate of baryta, by adding to it after weighing a little hydrochloric acid, since sometimes there was present a little carbonate, which of course produces effervescence, and when that is the case, which seldom occurred to any but the most trifling extent, the amount is easily ascertained and a proper deduction made, to obtain the true weight of the sulphate alone.

To the solution filtered as above, I next added acetate of lead in moderate quantity, and then caustic ammonia in slight excess, filtered and washed the precipitate. When dry this precipitate was carefully calcined in a porcelain crucible over a lamp, at as low a temperature as convenient, stirring with a glass rod until the whole was converted into a mixture of metallic lead, oxide and phosphate of lead. These were then dissolved in nitric acid, and ammonia added until a consider-

\* Communicated by the Chemical Society; having been read Dec. 7, 1846.

able precipitate of subnitrate was formed, when on adding acetic acid the whole of the phosphate alone was left insoluble, which, after standing awhile, was collected on a filter, washed, dried, ignited and weighed, and the amount of phosphorus obtained by calculation. That this method is correct, and that the phosphate which is thus obtained is  $\text{Pb}_3\text{P O}_8$ , I have proved by experiment.

It must be borne in mind that the amounts given below are those of sulphur and phosphorus, I having thought it right to give them as such, and not as sulphuric and phosphoric acids, since most certainly they do not exist invariably as such or as their salts in plants, and I therefore see no reason why they should not be given as elements, like the amounts of carbon, nitrogen, oxygen and hydrogen as usually given in such analyses.

The plants analysed, with only three exceptions, viz. the rye, mangel-wurzel, and hops, were obtained from the fields in which they were growing by myself, and hence I can vouch for their freedom from impurities, and have no doubt the others were as much so as possible.

Table of the Amounts of Sulphur and Phosphorus in various Agricultural Crops, calculated in 100 parts, dried at  $212^\circ\text{ F.}$  until the weight remained constant.

Name of Plant, &c.	Sulphur.	Phosphorus.
Four species of grass ( <i>Poa palustris</i> and <i>trivialis</i> , } <i>Festuca pratensis</i> , <i>Cynosurus cristatus</i> ) .....	·165	·164
Rye grass ( <i>Lolium perenne</i> ) .....	·310	·183
Italian rye grass .....	·329	·145
Red clover ( <i>Trifolium pratense</i> ) .....	·107	·149
Ditto .....	·087	·131
Very fine white clover ( <i>T. repens</i> ) .....	·099	·183
Ordinary ditto .....	·151	·139
Very fine trefoil ( <i>Medicago lupulina</i> ) .....	·136	·052
Lucern ( <i>M. sativa</i> ) .....	·274	·046
Ditto .....	·452	·215
Ditto .....	·293	·353
Lints ( <i>Vicia sativa</i> ) .....	·178	·183
Kidney potatoes ( <i>Solanum tuberosum</i> ) .....	·094	·213
Ditto, tops .....	·389	·357
Ditto, fruit .....	·071	·597
American native potatoes .....	·082	·212
Ditto, tops .....	·206	·483
Carrot ( <i>Daucus carota</i> ) .....	·092	·255
Ditto, tops .....	·745	·382
Mangel-wurzel ( <i>Beta altissima</i> ) .....	·058	·190
Ditto, tops .....	·502	·293
Early yellow-top turnip ( <i>Brassica rapa</i> ) .....	·351	·352
Ditto .....	·421	·346
Ditto, tops .....	·758	·360
Ditto .....	·615	·380



Name of Plant, &c.	Sulphur.	Phosphorus.
Swede turnip ( <i>Brassica oleracea</i> ) .....	·435	·172
Ditto, tops .....	·458	·250
Rape ( <i>B. oleifera</i> ) .....	·448	·233
Drum-head cabbage ( <i>B. campestris</i> ) .....	·431	·267
Wheat plant entire ( <i>Triticum vulgare</i> ) when just gone out of flower .....	·151	·248
Ditto .....	·170	·140
Ear of wheat when the grain was formed, but milky...	·075	·271
Straw from the above .....	·240	·132
Ear of wheat when ripe .....	·090	·336
Straw from the above .....	·213	·043
Red wheat .....	·070	·363
Ditto, straw .....	·293	·079
White wheat from the same field as the above red ....	·054	·366
Ditto, straw .....	·207	·112
Wheat (another sample) .....	·051	·410
Ditto, chaff .....	·091	·252
Very fine barley ( <i>Hordeum distichum</i> ) .....	·066	·498
Ditto, straw .....	·390	·087
Rather poor barley .....	·040	·367
Ditto, straw .....	·191	·065
Barley plant when in flower .....	·313	·236
Oat plant ( <i>Avena sativa</i> ) when just coming into flower	·226	·194
Ditto in flower .....	·189	·189
Green oats .....	·125	·317
Straw of ditto .....	·329	·128
Black tartarian oats .....	·080	·381
Straw of ditto .....	·271	·110
White oats .....	·090	·334
Straw of ditto .....	·401	·153
White oats (another sample) .....	·074	·382
Straw of ditto .....	·195	·057
Rye ears ( <i>Secale cereale</i> ) when young .....	·073	·076
Straw of ditto .....	·099	·153
Rye .....	·051	·160
Bean plant ( <i>Vicia faba</i> ) in flower .....	·045	·258
Beans .....	·071	·600
Ditto, straw .....	·148	·233
Peas ( <i>Pisum sativum</i> ) .....	·158	·206
Ditto, straw .....	·214	·076
Fine hops ( <i>Humulus lupulus</i> ) .....	·127	·574
Bind of ditto .....	·091	·138

From the above analyses it will be seen how very much the amounts vary in some cases, whereas in others they are pretty constant. This variation may probably be occasioned by that of the amount of phosphates and sulphates in the soils. I have observed as a general rule, that the finer the quality of the specimen the larger the proportions of sulphur and phosphorus present; though in the case of lucern, in which the phosphorus varies as much as in any crop of which I have analysed a sufficient number of specimens to be able to give an opinion, all the samples were growing luxuriantly. It may be presumed that when the amounts vary so much,

without apparently affecting the growth of the plant, the particular constituent so varying is not of such importance for the plant as when more constant.

It will be observed that in many plants different parts require different constituents, which indeed is well known; as for instance in the case of wheat it is probable that an abundance of sulphates in the soil would produce fine straw, and of phosphates, fine grain, and the converse, other necessary constituents being present.

It may also, perhaps, be inferred from these analyses that when grain is growing and ripening the amount of phosphorus increases in greater proportion than the total weight, and diminishes in the straw, the grain abstracting that constituent from it. Similar inferences may perhaps be derived from the above, many of which are given for such purposes.

The table given below is a practical application of these analyses, being intended to show the quantities of gypsum and bone-phosphate of lime represented by the sulphur and phosphorus which are removed annually from an acre of land by the respective crops; being also that which should be added to already fertile land to maintain it in that condition. No allowance for what would be removed by drainage is made, as that is too uncertain in its quantity, and cannot in many cases be at all material, or else uncultivated natural lands would long ago have become sterile. The quantities so removed are no doubt compensated for by the disintegration of the substrata, and the superficial removal of exhausted soil by washing.

In such a calculation as this, however, it is affectation to pretend to great exactness, the elements which unavoidably enter into it being so variable. The quantities on which I have calculated are the greatest which are shown in the analyses, unless where the variations are given, when I have taken the extremes, and the weight of crop per acre is what I learn to be a medium between the very best and average crops; but I am most willing to confess that I am not myself practically acquainted with this part of my subject, and hence cannot answer for their absolute correctness, but have obtained them from the best authorities accessible to me. The necessary alterations, however, are easily made by any one for himself, from the data I have given, but I think they cannot be far wrong. The quantities are pounds of gypsum and bone-phosphate of lime removed from one acre.

Name of Crop, &c.	Weight per acre of crop in tons.	Gypsum.	Bone-phosphate.
Hay .....	2	34 to 67	26 to 32
Clover hay (one crop) .....	2½	29	35
Clover, green (whole crop) .....	15	45	53
Lucern .....	12	93 to 152	14 to 103
Lints .....	8	34	31
Wheat .....	210 st.	7 to 10	44 to 50
Ditto, straw .....	1½	38 to 55	7 to 18
Barley .....	190 st.	8	55
Ditto, straw .....	1½	62	12
Oats .....	220 st.	13	48
Ditto, straw .....	1½	36 to 75	8 to 25
Beans .....	200 st.	10	69
Ditto, straw .....	1½	20	27
Peas .....	200 st.	21	24
Ditto, straw .....	1½	33	12
Rye .....	180 st.	6	17
Ditto, straw .....	2	21	28
Potatoes .....	10	23	46
Ditto, tops .....	10	74	80
Carrots .....	20	26	64
Ditto, tops .....	3	46	21
Mangel-wurzel .....	30	27	76
Ditto, tops .....	4	24	12
Swede turnips .....	20	104	36
Ditto, tops .....	4	28	13
Early yellow-top ditto ...	18	81	59
Ditto, tops .....	5	48	21
Rape .....	20	140	64
Cabbage .....	20	88	47

LII. *Observations on the oxidizing power of Oxygen when disengaged by means of Voltaic Electricity.* By H. KOLBE, Esq., Ph.D.\*

IN describing in a former paper† the properties of sesquichloro-carbohyposulphuric acid, I stated that this body resists the action of the most powerful oxidizing agents, such as chromic acid, nitric acid, and even nitro-hydrochloric acid. I likewise stated the change it undergoes by the action of a voltaic current, when an easily oxidizable metal is used for the positive pole. By this means another copulated acid is produced, in which one or more equivalents of chlorine are replaced by corresponding proportions of hydrogen.

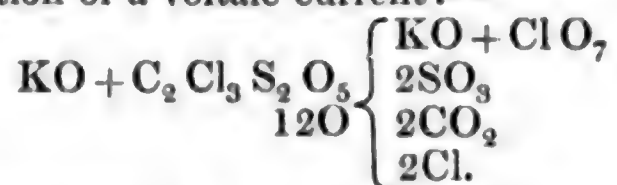
I have since observed that this acid is completely decomposed on employing two platina plates as electrodes; by which arrangement oxygen is disengaged at the positive pole. The following details, which contain a more perfect account of this observation, I hope will be acceptable to the Society.

\* Communicated by the Chemical Society; having been read Dec. 7, 1846.

† Liebig's *Annalen der Chemie*, liv. pp. 156, 160.

On decomposing a concentrated solution of sesquichloro-carbohyposulphate of potash by a strong voltaic current (obtained by four elements of Bunsen's battery), at the commencement no hydrogen can be observed at the negative pole in consequence of the formation of the above-mentioned copulated acids, whilst chlorine, carbonic acid, and afterwards oxygen, are evolved at the positive pole. At the moment decomposition commences a distinct acid reaction of the solution is perceptible, owing to the formation of free hydrochloric and sulphuric acids; in a later stage of the process, these acids increasing in quantity, hydrogen appears at the negative pole, until finally, after all the hydrochloric acid has been decomposed, and the disengagement of chlorine has ceased, small octahedral crystals of perchlorate of potash are deposited from the solution, which now contains a large amount of free sulphuric acid and bisulphate of potash. The formation however of perchloric acid in this process is always preceded by that of chloric acid; for on evaporating the solution in an earlier stage of the decomposition, beside the before-mentioned octahedral crystals, the well-known rhombic plates of chlorate of potash are obtained.

The following formula represents the most probable decomposition sesquichloro-carbohyposulphuric acid undergoes by the action of a voltaic current:—



The production of perchloric acid in an acid solution is certainly a fact worthy of attention, for according to all the observations hitherto made, combination between chlorine and oxygen could only be effected in presence of a free alkaline basis ready to unite with the newly-formed acid; but ascertained as it is by a great number of careful experiments, it is another proof of the unparalleled negative power of oxygen when evolved by means of voltaic action.

These observations induced me to make the following experiments.

A neutral solution of chloride of potassium was decomposed in the same manner by a strong voltaic current; it immediately became alkaline, whilst hydrogen and chlorine were disengaged. The formation of chlorate of potash, which took place under these circumstances, can evidently be considered only as the result of the ordinary chemical action of chlorine upon caustic potash. On mixing however a solution of chloride of potassium with sufficient sulphuric acid to set free all the



muriatic acid, and passing the voltaic current through it as before, chlorate of potash was nevertheless formed, which was subsequently converted into perchlorate of potash.

The transformation of oxy-acids of chlorine, such as liquid hypochloric acid ( $\text{ClO}_4$ ), or of a solution of chlorate of potash, into perchloric acid by means of voltaic action, has been mentioned by Berzelius. I have ascertained, moreover, that when a voltaic current is passed through hypochloric acid, especially when previously mixed with some sulphuric acid, free chloric and perchloric acids are formed, after the disengagement of a considerable quantity of chlorine.

A concentrated solution of chloride of ammonium evolves hydrogen at the negative pole but neither oxygen nor chlorine at the positive pole. But the surface of the platina plate representing the latter pole is covered with small yellowish oily drops of chloride of nitrogen, which as soon as the two poles are brought into contact decomposes with a more or less violent explosion, chlorine and nitrogen being evolved. This experiment illustrates at once the formation of this compound and its highly explosive character.

Cyanide of potassium if dissolved in water is easily oxidized by a voltaic current, and converted into cyanate of potash, but I did not succeed in obtaining a percyanate of potash. I was not more successful in endeavouring to form a fluorate of potash from the fluoride of potassium by the same means.

I have not prosecuted my experiments further upon inorganic substances, having from some observations on the behaviour of organic compounds under the action of the voltaic current obtained results of so much greater interest, as to induce me to give them my exclusive attention; and at a future time, when I have completed the investigation, I shall beg leave to communicate it to the Chemical Society.

LIII. *On the Discovery of Silurian Rocks in Cornwall.* By Sir RODERICK IMPEY MURCHISON, G.C. St. S., F.R.S., V.P.G.S. & R. Geogr. S., Honorary Member of the Royal Geological Society of Cornwall, Mem. Imp. Acad. Sc. St. Petersburg, Corr. Roy. Inst. France, &c. In a Letter addressed to Sir C. Lemon, Bart., M.P., President of the Royal Geological Society of Cornwall.

MY DEAR SIR CHARLES,

IN compliance with the promise I made when attending your last anniversary meeting at Penzance, I now give you a more decided opinion than I was then enabled to do, respecting the

age of the lowest and oldest of the sedimentary rocks of Cornwall.

Not having seen the fossils collected by Mr. Peach on the south coast of Cornwall, I then found it difficult to come to any other conclusion than that at which Professor Sedgwick and myself had long ago arrived; viz. that with the exception of the presence, in the north-eastern extremity of the county, of a portion of the culmiferous (carboniferous) trough of central Devon, the remaining and underlying strata of Cornwall were of the age of the Devonian or Old Red system. The few Cornish fossils which were then shown to me in your museum, were unquestionably similar to those with which I was formerly familiar in Devonshire and North Cornwall, as well as with those of the Rhenish provinces and the Eifel, which Professor Sedgwick and myself had shown to occupy a like geological position. They were, in fact, forms of the same type as those which, at the suggestion of Mr. Lonsdale and with the assistance of Mr. James Sowerby on one occasion\*, and with the help of MM. de Verneuil and d'Archiac on another†, we had published as characteristic of a group of intermediate characters, pertaining to strata lying *beneath* the carboniferous rocks and *above* the Silurian system. In a word, they were identical with some of the numerous fossils of Devon and North Cornwall, published in the work of Professor Phillips‡; who, in pointing out in certain tracts the connexion of this group with the carboniferous fossils, which he had so well described, and in others with the Silurian forms I had published, had also concluded that the great mass of fossiliferous strata which rise up from beneath the culm measures of central Devon were of the same intermediate characters. In his valuable Maps of Cornwall and Devon, Sir H. De la Beche gave essentially the same views of geological succession; and lastly, in his Report upon the geological structure of that region, he described certain detailed sections in the southern districts of Cornwall, to which I will presently advert.

In proposing the word "Devonian," as applied to the intermediate strata in question, Professor Sedgwick and myself fortunately thus qualified our meaning in regard to the extension of such rocks into Cornwall:—"In asserting that the stratified rocks of Devonshire and Cornwall are, upon a broad scale, the equivalents of the Carboniferous and Old Red systems, we do not however deny, that in certain tracts *the lowest members of some of these rocks may represent the upper division of the Silurian system*; for although we have as yet found few if any of the fossils most typical of that system, we admit that when the sedi-

\* Trans. Geol. Soc. n. s. vol. v. p. 633. † Ibid. vol. vi. pp. 221, 303.

‡ Palæozoic Fossils of Devon and Cornwall.

ments of a given epoch have been accumulated under peculiar conditions, we must expect to find considerable variations in the forms of animal life. Again, we know that the rocks of this region have undergone great changes in assuming their hard and slaty character; and under such circumstances, the difficulty of precisely limiting the boundary line of any portion of them is prodigiously increased\*."

The truth is, that neither Sir H. De la Beche and Professor Phillips, nor Professor Sedgwick and myself, had, at the time when our works were published, seen any fossils from *South Cornwall* sufficiently distinct to warrant the conclusion, that it contained forms of an older type than those which had been detected in North and South Devon and in the west of Cornwall. It was therefore believed (and all geological maps were coloured accordingly) that the zone of rocks occupying the southern headlands of Cornwall, between the Bay of Plymouth on the east and the Lizard Head on the west, were simply downward expansions of the fossiliferous "Devonian" strata. In this state of the question, your associate Mr. Peach began his labours in collecting fossils along the southern headlands of Cornwall. He first ascertained that certain forms first discovered by Messrs. Couch in the environs of Polperro were fishes, which he exhibited at the Cork Meeting of the British Association, and concerning which Professor Phillips and myself could only venture (so obscure did they appear to us) to give the guarded, though suggestive opinion, which Mr. Peach has recorded in your thirtieth Report. I then ventured to surmise, that these ichthyolites might belong to the Upper Silurian rocks, the oldest in which the remains of any vertebrated animals had yet been discovered, because "they occurred in rocks forming the axis of South Devon and Cornwall, which I had always considered to be the oldest in that country."

In pursuing his researches, Mr. Peach published in 1844 a synopsis of the Cornish fossils from various localities, in which, besides the ichthyolites of Polperro, he identified several mollusca from Gorran Haven, Caerhayes, and Carn Gorran Bay, with typical Silurian species. These were the fossils I was so anxious to see at Penzance; and Mr. Peach having obligingly forwarded them to me in London, I no sooner unpacked the box, than I found that true Silurian and even Lower Silurian rocks existed in Cornwall,—the proofs being the presence of certain simple-plaited *Orthidæ*, which are invariably typical of that age. But although Mr. Peach had come to a correct general conclusion, the specific names he attached to the South Cornish fossils in your thirtieth Report are not correct. In respect to the ichthyolites from the slates

\* Phil. Mag. 1839, vol. xiv. p. 241.

of Polperro, Pentuan, &c., they have been referred to our mutual friend Sir Philip Egerton, who is better versed in the classification of Agassiz than any of our countrymen, and he thus writes to me concerning them:—"These remains are very enigmatical, and I cannot identify a single specimen with any form I know. I do not think any one of the fragments belongs either to *Cephalaspis* or *Holoptychius*. The nearest approach is to *Bothriolepis*. The dorsal fin named by Mr. Peach *Onchus Murchisoni* (Agass.) is not that species, as far as I can determine from the description of Agassiz, unless it be a more perfect specimen than he has seen. The longitudinal ribs, instead of being uniform (as figured by Agassiz), are notched, more after the manner of *Ctenacanthus*. The other *Onchus* may be *O. tenuiserratus*, but I have not here the means of comparison. From the general appearance of the collection, I should say they differ from any Old Red or Devonian fishes I have ever seen."

If these ichthyolites do not decisively help us to settle the age of the Polperro zone of rocks, they are still of great interest, as being the only group of fishes worth noticing which has been found in the older rocks of Devonshire and Cornwall\*, and also as being associated with shells, which Mr. J. Sowerby identifies with the *Bellerophon trilobatus* (Sil. Syst.) and the *Loxomena lineta* (Phillips). The first-mentioned of these shells is characteristic of the tile-stones in Herefordshire and Shropshire, and is also found in strata of the same age in Cumberland (between Kirby Lonsdale and Kendal), which form the uppermost band of the Silurian rocks, or a transition from the Silurian into the Devonian system. Now as Professor Sedgwick and myself had inferred that the limestones of Looe and Fowey belonged to the lower calcareous zone of Devonshire, and as the sections of Sir H. De la Beche show that the Polperro beds dip beneath the Looe and Fowey rocks, the zoological evidences seem to harmonize with recorded physical facts, and we thus obtain reasonable grounds for believing, that the lowest Devonian and the uppermost Silurian strata are exposed in the district which ranges along the shores of that part of Cornwall, by Polperro, Pentuan, &c.

But if doubts should exist as to whether the Polperro slates ought to be referred to the bottom of the Devonian or top of the Silurian system, the discoveries of Mr. Peach in the headland of the Dodman, and in the prolongation of its strata to Veryan Bay, completely demonstrate, that still older and unquestionable Silurian rocks are there present. This is the district in which both

\* Professor Phillips mentions two very imperfect and doubtful scales of fishes, the one in South Devon, the other in North Devon. *Palæozoic Fossils*, p. 133, figs. 256, 257.



Professor Sedgwick and Sir H. De la Beche had noted the existence of a line of elevation\*, running from north-east to south-west, which bringing up certain quartzose or argillaceous slates had thrown the beds off, both to the south-east and north-west, the published section of the latter having clearly indicated these relations.

The fossils found by Mr. Peach at Gerrans Bay, as determined by Mr. J. Sowerby, are *Orthis lata*, *O. orbicularis*, another species resembling *O. plicata*, and a fourth which does not appear to have been published. At Cacrhayes, Mr. Peach has collected other forms of *Orthidæ*, one of which approaches nearest to the *O. alternata* of the Silurian system. The remainder are not, however, referrible, as he had supposed, to *Leptæna lata*, *Terebratula nucula*, *Atrypa striatula*, &c. The fossils from the Great Peraver quarries in Gorran Haven, on the eastern face of the Dodman, are still more decisive; for the species which Mr. Peach has named *Orthis flabellulum* and *O. testudinaria* both belong, unquestionably, to the *Orthis* (*callactis* B)† *calligramma* (Sil. Syst.), and with it is a form undistinguishable from the *Orthis* (*canalis*) *elegantula* (Sil. Syst.). The only well-preserved trilobite in this rock appears to me to be the *Calymene pulchella*? (Dalman); a second species resembles *C. Blumenbachii*.

No one accustomed to the Palæozoic rocks can throw his eye over the fossils from these three localities, without at once recognising them as true Silurian types. They have an entirely distinct *facies* from the fossils of the overlying Devonian system, and none of the species so abundant in North-western Cornwall are here present. With my imperfect knowledge of the country, it would be premature to say that subdivisions can be established in this highly dislocated region, so as to define Upper and Lower Silurian bands. But it may safely be asserted, that the fossils of Gorran Haven are Lower Silurian types; there being no one species more eminently characteristic of the inferior portion of that system than the *Orthis calligramma*, which in Shropshire and the adjacent Welsh counties is found to range downwards, from the very uppermost beds of the Caradoc sandstone into the heart of the Snowdon slates, and is equally typical of the Lower Silurian rocks of Russia and Scandinavia. At the same time, I do not think that the Gorran Haven beds lie deep in the Lower Silurian

\* See Trans. Geol. Soc., n. s., vol. v. p. 666; and Report on Cornwall and Devon, p. 84.

† This shell was called *Orthis callactis* B in the Sil. Syst. pl. 19, fig. 5, but subsequent comparisons have shown that it is identical with the *O. calligramma* (Dalman) of Scandinavia, Russia, &c.

In like manner, the *Orthis canalis* of the Silurian system has proved to be the *O. elegantula* of Dalman; and the names of that author being the oldest, are now necessarily adopted.

group: they probably represent the upper portion only of the Caradoc sandstone; for the *Orthis canalis*, or *elegantula*, and the *Calymene pulchella*\* (Dalm.), closely allied to *C. Blumenbachii*, are Wenlock, as well as Caradoc, fossils. Judging from the fossils only, I should say that the beds at Gerrans Bay with the *Orthis orbicularis* are younger than those of Peraver and Gorran Haven. Time and careful researches will, however, determine this question of detail, and all I can now express is my opinion, that the quartzose rocks and killas which extend from the tracts above alluded to, to the mouth and centre of the bay and harbour of Falmouth (probably much further to the south-west), are of Silurian age also.

The energy of Mr. Peach having thus afforded us the key by which new lights are thrown upon the succession of Cornish strata, I cannot but hope that, when the government geological surveyors revisit Cornwall, they will define the exact demarcations between these Silurian masses and their overlying Devonian neighbours. In fact, I have within these few days been talking over this subject with my friend the Director-General of the Survey, and he has pointed out to me on his detailed map, how, from the enormous flexures which the strata have undergone in their range from Devon into Cornwall, it is highly probable that Silurian rocks (the equivalents of those alluded to) may be recognised in other parts of Cornwall. Thus, the quartzose rocks of Pydar Down or Moor, to the north of St. Columb, which form an east and west axis, dipping to the north and south under fossiliferous Devonian strata, may (he thinks) prove to be also of Silurian age. But, forbearing to speculate on the probable results of future researches, it is my decided belief, that the slaty rocks constituting the great southern headland of Devonshire, at least all the schists, &c. to the north of the Start Point, will eventually be classed with the Silurian group of South Cornwall; for if the Plymouth group of limestones, so prolific in animal remains, afforded us the means of deciphering the age of less clearly developed zones on the same horizon in Cornwall, the Silurian types collected by Mr. Peach may enable us to carry out a more correct classification in still older strata, from Cornwall into the obscure southernmost promontory of Devonshire. In the mean time, confining ourselves to what we now know, it is manifest that Cornwall exhibits in ascending order from north to south,—1st, a band of true Silurian rocks; 2nd, a zone of intermediate character, forming a transition between the Silurian and Devonian systems; 3rd, a copious Devonian system, characterized by

\* The *Calymene pulchella* (Dalm.) occurs both in the inferior part of the Upper Silurian, and the higher part of the Lower Silurian, in Sweden, Siluria and Wales.

lower and upper limestones; and 4th, a limb of the culmiferous or carboniferous basin of Devonshire.

This view will, I trust, be perfectly intelligible to the members of your Society who have occupied themselves with the consideration of this branch of geology, and on which Mr. R. Q. Couch has recently written with perspicuity and talent. I doubt, however, if anything I have stated will make a due impression upon one of that number, my good-humoured antagonist the Rev. D. Williams, whose views of the Cornish succession of strata seem to be opposed to those of all his contemporaries. Geologists, however, who have long lived in Cornwall, and have so well illustrated its mineral structure, will, I am persuaded, be the first to admit the value of the Palæozoic classification, which having been worked out and established in tracts exempt from much dislocation and alteration, has been so applied, as to enable us to interpret the true history of the highly convulsed and metamorphosed rocks of their county. It is, in fact, the greatest triumph which could have been anticipated on the part of those who have steadily proceeded from the known to the unknown.

Looking from your own country to the opposite side of the channel, you are doubtless well-aware that there is the strongest analogy between the slates and granites of Cornwall and those of Brittany and Normandy. Many persons have remarked upon the strong resemblance between the Mounts St. Michael in the two countries; and no one can have traversed these two regions, without perceiving that, just as they evidently belong to the same mineral type, so are their respective inhabitants descended from a common stock, whose names of places have passed down to their French and English descendants. The existence of Caradoc sandstones and other Silurian rocks in that part of France having been already indicated (Devonian and carboniferous strata being also abundantly developed), their discovery in Cornwall is a happy addition to that union of geological and historical records, by which these widely-separated residences of the Celtic race are illustrated.

With regard to the highly mineralized or metamorphosed conditions of great portions of the killas and sandstone of Cornwall, I can do little more than refer you to the few observations I made at your anniversary meeting, and of which a brief abstract has appeared in your newspapers. There are, as you well said in your anniversary discourse, many analogies between the metamorphic rocks of Cornwall on the one hand and those of Scandinavia and the Ural mountains on the other; whilst the parallel is now drawn closer by the recent discovery of the Cornish Silurian rocks. Your last erupted granites, elvans, and porphyries, have played exactly the same part in traversing your Palæo-

zoic sediments, as like rocks have done in Norway. Cornwall may also be compared to large portions of Siberia, and notably to the Ural mountains, whose chief eruptions have taken place through deposits of Silurian, Devonian, and carboniferous age. In both countries the eruptive rocks are granites, porphyries, greenstones and serpentines: even in their superficial accumulations there is this striking resemblance, that the Cornish detritus and gravel (as clearly pointed out by Mr. Carne) is purely *local*,—the county being quite as exempt from all far-transported materials as the Ural mountains and Siberia. With this absence of all foreign transport or drift, Cornwall is as instructive as the Ural in never exhibiting those “*roches moutonnées*” and those polished and striated surfaces which have (in my opinion) been so erroneously referred to the action of land glaciers, in all those low regions of the earth, where they have clearly been caused by the action of powerful aqueous drift, in the manner I have elsewhere attempted to explain\*.

Your *stanniferous* gravel bears, indeed, precisely the same relations to your granite and killas, as the *auriferous* deposits of the Ural to the eruptive and schistose rocks of that chain. Both are mere local, shingle accumulations, derived from veinstones which have been denuded from the surface of adjacent crystalline rocks. With these analogies there is however a marked distinction between Siberia and Cornwall. All richly auriferous chains (Humboldt first remarked the fact) have a meridian direction, as in the Ural, and various north and south parallel ridges in Siberia and other parts of the globe. The axis of Cornwall, on the contrary, is transverse to that direction, viz. from E.N.E. to W.S.W.; and though containing copper ore in common with the Russian mountains, it differs from them in not producing gold or platinum; whilst it is peculiarly distinguished by containing tin, which is unknown in the Ural. Let us hope that the day is fast approaching, when the cause of the production of such striking phenomena as these will receive some explanation at the hands of those physical philosophers, who are advancing a line of research in which your own countryman Mr. R. W. Fox has already so distinguished himself. But if gold does not exist (in any appreciable quantity at least) in your otherwise richly endowed mineral county, there are, I am happy to say, good grounds for hope, that in their most distant great colony Englishmen may find it abundantly. In an address to the Royal Geographical Society, delivered in May 1845, when commenting upon the valuable labours of Count Strzelecki in deciphering the structure of the great north and south chain which ranges along

\* See Russia in Europe and Ural Mountains; and Journal of the Geol. Soc., No. 8.



the eastern shores of Australia, I specially insisted upon its striking resemblances to the Ural mountains, whether in direction, in structure, or in alluvia; remarking, by the way, that *as yet* no gold had been found in this alluvium. I now learn, however, that fine specimens of gold have been found on the western flank of the Australian cordillera, particularly at the settlement of Bathurst, where it occurs in fragments composed of the same matrix (viz. quartz rock) as in the Ural. My friend and associate in the Imperial Academy of Petersburg, Colonel Helmersen, has also recently suggested, that a careful search for gold ore in the Australian detritus will, it is highly probable, lead to its detection in abundance; since the Russians had long colonized the Ural mountains, and had for many years worked mines of magnetic iron and copper in solid rocks, before the neglected shingle, gravel and sand, on the slopes of their hills and in their valleys, were found to be auriferous. If, then, in the course of your statistical inquiries, you may know of any good Cornish miner about to seek his fortune in Australia, be pleased to tell him to apply his knowledge of the mode of extracting tin ore from his own gravel to the drift and debris on the flanks of the great north and south chain of Australia\*, or any smaller parallel ridges of that vast country; for great would be my pleasure to learn, that through the application of Cornish skill, such regions should be converted into a British "El Dorado."

Requesting you to pardon this little digression, which after all may be turned to profit, and hoping that you will be as proud as I am of the connexion which is now established between Cornwall and Siluria,

Believe me to be, my dear Sir Charles,

Yours most faithfully,

R. I. MURCHISON.

\* The grand, rich and well-watered region which lies between Moreton Bay on the south and the Gulf of Carpentaria on the north, is that to which I would specially direct attention, now that its true characters have been opened out to geographers and naturalists by the undaunted and able explorations of Dr. Leichhardt. Some of the tracts recently passed through with so much zeal, by the Surveyor-General of the colony, Sir Thomas Mitchell, may also prove valuable in gold, though they lie further from the axis of elevation. In the mean time, gold ore has been found on the other side of the Australian continent, in the ridges which extend northwards from Adelaide towards the scene of the adventurous and toilsome journey of Major Sturt. These gallant geographers, the pioneers of civilization, are explaining to us the condition of tracts which thousands of our countrymen may soon colonize with the best effects.—London, April 12, 1847.—R. I. M.

LIV. *On the Production of Light by Heat.* By JOHN WILLIAM DRAPER, M.D., *Professor of Chemistry in the University of New York* \*.

**A**LTHOUGH the phænomenon of the production of light by all solid bodies, when their temperature is raised to a certain degree, is one of the most familiar in chemistry, no person so far as I know has hitherto attempted a critical investigation of it. The difficulties environing the inquiry are so great, that even among the most eminent philosophers a diversity of opinion has prevailed respecting some of the leading facts. Thus Sir Isaac Newton fixed the temperature at which bodies become self-luminous at  $635^{\circ}$ , Sir Humphry Davy at  $812^{\circ}$ , Mr. Wedgwood at  $947^{\circ}$ , and Mr. Daniell at  $980^{\circ}$ . As respects the nature of the light emitted there are similar contradictions. In some philosophical works of considerable repute, it is stated that when a solid begins to shine it first emits red and then white rays; in others it is asserted that a mixture of blue and red light is the first that appears.

I have succeeded in escaping or overcoming many of the difficulties of this problem, and have arrived at satisfactory solutions of the main points; and as the experiments now to be described lead to some striking and perhaps unexpected analogies between light and heat, they commend themselves to our attention as having a bearing on the question of the identity of those imponderable principles. It is known that heretofore I have been led to believe in the existence of cardinal distinctions, not only between these but also other imponderable agents; and I may therefore state, that when this investigation was first undertaken, it was in the expectation that it would lead to results very different from those which have actually arisen.

The following are the points on which I propose to treat:—

1. To determine the point of incandescence of platinum, and to prove that different bodies become red-hot at the same temperature.
2. To determine the colour of the rays emitted by self-luminous bodies at different temperatures. This is done by the only reliable method—analysis by the prism.

From these experiments it will appear, that as the temperature rises the light increases in refrangibility; and making a due allowance for the physiological imperfection of the eye, the true order of the colours is red, orange, yellow, green, blue, indigo, violet.

\* Communicated by the Author.

3. To determine the relation between the brilliancy of the light emitted by a shining body and its temperature.

Here we shall find that the intensity of the light increases far more rapidly than the temperature. For example, platinum at  $2600^{\circ}$  emits almost forty times as much light as it does at  $1900^{\circ}$ .

As I prefer to give a complete description of the apparatus employed in these investigations after the general results are stated, it is sufficient here to understand that the source of light is in all instances a very thin strip of platinum 1.35 inch long and  $\frac{1}{20}$ th of an inch wide, brought to the temperature under investigation by a voltaic current. Platinum was selected from its indisposition to oxidize, and its power of resisting a high temperature without fusion.

The slip of platinum, thus to be brought to different temperatures by an electric current of the proper force, was fastened at one end to an inflexible support, and at the other was connected with a delicate lever-index, which enabled me to determine its expansion, and thereby its temperature. For this purpose I have used the coefficient of dilatation of Dulong and Petit. The temperatures here given are upon the hypothesis of the invariability of that coefficient at all thermometric degrees; they are therefore to some extent in error.

By the aid of resisting wires of different lengths and a rheostat, I was able to vary the force of the electric current in the platinum, and thereby vary its temperature. My first attempts were to discover the point at which the metal begins to emit light.

The platinum and the voltaic battery were placed in a dark room, the temperature of which was  $60^{\circ}$ ; and after I had remained there a sufficient length of time to enable my eyes to become sensible to feeble impressions of light, I caused the current to pass, gradually increasing its force, until the platinum was visible. In several repetitions of this experiment it was uniformly found that the index to which the platinum was attached stood at the eighth division when this took place. The metal had therefore dilated  $\frac{1}{22}$  of its length; the elevation of its temperature was about  $917^{\circ}$ , which added to the existing height of the thermometer,  $60^{\circ}$ , gives for the temperature of incandescence  $977^{\circ}$  F.

To the correctness of the number it may be objected, that owing to the narrowness of the metallic strip it is not well calculated to make an impression on the eye when the light it emits is so feeble; nor can we take the dilatations given by the index, as representing the uniform temperature of the whole platinum, which must necessarily be colder near its

points of support, by reason of the conducting power of the metals to which it is attached.

Physiological considerations would also lead us to suspect that the self-luminous temperature must vary with different eyes. The experiments of Bouguer, hereafter to be referred to, indisputably show that some persons are much more sensitive to the impressions of light than others. So far as my limited investigation of this matter has gone, I have not however found appreciable differences in the estimate of the temperature of incandescence. Different individuals, observing the platinum, have uniformly perceived it at the same time.

Against the number  $977^{\circ}$  it may also be objected, that antimony melts at a much lower temperature, and yet emits light before it fuses. If this statement were true, it would lead us to believe that all bodies have not the same point of incandescence. But I think the experiments of Mr. Wedgwood on gold and earthenware are decisive of that question; and, moreover, I have reason to believe that the melting-point of antimony is much higher than commonly supposed.

With a view of determining directly whether different bodies vary in their point of incandescence, I took a clean gun-barrel, and having closed the touch-hole, exposed the following substances in it to the action of the fire:—platinum, chalk, marble, fluor spar, brass, antimony, gas-carbon, lead; each specimen was small; the platinum was in the form of a coil of stout wire.

When one of these bodies was placed in the gun-barrel and the temperature raised, it is clear that any difference in their point of incandescence would be detected by the eye. Thus, if the ignition of platinum required a higher degree than iron, on looking down the barrel the coil of wire should be dark, when the barrel itself begins to shine; or, if the platinum was incandescent first, the wire should be seen before the barrel is visibly hot; and these results might be corroborated by observing the inverse phænomena, when the barrel is taken from the fire and suffered to cool.

With respect to platinum, brass, antimony, gas-carbon and lead, they all became incandescent at the same time as the iron barrel itself. I could not discover the slightest difference between them, either in heating or cooling; and it is worthy of remark, that the lead was of course in the liquid condition. But the chalk and marble were visible before the barrel was red-hot, emitting a faint white light; and the fluor spar still more strikingly so, its light being of a beautiful blue; and even when the barrel had become bright red I could still see the spar, which had decrepitated to a coarse powder, by its



faint blue rays. In these cases it was not, however, incandescence but phosphorescence that was taking place. I infer then that all solids, and probably melted metals, shine at the same thermometric point.

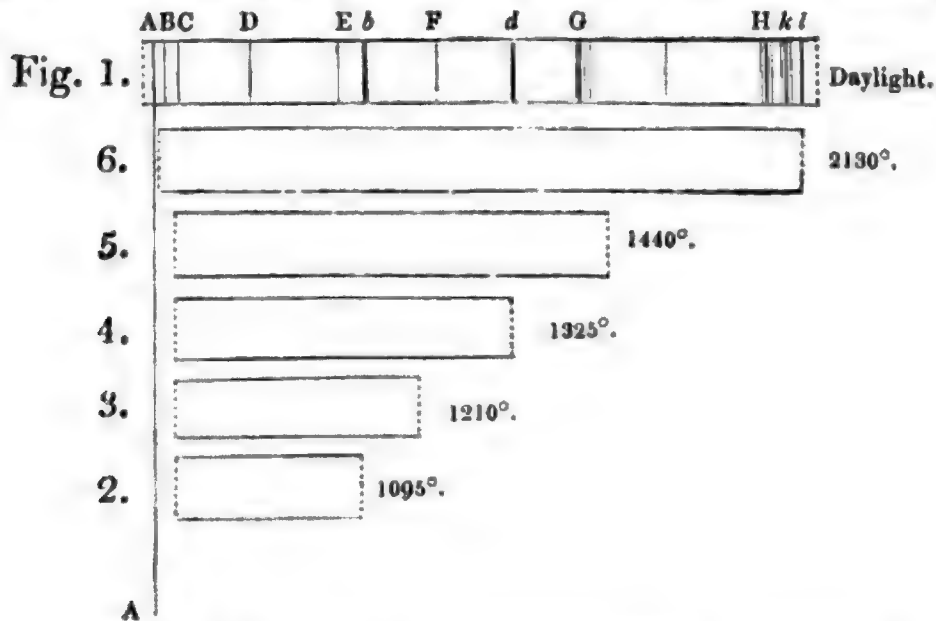
The temperature of incandescence seems to be a natural fixed point for the thermometer; and it is very interesting to remark how nearly this point coincides with  $1000^{\circ}$  of the Fahrenheit thermometer, when Laplace's coefficient for the dilatation of platinum is used. Upon that coefficient the point of incandescence is  $1006^{\circ}$  F.

In view of these considerations, and recollecting that the number given by Daniell is  $980^{\circ}$ , and that of Wedgwood  $947^{\circ}$ , I believe that  $977^{\circ}$  is not very far from the true temperature at which solids begin to shine. It is to be understood, of course, that this is in a very dark room.

I pass now to the second proposition. The rays emitted by the incandescent platinum were next received on a flint glass prism, placed so as to give the minimum deviation, and after dispersion viewed in a small telescope. A movement could be given to the telescope, which was read off on an annexed scale. However, instead of bringing the parts of the spectrum under measurement to coincide with the wires stretched across the field of the instrument, I found it more satisfactory to determine them by bringing them to one or other of the edges of the field; a process well adapted to ascertain the position of the extreme rays, the faint light of which contrasted well with the darkness by which it was surrounded. They could not have been so accurately seen while the rest of the spectrum was in view; and as it was absolutely necessary to have fixed points of reference, that all the observations might be brought to a common standard of comparison, and as there are no fixed lines in the light of incandescence, such as are in the sunshine and daylight, I therefore previously determined the position of the fixed lines in a spectrum formed by a ray of reflected daylight which passed through a fissure  $\frac{1}{20}$ th of an inch wide and one inch long, occupying exactly the position subsequently to be occupied by the incandescent platinum. Fig. 1 represents the results of this observation.

The strip of platinum was now placed in the position of the fissure which had given the spectrum fig. 1, and its temperature was raised by the passage of a voltaic current. Although I could distinctly see the metal when the heat had reached about  $1000^{\circ}$  by the naked eye, yet the loss of light in passing the prism and telescope was so great that I found it necessary to carry the temperature to  $1210^{\circ}$  before a satisfactory observation could be made. At this point the spectrum extended

from the position of the fixed line B in the red, almost as far as the line F in the green; the colours present being red,



Spectra of incandescent platinum at different temperatures.

orange, and a tint which may be designated as gray. There was nothing answering to a yellow. The first rays visible through this apparatus may therefore be designated as red and greenish gray; the former commencing at the line B, and the latter continuing to F. The magnitude and other relations of this spectrum are given in fig. 3.

The voltaic current was now increased, and the temperature rose to 1325°. The red end of the spectrum remained nearly as before, but the more refrangible extremity reached to the position of the little fixed line *d*. Traces of the yellow were now visible; and, with a certain degree of distinctness, I could see red, orange, yellow, green, and a fringe of blue. Fig. 4 shows the result.

The temperature was now carried to 1440°. I thought the red extremity was advancing more to the line A: the blue had undergone a well-marked increase. It reached considerably beyond the line G, as shown in fig. 5.

On bringing the platinum to 2130° all the colours were present, and exhibited considerable brilliancy. Their extent was somewhat shorter than that of the daylight spectrum, as is seen in fig. 6.

Having thus by repeated experiments ascertained the continued extension of the more refrangible end as the temperature rose, it became necessary to obtain observations for points below 1210°, the limit of visibility through the telescope. I therefore carried the prism nearer to the platinum, and looking with the unassisted eye directly through it at the refracted image, I found it could be distinctly seen at a temperature as

low as  $1095^{\circ}$ . Under these circumstances the total length could not be compared by direct measurement with the other observations, and the result given in fig. 2 is from the best judgement I was able to form: the colours were red and greenish-gray.

The gray rays emitted by platinum just beginning to shine appear to be more intense than the red; at all events the wires in the field of the telescope are more distinctly seen upon them than upon the other colour. I give them the designation of gray, for they appear to approach that tint more closely than any other; and yet it is to be remarked that they are occupying the position of the yellow and green regions.

Already we have encountered a fact of considerable importance. The idea, that as the temperature of a body rises it begins to emit rays of increasing refrangibility, has obviously to be taken with a certain restriction. Instead of first the red, then the orange, then the yellow, &c. rays, in succession making their appearance, in which case the spectrum should regularly increase in length as the temperature rises, we here find, at the very first moment it is visible to the eye, it yields a spectrum reaching from the fixed line B to nearly F; that is to say, equal to about two-thirds the whole length of the interference spectrum, and almost one-half of the prismatic.

It is to be remarked, that while the more refrangible end undergoes a great expansion, the other extremity exhibits a corresponding though a less change. As very important theoretical conclusions depend on the proper interpretation of this fact, we must not forget that, to a certain extent, it may be an optical deception, arising from the increased brilliancy of the light. While the rays are yet feeble, the extreme terminations may be so faint that the eye cannot detect them; but as the intensity rises, they become better marked, and an apparent elongation of the spectrum is the consequence.

It is agreed by optical writers, that to the human eye the yellow is the brightest of the rays. In the prismatic spectrum the true relationship of the colours is not perceived, because the less refrangible are crowded together, and the more refrangible unduly spread out. But in the interference spectrum, where the colours are arranged side by side in the order of their wave-lengths, the centre is occupied by the most luminous portion of the yellow; and from this point the light declines away on one side in the reds, and on the other in the blues, the terminations being equidistant from the centre of the yellow space.

Now if the rays coming from shining platinum were passed through a piece of glass, on which parallel lines had been

drawn with a diamond point, so as to give an interference spectrum, even admitting the general results of the foregoing experiments to be true, viz. that as the temperature rises rays of a higher refrangibility are emitted, it is obvious that it by no means follows that the first ray visible should be the extreme red. Our power of seeing that depends on its having a certain intensity. Even when it has assumed that extreme brilliancy which it has in a solar beam it is barely visible. We ought therefore to expect that rays of a higher refrangibility should first be seen, because they act more energetically on our organ of vision; and as the temperature rises, the spectrum should undergo a partial elongation in the direction of its red extremity.

I may here remark, that the general result of these experiments coincides exactly with that of M. Melloni respecting heat at lower thermometric points. In his second memoir\*, he shows that when the rays from copper at  $390^{\circ}$  and from incandescent platinum are compared by transmission through a rock-salt prism, as the temperature rises the refrangibility of the calorific emanations correspondingly increases. Those philosophers who regard light and heat as the same agent, will therefore see in this coincidence another argument in favour of their opinion.

In view of the foregoing facts I conclude, that, *as the temperature of an incandescent body rises, it emits rays of light of an increasing refrangibility*; and that the apparent departure from this law, discovered by an accurate prismatic analysis, is due to the special action of the eye in performing the function of vision.

As the luminous effects are undoubtedly owing to a vibratory movement executed by the molecules of the platinum, it seems from the foregoing considerations to follow, that the frequency of those vibrations increases with the temperature.

In this observation I am led by the principle, that "to a particular colour there ever belongs a particular wave-length, and to a particular wave-length there ever belongs a particular colour;" but in the analysis of the spectrum made by Sir D. Brewster by the aid of absorptive media, this principle is indirectly controverted; that eminent philosopher showing that red, yellow, blue, and consequently white light, exist in every part of the spectrum. This must necessarily take place when a prism which has a refracting face of considerable magnitude is used; for it is obvious that a ray falling near the edge, and one falling near the back, after dispersion, will paint their several spectra on the screen; the colours of the one not co-

\* Taylor's Scientific Memoirs, vol. i. p. 56.



inciding with, but overlapping the colours of the other. In such a spectrum there must undoubtedly be a general commixture of the rays; but may we not fairly inquire whether, if an elementary prism were used, the same facts would hold good; or, if the anterior face of the prism were covered by a screen, so as to expose a narrow fissure parallel to the axis of the instrument, would there be found in the spectrum it gave every colour in every part, as in Sir David Brewster's original experiment? M. Melloni has shown how this very consideration complicates the phænomena of radiant heat; and it would seem a very plausible suggestion that the effect here pointed out must occur in an analogous manner for the phænomena of light.

I proceed now to the third branch of the inquiry,—to examine the relation between the temperatures of self-luminous bodies and the intensity of the light they emit, premising it with the following considerations.

The close analogy which is traced between the phænomena of light and radiant heat lends countenance to the supposition, that the law which regulates the escape of caloric from a body will also determine its rate of emission of light. Sir Isaac Newton supposed that whilst the temperature of a body rose in arithmetical progression, the amount of heat escaping from it increased in a geometrical progression. The fallacy of this was subsequently shown by Martin, Erxleben, and Delaroche; and finally Dulong and Petit gave the true law, “when a body cools *in vacuo*, surrounded by a medium whose temperature is constant, the velocity of cooling for excess of temperature in arithmetical progression increases as the terms of a geometrical progression, diminished by a constant quantity.” The introduction of this constant depends on the operation of the theory of exchanges of heat; for a body, when cooling under the circumstances here given, is simultaneously receiving back a constant amount of heat from the medium of constant temperature.

Whilst Newton's law represents the rate of cooling of bodies, and therefore the quantities of heat they emit, when the range of temperature is limited, and the law of Dulong and Petit holds to a wider extent, there are in our inquiry certain circumstances to be taken into account not contemplated by those philosophers. Dulong and Petit throughout their memoir regard radiant heat as a homogeneous agent, and look upon the theory of exchanges, which is indeed their starting-point and guide, as a very simple affair. But the progress of this department of knowledge since their times has shown, that precisely the same modifications as are found in the co-

lours of light, occur also for heat; a fact conveniently designated by the phrase "ideal coloration of heat;" and further, that the colour of the heat emitted depends upon the temperature of the radiating source. It is one thing to investigate the phenomena of the exchanges of heat-rays of the same colour, and another when the colours are different. A perfect theory of the exchanges of heat must include the principle of ideal coloration, and, of course, so too must a law of cooling applicable to any temperature.

There is another fact to some extent considered by Dulong and Petit, but not of such weight in their investigations, where the range of temperature was small, as in ours, where it rises as high as nearly 3000° F.; I mean the difference of specific heat of the same body at different temperatures. At the high temperatures considered in this memoir, there cannot be a doubt that the capacity of platinum for heat is far greater than that at a low point. This therefore must control its rate of calorific emission, and probably that for light also.

From these and similar considerations, we should be prepared to discover that as the temperature of an incandescent solid rises, the intensity of the light emitted increases very rapidly.

I pass now to the experimental proofs which substantiate the foregoing reasoning.

The apparatus employed as the source of the light and measure of the temperature was the same as in the preceding experiments,—a strip of platinum, brought to a known temperature by the passage of a voltaic current of the proper force, and connected with an index which measured its expansion.

The principle upon which I have determined the intensities of the light is that first described by Bouguer, and recently introduced by M. Masson. After many experiments I have been led to conclude that this is the most accurate method known.

Any one who will endeavour to determine the intensities of lights by Rumford's method of contrasting shadows, or by that of equally illuminated surfaces, will find, when every precaution has been used, that the results of repeated experiments do not accord. There is moreover the great defect, that where the lights differ in colour it is impossible to obtain reliable measures, except by resorting to such contrivances as that described by me\*.

Bouguer's principle is far more exact; and where the lights differ in colour, that difference actually tends to make the

\* Phil. Mag., August 1844.

result more perfect. As it is not generally known, I will indicate the nature of it briefly.

Let there be placed at a certain distance from a screen of white paper, a candle so arranged as to throw the shadow of a ruler, or other opaque body, on the screen. If a second candle be placed also in front of the paper and nearer than the former, there is a certain distance at which its light completely obliterates all traces of the shadow. This distance is readily found; for the disappearance of the shadow can be determined with considerable exactness. When the lights are equal, Bouguer found that the relative distances were as 1 : 8; he inferred therefore, correctly, that in the case of his eye, the effect of a given light was imperceptible when it was in presence of another sixty-four times as intense. The precise number differs according to the sensibility of different eyes, but for the same organ it is constant.

Upon a paper screen I threw the shadow of a piece of copper, which intercepted the rays of the incandescent platinum: then taking an Argand lamp, surrounded by a cylindrical metal shade through an aperture in which the light passed, and the flame of which I had found by previous trial would continue for an hour almost of the same intensity, I approached it to the paper until the shadow cast by the copper disappeared. The distance at which this took place was then measured, and the temperature of the platinum determined.

The temperature of the platinum was now raised; the shadow became more intense, and it was necessary to bring the Argand lamp nearer before it was effaced. When this took place the distance of the lamp was again measured, and the temperature of the platinum again determined.

In this manner I obtained several series of results, one of which is given in the following table. They exhibited a more perfect accord among each other than I had anticipated.

Table of the Intensity of Light emitted by Platinum at different Temperatures.

Temperature of the platinum.	Distance of Argand lamp.		Mean.	Intensity of light.
	Experiment 1.	Experiment 2.		
980	.....	.....	.....	0.00
1900	54.00	54.00	54.00	0.34
2015	39.00	41.00	40.00	0.62
2130	24.00	24.00	24.00	1.73
2245	18.00	19.00	18.50	2.92
2360	14.50	15.50	15.00	4.40
2475	11.50	12.00	11.75	7.24
2590	9.00	9.00	9.00	12.34

The intensity of the light of the platinum is of course inversely proportional to the square of the distance of the Argand lamp at the moment of the obliteration of the shadow.

In this table the first column gives the temperatures under examination in Fahrenheit degrees; the second and third the distances of the Argand lamp from the screen, in English inches, in two different sets of experiments; the fourth the mean of the two; and the fifth the corresponding intensity of the light.

From this it is at once perceived, that the increases in the intensity of the light, though slow at first, become very rapid as the temperature rises. *At 2590° the brilliancy is more than thirty-six times as great as it is at 1900°.*

Thus, therefore, the theoretical anticipation which we founded on the analogy of light and heat is completely verified; and we discover that as the temperature of a self-luminous solid rises, it emits light in a greater proportion than would correspond to the mere difference of temperature. To place that analogy in a still more striking point of view, I will here introduce some experiments I have made in relation to radiant heat. No chemist, so far as I am aware, has hitherto published results for high temperatures, or endeavoured to establish, through an extensive scale, the principle of Delaroché, that "the quantity of heat which a hot body gives off in a given time by way of radiation to a cold body, situated at a distance, increases, other things being equal, in a progression more rapid than the excess of the temperature of the first above that of the second."

As my object on the present occasion is chiefly to illustrate the remarkable analogy between light and heat, the experiments now to be related were arranged so as to resemble the foregoing; that is to say, as in determining the intensities of light emitted by a shining body at different temperatures, I had received the rays upon a screen placed at an invariable distance, and then determined their value by photometric methods; so, in this case, I received the rays of heat upon a screen placed at an invariable distance, and determined their intensity by thermometric methods. In this instance the screen employed was in fact the blackened surface of the thermoelectric pile. It was placed at a distance of about one inch from the slip of incandescent platinum, a distance sufficient to keep it from any disturbance from the stream of hot air arising from the metal; care also was taken that the multiplier itself was placed so far from the rest of the apparatus, that its astatic needles could not be affected by the voltaic current



igniting the platinum, or the electro-magnetic action of the wires used to modify the degrees of heat.

The experiments were conducted as follows:—The needles of the thermo-multiplier standing at the zero of their scale, the voltaic current was passed through the platinum, which immediately rose to the corresponding temperature, and radiated its heat to the face of the pile. The instant the current passed, the needles of the multiplier moved, and kept steadily advancing upon the scale. At the close of one minute, the deviation of the needle and the temperature of the platinum were simultaneously noted, and then the voltaic current was stopped.

Sufficient time was now given for the needle of the multiplier to come back to zero. This time varied in the different cases, according to the intensity of the heat to which the pile had been exposed: in no instance, however, did it exceed six minutes, and in most cases was much less. A little consideration will show that the usual artifice employed to drive the needles back to zero, by warming the opposite face of the pile, was not admissible in these experiments.

The needles having regained their zero, the platinum was brought again to a given temperature, and the experiment conducted as before. The following table exhibits a series of these results.

Table of the Intensity of Radiant Heat emitted by Platinum at different Temperatures.

Temperature of the platinum.	Intensity of heat emitted.		Mean.
	Experiment 1.	Experiment 2.	
980	·75	1·00	·87
1095	1·00	1·20	1·10
1210	1·40	1·60	1·50
1325	1·60	2·00	1·80
1440	2·20	2·20	2·20
1555	2·75	2·85	2·80
1670	3·65	3·75	3·70
1785	5·00	50·0	5·00
1900	6·70	6·90	6·80
2015	8·60	8·60	8·60
2130	10·00	10·00	10·00
2245	12·50	12·50	12·50
2360	15·50	15·50	15·50

In this table the first column gives the temperatures of the

platinum in Fahrenheit degrees; the second and third two sets of experiments, expressing the arc passed over by the needle at the close of a radiation lasting for one minute, each number being the mean of several successive trials; and the fourth the mean of the two. It therefore gives the radiant effect of the incandescent platinum upon the thermo-multiplier for the different temperatures.

Of course it is understood that I here take the angular deviations of the needle as expressing the force of the thermo-electric current, or in other words, as being proportional to the temperatures. This hypothesis, it is known, is admissible.

It therefore appears that the quantity of heat radiated by incandescent platinum at  $980^{\circ}$  being taken as unity, it will have increased at  $1440^{\circ}$  to 2.5; at  $1900^{\circ}$  to 7.8; and at  $2360^{\circ}$  to 17.8, nearly: the rate of increase is therefore very rapid. Further, it may be remarked, as illustrative of the same fact, that the increased quantity of heat radiated by a mass of platinum in passing from  $1000^{\circ}$  to  $1300^{\circ}$ , is nearly equal to the amount it gives out in passing from common temperatures up to  $1000^{\circ}$ .

I cannot here express myself with too much emphasis on the remarkable analogy between light and heat which these experiments reveal. The march of the phænomena in all their leading points is the same in both cases. The rapid increase of effect as the temperature rises is common to both.

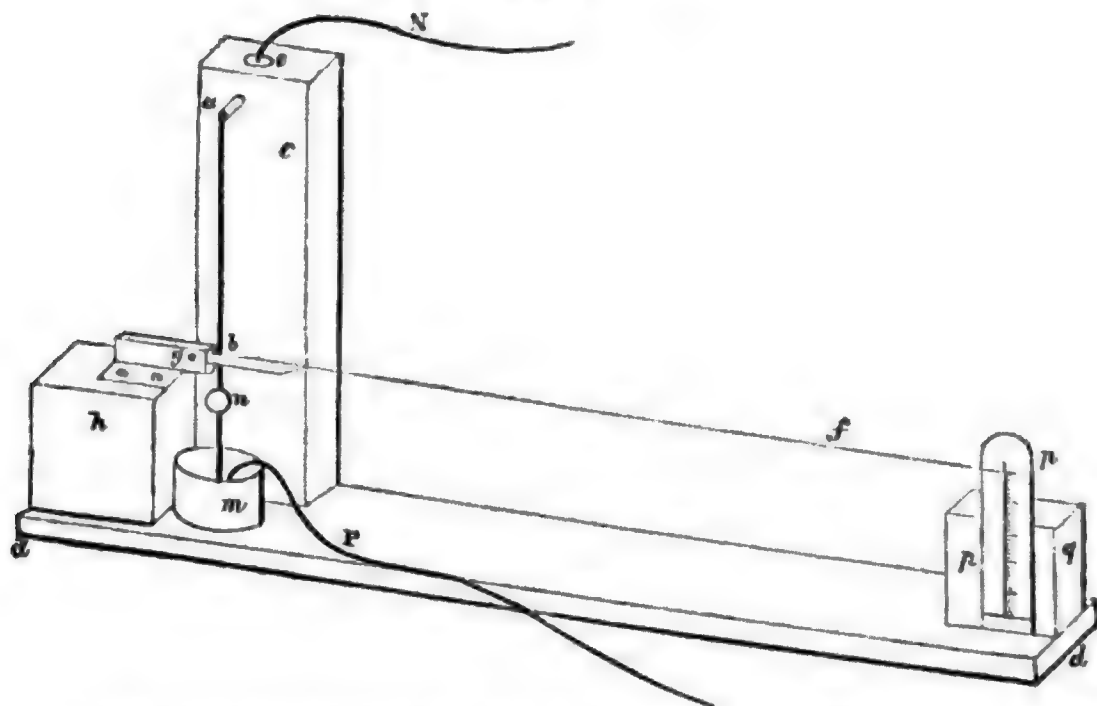
It is not to be forgotten, however, that in the case of light we necessarily measure its effects by an apparatus which possesses special peculiarities. The eye is insensible to rays which are not comprehended within certain limits of refrangibility. In these experiments, it is requisite to raise the temperature of the platinum almost to  $1000^{\circ}$  before we can discover the first traces of light. Measures obtained under such circumstances are dependent on the physiological action of the visual organ itself, and hence their analogy with those obtained by the thermometer becomes more striking, because we should scarcely have anticipated that it could be so complete.

#### *Description of the apparatus employed in the foregoing experiments.*

The source of light is in all instances a slip of platinum foil 1.35 inch long, and  $\frac{1}{20}$ th of an inch broad, ignited by the passage of a voltaic current, and placed in such a position that its dilatation could be measured by the movements of an index over a graduated scale.

In fig. 7, *a b* represents the slip of platinum, the upper end of which is soldered to a stout and short copper pin *a*, firmly sunk in a block of wood *c*, which is immoveably fastened on the basis *d d* of the instrument. A cavity *e*, half an inch in diameter, is sunk in the block *c*, and into this cavity the pin *a* projects; so that when the cavity is filled with mercury, a voltaic current may be passed through the pin and down the platinum.

Fig. 7.



The other extremity of the platinum *b* is fastened to a delicate lever *b f*, which plays on an axis at *g*, the axis working in brass holes supported on a block *h*. Immediately beneath the platinum strip, and in metallic communication with it, a straight copper wire dips down into the mercury cup *m*; on this wire there is a metal ball *n*, weighing about 100 grains. The further end of the index plays over a graduated ivory scale *p p*, which is supported on a block *q*, and can be moved a little up and down, so as to bring its zero to coincide with the index at common temperatures.

The action of the instrument is readily understood. In the mercury cup *e* dip one of the wires *N* of a Grove's battery of three or four pairs, the other wire *P* being dipped into the cup *m*. The current passes through the platinum, which immediately expands, the weight *n* lightly stretching it. The index *f* moves promptly over the scale, indicating the amount of expansion, and therefore the degree of heat. Remove the wire *N* out of its mercury cup *e*, the platinum instantly becomes cold, and pulls the lever to the zero point.

When the platinum is thin, so as to be quite flexible at the

point *b*, where it is fastened to the index, the movements take place with such promptitude and precision as to leave nothing to be desired. When the heat has been very high and long continued, the limit of elasticity of the platinum is somewhat overpassed, and it suffers a slight permanent extension. But as the ivory scale *p p* can slide up and down a little, the index is readily re-adjusted to the zero point.

The temperature of the platinum depends entirely on the force of the current passed through it. By intervening coils of brass wire of lengths adjusted beforehand, so as to resist the current to a given extent, any desired temperature may be reached. I found it convenient to intervene in the course of the current one of Prof. Wheatstone's rheostats, so as to be able to bring the index with precision to any degree, notwithstanding slight changes in the force of the voltaic battery.

The following are the dimensions and measures of the instrument I have used:—Length of the platinum strip, 1.35 inch; length of the part actually ignited, 1.14 inch; width of ditto,  $\frac{1}{20}$ th of an inch; length of the index from its centre of motion to the scale, 7.19 inches; distance of the centre of motion of index from the insertion of the platinum at the point *b*, .22 inch; multiplying effect of the index, 32.68 times; length of each division on the ivory scale, .021 inch. From this it would appear, by a simple calculation, using the coefficient of dilatation of platinum given by Dulong and Petit, that each of the divisions here used is equal to 114.5 Fahrenheit degrees. For the sake of perspicuity I have generally taken them at 115°.

The Grove's battery I have employed has platinum plates three inches long and three quarters wide; the zinc cylinders are two inches and a half in diameter, three high, and one-third thick. As used in these experiments, it could maintain a current nearly uniform for an hour. I commonly employed four pairs.

Among writers on optics, it has been a desideratum to obtain an artificial light of standard brilliancy. The preceding experiments furnish an easy means of supplying that want, and give us what might be termed a "unit-lamp." A surface of platinum of standard dimensions, raised to a standard temperature by a voltaic current, will always emit a constant light. A strip of that metal, one inch long and  $\frac{1}{20}$ th of an inch wide, connected with a lever by which its expansion



might be measured, would yield at  $2000^{\circ}$  a light suitable for most purposes. Moreover, it would be very easy to form from it an available photometer, by screening portions of the shining surface. An ingenious artist would have very little difficulty, by taking advantage of the movements of the lever, in making a self-acting apparatus, in which the platinum should be maintained at a uniform temperature, notwithstanding any change taking place in the voltaic current.

University, New York,  
Feb. 27, 1847.

LV. *On the Acid contained in the North American Columbite.*  
By HENRY ROSE\*.

THE columbite of North America has the same crystalline form as that from Bodenmais in Bavaria, but is distinguished from it in general by a far lower specific gravity; however, we find the same difference in the specific gravity of the American mineral as occurs in the different crystals of the Bodenmais columbite. The lightest crystals from the last locality have the same specific gravity (5.704) as the heaviest crystals from North America (5.708).

I have already communicated two analyses of North American columbites, of one of which however it was doubtful whether it came from America. The following analysis of American columbite was made by M. Grewink in my laboratory; it yielded,—

Acid . . . . .	80.06
Protoxide of iron . . . . .	12.59
Protoxide of manganese . . . . .	5.97
Oxide of tin . . . . .	0.96
Oxide of copper and lead . . . . .	0.44
	<hr/>
	100.02

The specific gravity in fragments was 5.323; in powder, 5.3202.

This columbite comes nearest in composition and also in specific gravity to that examined by M. Schlieper.

I have on a former occasion shown that the different specific gravity of the crystals of the Bavarian columbite was owing to the different proportions of niobic and pelopic acids which are found in the different crystals. The specific gravity of these two acids is widely different, but unequally so, according to the temperatures to which they have been exposed previous to weighing.

Owing to want of material, I found it impossible to make a

\* Translated from Poggendorff's *Annalen*.

thorough examination of the two acids which are contained in the North American columbite: I very soon ascertained, after the discovery of niobic acid, that this was the principal acid constituent in the American columbite, but I could not determine whether it was mixed with pelopic or with tantalic acid; I therefore addressed myself to Mr. B. Silliman of New-haven, who with the greatest readiness procured me a very considerable quantity (half a pound) of this now very rare mineral.

A large quantity of this columbite was used for the preparation of the acid. When treated in the same manner as that from the Bavarian columbite, it proved to consist principally of niobic acid combined with pelopic acid; but the amount of the latter was far smaller than in the Bodenmais mineral, so that I do not think it would have been possible for me to have examined the properties of pelopic acid so completely as was necessary in order to recognise it as an essentially distinct acid from tantalic acid, if I had had only the American mineral at my disposal. But both the acids were so perfectly identical in all their properties with the two acids prepared from the Bodenmais mineral, that I did not find the least difference, even as regards the specific gravity.

As the specific gravity of pelopic acid is considerably higher than that of niobic acid, when the two are heated in the same manner, the higher specific gravity of the Bavarian columbite is thus satisfactorily explained.

I have moreover found small quantities of tungstic acid in the acids from the American columbite, as well as in those from the Bodenmais mineral.

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LVI. *Abstract of Meteorological Observations made during the year 1846 at Gongo Soco, in the interior of Brazil.* By WILLIAM JORY HENWOOD, F.R.S., F.G.S., Member of the Geological Society of France, Chief Commissioner of the Gold Mines of Gongo Soco, Catta Preta, and Antonio Pereira, &c. &c.\*

I HAVE nothing to add to what has been already said† respecting the locality and the positions of the instruments. I have again to thank Captains Blaney, Luke and Guy, for the continuation of their midnight observations, as well as for a second series, made at 3 A.M.

\* Communicated by the Author.

† Phil. Mag. 1846, xxviii. pp. 364, 366.

Table I.  
Hourly mean and extreme temperatures for every month.

	3 A.M.			6 A.M.			9 A.M.			Noon.			4 P.M.			6 P.M.			8 P.M.*			9 P.M.			Midnight.		
	Max.	Min.	Mean.	Max.	Min.	Mean.	Max.	Min.	Mean.	Max.	Min.	Mean.	Max.	Min.	Mean.	Max.	Min.	Mean.	Max.	Min.	Mean.	Max.	Min.	Mean.	Max.	Min.	Mean.
Jan.	69.8	60.8	66.	69.8	63.	67.	76.	66.4	70.6	81.	67.5	76.1	83.5	67.5	76.1	81.	67.2	74.	75.5	65.4	71.	73.8	65.	70.1	70.8	62.8	67.1
Feb.	73.8	61.8	68.5	71.	63.2	68.	79.5	65.	73.2	86.	66.	78.6	86.2	67.1	78.8	83.	66.2	75.8	77.8	65.	72.7	76.8	64.6	71.7	72.8	61.8	69.5
Mar.	70.8	63.8	67.	71.2	62.5	67.3	77.	67.2	72.5	82.5	72.	77.7	84.9	71.	77.6	81.	69.3	75.2	77.5	68.	71.8	76.	67.8	70.9	75.8	64.8	68.4
Apr.	69.8	51.8	62.3	67.5	51.	62.4	72.6	61.2	68.	78.	67.3	72.6	78.3	67.5	73.1	73.8	65.	69.7	71.5	60.6	67.6	70.3	59.5	66.5	70.8	54.8	63.7
May	68.8	52.8	61.1	68.	53.5	61.1	73.5	58.2	65.2	80.	63.	71.1	78.2	63.8	71.6	75.	61.8	67.9	75.5	59.2	65.4	74.	58.6	64.4	70.8	55.8	62.5
June	61.8	43.8	54.4	61.5	43.8	54.6	67.	53.8	60.3	71.5	58.	66.6	73.	59.	67.2	66.8	54.	62.3	64.5	50.7	59.1	63.6	49.5	58.1	61.8	45.8	56.2
July	58.8	40.8	52.4	59.5	41.8	52.3	62.5	50.	57.5	67.	58.	63.7	68.5	56.	64.2	64.7	53.	60.2	63.	50.	56.5	62.5	49.	55.6	60.8	41.8	52.8
Aug.	60.8	44.8	53.4	60.	44.	53.6	69.	52.8	59.9	73.	58.7	67.1	75.	58.5	68.7	69.6	58.	64.7	65.	54.2	59.9	64.2	52.8	58.7	60.8	48.8	55.1
Sep.	70.8	48.8	58.3	70.	45.8	57.3	72.	55.1	62.7	82.	58.	71.5	82.8	58.6	72.6	80.1	56.8	68.5	75.	54.5	64.1	73.8	52.5	63.1	71.8	48.8	59.8
Oct.	72.8	56.8	64.8	72.	57.	63.8	78.6	61.5	70.7	88.5	67.8	79.3	88.8	71.	80.7	85.5	68.	76.8	77.	63.5	71.2	76.2	62.4	69.9	74.8	57.8	66.8
Nov.	69.8	58.8	65.7	71.5	59.2	66.5	78.	64.	69.8	90.	68.5	75.9	91.7	69.5	77.	84.8	67.8	74.4	76.8	65.	71.4	75.7	64.3	70.5	74.8	60.8	67.5
Dec.	70.8	61.8	66.5	69.8	62.	66.9	76.	66.	71.	85.	70.	76.8	86.	71.4	77.3	81.8	69.6	75.6	75.4	62.	71.6	74.5	62.	70.9	70.8	64.8	67.5

Table II.		Mean temperature of each month.	
January .....	70.87	May .....	65.11
February.....	73.01	June .....	59.96
March.....	72.07	July .....	57.34
April .....	67.29	August .....	60.15
		September ...	64.24
		October .....	71.60
		November ...	70.91
		December ...	71.56

\* The observations at 8 P.M. are not employed in obtaining the monthly means.

Table III.—Mean temperature of each of nine hours.

3 A.M.	. 61 <sup>o</sup> ·7	4 P.M.	. . 73 <sup>o</sup> ·74
6 ...	. 61·73	6 ...	. . 70·42
9 ...	. 66·78	8 ...	. . 66·85
Noon	. 73·08	9 ...	. . 65·86
		Midnight	. 63·07

This gives 67° as the mean temperature of the year 1846 ; a result which probably differs but very slightly, if at all, from the truth.

Table IV.—Comparative temperature in shade and in open sunshine.

Date.	Therm. shade.	Therm. sunshine.	Remarks.
Jan. 3, 4 P.M.	81 <sup>o</sup> ·2	93 <sup>o</sup> ·5	Gale N.
Feb. 8, ...	85·1	94·5	Light breeze N.E.
... 14, ...	86·7	104·5	Brisk breeze N.
... 15, Noon.	85·4	99·8	Brisk breeze W.
May 3, ...	73·	86·	Brisk breeze E.
... 24, ...	71·8	84·	Brisk breeze N.E.
June 28, ...	63·2	80·7	Brisk breeze N.E.
July 19, ...	66·5	84·7	Light E.
Aug. 9, 4 P.M.	72·	81·	Light W.
... 22, ...	71·	85·4	Light E.
... 23, Noon.	71·5	85·8	Brisk E.
... 30, ...	72·8	84·8	Gale E.
Sept. 27, ...	75·	89·2	Brisk W.
... ... 4. P.M.	79·8	91·3	Light S.W.
Oct. 11, Noon.	80·8	96·	Brisk E.
... 18, ...	77·	87·5	Brisk W.
... 25, ...	84·	95·2	Brisk E.
... ... 4 P.M.	87·	90·5	Calm.
Nov. 15, Noon.	76·2	88·5	Light W.
Dec. 27. ...	77·4	96·	Brisk N.W.

On the mornings of the 27th, 28th, 29th and 30th of July, there was sharp hoar-frost.

At 3 P.M. of the 28th of October there was a heavy fall of hail, the thermometer in the shade standing at 78°.

The first fire-fly appeared on the 7th of August (the mean temperature of the twenty-four hours being 61<sup>o</sup>·6), and on the 11th they were numerous.

The American robin (*Sabea*) was first heard on the 4th of September, when the mean temperature was 60<sup>o</sup>·7\*.

\* The song of this bird, almost our only songster, is considered by the natives a certain sign of the rainy season.



364 *Abstract of Meteorological Observations in Brazil.*

The coldest day was the 28th of July, when the mean of the twenty-four hours was 49°. The hottest was the 1st of November, which averaged 75°·9.

My hygrometrical observations are but few, yet they may not be entirely worthless.

Table V.

	Date.	Temp. of air—shade.	Dew-point.	Remarks.
Oct.	4, Noon.	70·8	61·5	Cloudy.
...	11, 9 A.M.	70·	62·	Light clouds.
...	... Noon.	80·8	65·	Clear.
...	25, 9 A.M.	75·	64·5	Close, smoky.
...	26, 6 P.M.	82·8	68·5	Light clouds.
Nov.	1, Noon.	86·8	67·5	Hazy, thunder.
...	8, 9 A.M.	68·3	65·	Showery.
...	9, ...	71·	63·5	Showery.

Table VI.—Quantity of rain.

	No. of rainy days.	Rain.
January . . . .	20	24·25 inches.
February . . . .	22	19·30
March . . . .	15	7·08
April . . . .	15	5·48
May . . . .	10	2·96
June . . . .	6	3·24
July. . . .	11	1·40
August. . . .	3	0·54
September . . .	10	3·45
October . . . .	5	4·22
November . . .	22	18·16
December . . . . .		16·64

Total in 1846 . . 106·72

The heaviest showers during the year were

January 20, when 5·24 inches fell in 10½ hours.

October 1, ... 2·24 ... 1¼ ...

... 29, ... 0·66 ... 20 minutes.

The greatest quantity in twenty-four hours was on the 1st of December, when it amounted to 6·3 inches.

No one can be more fully aware than I am that many other observations are necessary to present a perfect view of the climate; but the want of instruments, as well as other more pressing occupations, will, I hope, be a sufficient excuse for the deficiency.

W. J. HENWOOD.

Gongo Soco Gold Mines,  
January 16, 1847.

LVII. *Proceedings of Learned Societies.*

## CAMBRIDGE PHILOSOPHICAL SOCIETY.

[Continued from p. 130.]

May 11, **A** THEORY of Luminous Rays on the Hypothesis of 1846. Undulations. By the Rev. J. Challis, M.A., Plumian Professor of Astronomy and Experimental Philosophy in the University of Cambridge.

In this communication, the æther, which is supposed to be the medium of the transmission of light, is regarded as a continuous fluid substance, such that small increments of its pressure are proportional to small increments of density, and is treated mathematically according to hydrodynamical principles. The author shows, by means of the usual hydrodynamical equations, and by an additional equation of continuity, the existence and necessity of which he has considered in the Cambridge Philosophical Transactions (vol. vii. part iii. pp. 385 and 386), that a given slender cylindrical portion of the fluid may continue in motion without tendency to lateral spreading, while all other parts remain at rest. It is shown,—1, that the motion in this filament of fluid may be propagated with a uniform velocity; 2, that in one straight line, which may be called its axis, the motion is entirely longitudinal; 3, that at all other points the motion is partly longitudinal and partly transversal; 4, that the motion is vibratory, the vibrations both longitudinal and transversal following the law of sines; 5, that the condensation ( $s$ ) in any transverse plane, at a point whose co-ordinates in that plane reckoned from the axis are  $x$  and  $y$ , is given by the equation

$$\frac{d^2s}{dx^2} + \frac{d^2s}{dy^2} + gs = 0,$$

$g$  being a certain constant. It follows that the condensation in any transverse plane, being determined by a partial differential equation, is arbitrary, and by consequence that the transverse velocity varies at a given time from point to point of any transverse plane in an arbitrary manner. To obtain the foregoing equation, it is assumed that the condensation at any point of a transverse plane, has to the condensation at the intersection of the plane with the axis, a ratio not variable with the time.

Each fluid filament in vibration is supposed in this theory to correspond to a ray of light. The vibrations in different fluid filaments may co-exist, and consequently rays be propagated in the same direction independently of each other. A ray of common light has the condensation symmetrically arranged about the axis.

May 25, 1846.—A Theory of the Polarization of Light on the Hypothesis of Undulations. By the same Author.

This paper is a continuation of the foregoing. A ray in which the condensation is not arranged symmetrically about the axis is considered to be *polarized*. Polarization in this theory corresponds to difference of condensation in different directions transverse to the

axis of the ray. The sensation of light is due to the *transverse* vibrations. By assuming that the bifurcation of a ray takes place so that the transverse velocity at each point is resolved into two velocities at right angles to each other, and that these are respectively the velocities at the corresponding points of the two parts into which the ray is divided, Professor Challis finds,—1, that if the original ray be one of common light, the two parts are symmetrical about planes at right angles to each other passing through the axis, and are each of half the intensity of the original ray; 2, that if the original ray has been once polarized, the ratio of the two parts is equal to the square of the tangent of the angle which the plane of the second polarization makes with that of the first; 3, that whether the original ray be one of common light or a polarized ray, the two parts, on pursuing the same path, form a compound ray the intensity of which is independent of the difference of phase. According to this theory, elliptically or circularly polarized light is produced whenever a ray of first polarization is divided into two parts which subsequently pursue the same path in different phases. If the parts be made to meet in the same phase, they constitute the original polarized ray. Hence is explained the necessity of the analysing plate for the production of colours by polarized rays transmitted through thin pieces of uniaxal or biaxal crystals. The compound rays, if received directly by the eye on leaving the crystal, would be of the same intensity whatever be the difference of phase. But when they fall on the plate, those incident in the same phase, being equivalent to rays of first polarization, are incapable of reflexion, while the remainder, which are incident in the form of elliptically or circularly polarized light, are reflected in different degrees of intensity according to the difference of phase. The author states that he has extended this theory to the phenomena of double refraction.

On a Change in the State of Vision of an Eye affected with a mal-formation.

Twenty years ago, the author communicated to the Society a statement of the effects of a mal-formation in his left eye. The rays of light coming from a luminous point, and falling on the whole surface of the pupil, do not converge to a point at any position within the eye, but converge so as to pass through two lines at right angles to each other, and, in the ordinary position of the head, inclined to the vertical, as formerly described (*Transactions of the Society*, vol. ii.). As the luminous point is moved further from or nearer to the eye, the image of the point becomes a straight line in one or other of the positions above-mentioned. Since 1825 the inclinations of the two focal lines to the vertical, their length, and their sharpness do not appear to have undergone any sensible change, but the distances at which the luminous point must be placed to bring the focal lines respectively exactly upon the retina are increased, having been formerly 3·5 and 6 inches, and being now 4·7 and 8·9 inches. Thus while the shortsightedness of the eye is diminished the astigmatism remains the same,

“On the Geometrical Representation of the roots of Algebraic

Equations." By the Rev. H. Goodwin, late Fellow of Caius College, and Fellow of the Cambridge Philosophical Society.

The changes of value of any function of  $x$ ,  $f(x)$ , may be very clearly, and for some purposes very usefully represented, by tracing the curve defined by the equation  $z=f(x)$ ; and the positive and negative roots of the equation  $f(x)=0$  will be the distances from the origin at which the curve cuts the axis of  $x$ .

In this memoir a similar method is applied to the representation of the changes of value of a function of  $(x)$ , corresponding not only to real values of  $x$ , but also to values of the form  $x+y\sqrt{-1}$ . If we make  $z=f(x+y\sqrt{-1})$ , and restrict ourselves to real values of  $z$ , the equation separates itself into two, which, it is shown, may be represented symbolically by

$$z=\cos\left(y\frac{d}{dx}\right)f(x)$$

$$\text{and } 0=\sin\left(y\frac{d}{dx}\right)f(x),$$

and these will correspond to a curve of double curvature, the intersections of which with the plane of  $xy$  will determine by the distance of those points from the origin the imaginary roots of the equation  $f(x)=0$ .

The properties of this curve are fully discussed for the case of  $f(x)$  being equivalent to  $x^n+p_1x_{n-1}+p_2x_{n-2}+\dots+p_n$ , where  $p_1, p_2, \dots, p_n$  are real; and the following results are obtained.

1. The ordinate of the curve admits of no maximum or minimum value.

2. The curve goes off into infinite branches, which lie in asymptotic planes equally inclined to each other, and which tend alternately to positive and negative infinity.

3. Any plane parallel to the plane of  $xy$  cuts the curve in  $n$  points and no more.

From this last result the existence of  $n$  roots and no more for an equation of  $n$  dimensions is the immediate result.

Several well-known theorems are deduced from this view of the subject, and are given as illustrations.

The actual curves are traced, corresponding to the various cases of the quadratic, the cubic, and the biquadratic equations, and to the equation  $x^2-1=0$ .

In the conclusion of the memoir it is remarked that the results obtained are not exclusively applicable to the case of algebraic equations, and the methods are applied to the case of  $f(x)=\sin x$ .

The author trusts that the contents of this memoir, though not adding to the number of known theorems, may yet be useful as putting the subject in a new light, and as furnishing a method of demonstrating the existence of the roots of algebraic equations more simple and direct than any other which he has seen.



LVIII. *Intelligence and Miscellaneous Articles.*

## ELECTRICAL EXPERIMENTS.

PROF. HENRY, of Princeton, communicated to the American Philosophical Society the *result* of a series of experiments on electricity made last winter. They had reference, first, to the discharge of electricity through a long wire, connected with the earth at the farther end; secondly, to the discharge of a jar through a wire; and thirdly, to an attempt to account for the phenomena of dynamic induction.

Prof. Henry first showed, that when a charge of electricity is given to one end of a wire, the different parts of the wire become charged successively, as though a wave of electricity passed along it. He then showed that the charge passed along the surface of the wire, and not through its whole mass, as was supposed from the analogy of galvanic conduction. Hence he inferred that dynamical electricity obeys the same laws as the statical. He then detailed some experiments upon the passage of electricity through plates, and showed that when a charge was transmitted across a plate, the tension was greatest at the edges, the electricity apparently exercising a self-repelling action, while, if the charge were passed through two pieces of tinfoil, these slips attract each other.

Prof. Henry believes that it may be justly inferred, from these experiments, that the attraction is due to ponderable matter, while the repulsion is due to electricity; thus showing that electricity is a separate principle, and not a mere property of matter.

Prof. Henry next passed to the subject of the discharge of a jar. It was necessary, in his experiments, to get rid of the free electricity arising from the thickness of the glass, and it occurred to him that this might be done by removing the knob, and making the coating upon the inside of less area than that upon the outside. With this arrangement, when the discharge was made through a long wire, and a test jar brought near it during discharge, a bright spark passed; but upon approaching the jar to a delicate electrometer, it gave no indications of free electricity. Reflecting upon this, and upon an experiment of Prof. Wheatstone, he was led to believe that the jar is discharged by two waves, a negative and a positive one, starting simultaneously from the two ends of the wire. To prove this, he broke the wire, and interposed a pane of glass dusted with red lead and sulphur; two figures of positive and negative electricity were produced. He made several other experiments tending to prove this same fact. He showed how these experiments serve to explain that of Dr. Priestley, where a spark was found to pass between the ends of a long bent wire, the ends being brought within a few inches of each other.

He next passed to the connexion between statical and dynamical induction. Statical induction has heretofore only been observed at short distances. Prof. Henry's first experiment proved that it could be observed at the distance of nineteen feet, the floor of a chamber intervening, showing that statical induction takes place at great distances,

though not at so great distances as the dynamical. He then explained his views of the nature of dynamical induction. When a spark is thrown upon a wire, it passes in a wave, whose length might be determined if we knew the velocity of electricity; now, if we have another parallel wire, a negative wave will be formed in this, and the two waves will travel simultaneously in the same direction. But this is equivalent to a positive induced wave in the opposite direction. In this way the phenomena accompanying the discharge of a jar are easily explained. Again, if we conceive that in a galvanic battery the discharge consists of a series of such waves, we may very simply explain the phenomena of galvanic induction.

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ON THE EXAMINATION OF THE ASHES OF ORGANIC BODIES.

BY H. ROSE.

Considerable attention has recently been paid to the analysis of the ashes of plants and animal substances. We now possess a very large number of quantitative analyses of different ashes, which may considerably enlarge our knowledge of the chemical composition of the inorganic constituents of organic substances. Frequently, however, very incorrect conclusions are liable to be drawn from the results of these analyses, as they have hitherto been made, with respect to the constituents in the organic body. This observation struck me on perusing M. Enderlin's paper\*, who found in the ashes of the blood only phosphate of soda ( $3\text{NaO} + \text{P}^2\text{O}^5$ ), phosphate of lime and magnesia, peroxide of iron and perphosphate of iron, sulphate of soda, sulphate of lime, chloride of sodium and potassium, but no alkaline carbonates. From the results found he asserted the total absence of carbonated alkalies and of alkaline salts with organic acids in the blood. It is evident, however, that these conclusions may be erroneous, in so far as at a high temperature the ordinary phosphate of soda ( $2\text{NaO} + \text{HO} + \text{P}^2\text{O}^5$ ) is capable of expelling the carbonic acid from the carbonate of soda and forming basic phosphate of soda.

This observation, respecting the conclusions which M. Enderlin has drawn from his investigations, has already been made by several persons. Marchand† has attempted to refute Enderlin; Berzelius, in his most recent report, likewise draws attention to the subject; and Golding Bird‡ has prepared the basic phosphate of soda ( $3\text{NaO} + \text{P}^2\text{O}^5$ ) by heating the ordinary phosphate of soda ( $2\text{NaO} + \text{HO} + \text{P}^2\text{O}^5$ ) to redness with acetate of soda.

I have also examined the ashes of the blood (ox-blood), but have obtained very different results from those of M. Enderlin. The process I adopted was the following:—I exposed the blood in a covered platinum crucible to a very faint red heat, extracted the cold mass with water, and evaporated the colourless liquid to dryness; it consisted of alkaline chlorides and carbonates, with very minute quantities of alkaline sulphates and phosphates. The charred

\* Chem. Gaz., vol. iii. p. 230.

† *Ibid.*, vol. iv. p. 209.

‡ Phil. Mag., vol. xxvi. p. 532.

mass, extracted with water, was now treated with hydrochloric acid; the filtered solution did not yield with ammonia a very considerable precipitate, which, although it looked almost like pure hydrated oxide of iron, contained some phosphoric acid as well as lime and magnesia. In the filtered solution, I obtained with oxalate of ammonia a pretty considerable precipitate of oxalate of lime, proving the presence of carbonate of lime in the charred blood; and in the liquid separated there was also a small quantity of magnesia. The cinder, after treatment with water and hydrochloric acid, yielded a very considerable quantity of a red-coloured ash on being burnt in an atmosphere of oxygen. It was in a semifused state, and contained peroxide of iron (which formed the chief part), earthy and alkaline phosphates\*.

If the fixed constituents in plants are examined according to the above process, totally different results are frequently obtained to those yielded by the ash analyses which have been published up to this time. The ashes of peas have been examined by Fresenius and Will, Bichon, Thon and Boussingault; none of them enumerate carbonic acid among the constituents. From the investigations which have been made by Drs. Gibbs and Bromeis in my laboratory, the amount of carbonic acid in the salts which are extracted from the charred peas by water amounts to somewhat more than 27 per cent. Evidently, in the former method of reducing to ash, the carbonic acid has been entirely expelled. Phosphoric acid is only present in small quantity in the aqueous extract of charred peas. When the charred peas are subsequently treated with hydrochloric acid, a solution is obtained which contains a moderate quantity of earthy phosphates. If the charred peas are now perfectly reduced to ash in an atmosphere of oxygen, a very considerable quantity of ash is obtained, which consists principally of earthy and alkaline phosphates.

That the method of preparing the ash of organic substances has considerable influence upon the composition of the ash has already been noticed by several chemists, especially by Erdmann†, who showed that acid phosphates, which yielded white precipitates with nitrate of silver by ignition with carbon, lost a considerable portion of the phosphoric acid, and then produced a yellow precipitate in the solution of silver; he observed further, that chlorine and sulphuric acid might be contained in very different quantities in the ashes, according to the mode of preparing them; and that in the ashes of many seeds not a trace of chlorine had been found, while the aqueous extract of the seed contains very perceptible quantities of chloride of sodium. Mitscherlich‡ has likewise drawn attention to several circumstances, by which, in incinerating organic substances, the ashes are frequently decomposed and rendered impure.

The mode of determining the fixed constituents of an organic substance, as above described, appears to me more advantageous

\* After these observations had been penned, I observed that Lehmann had proved the presence of alkaline carbonate in the blood by a different method.— See *Chem. Gaz.*, vol. v. p. 133.

† *Chem. Gaz.*, vol. iv. p. 230.

‡ *Ibid*, vol. iv. p. 69.

and far more rational than the methods usually employed. It may be objected to it, that it takes more time; but not only are far more correct results obtained by it, but it is also capable of answering several questions as to how or in what combinations the constituents found in the ash were contained in the organic substance. According to the method which I propose, the organic substance is charred at a very faint red heat, so that the water with which it is extracted is not coloured yellowish or brownish. At this temperature, which, owing to the volatilization of so many substances, is much lower than would appear, no alkaline chlorides are volatilized, nor can chlorine be expelled from them in the form of hydrochloric acid by acid phosphates. The alkaline and earthy phosphates are not able to expel the carbonic acid from the alkaline carbonates, either contained in the organic substance or formed by the charring; nor can phosphoric acid be eliminated from its combinations by silica, reduced by carbon, and volatilized in the form of phosphorus. The charring is effected either in a spacious covered platinum crucible over a spirit-lamp, or, with larger quantities of the organic substances, in a spacious covered Hessian crucible, especially if they do not fuse. When there is no longer much empyreumatic odour perceptible, the heating is discontinued, the cold mass left for some time in contact with water, and the solution of the soluble salts furthered by heating; the edulcoration requires considerable time and much hot water; but if the highest degree of accuracy is not desired, the edulcoration may be discontinued when several drops of the wash-water leave a scarcely perceptible residue on evaporation upon a slip of platinum; this point is very soon attained.

The aqueous extract contains the alkaline salts. The alkaline chlorides were contained as such in the organic substance previous to the charring, as well as at least a part of the alkaline sulphates and phosphates. If, as in most cases, carbonated alkali is found in the aqueous extract of the charred mass, it either pre-existed in the organic substance, or the alkali in it was combined with an organic acid or some other organic body, which acted the part of an acid towards the alkali. If the organic substance contain sulphate of lime, this, when carbonated alkalies are present in sufficient quantity in the charred mass, is converted, on treating the latter with water, into carbonate of lime and alkaline sulphate. In the same way, when phosphate of lime is present, a certain quantity of alkaline phosphates is formed from it in the aqueous extract by the alkaline carbonates. Carbonated alkali and phosphate of lime are not perfectly decomposed even by fusion at very high temperatures. The decomposition in presence of much water is likewise imperfect, and the more alkaline phosphate is obtained in the aqueous extract, the more concentrated the solution, the more carbonated alkali it contains, and the longer the charred mass has been digested at an elevated temperature. Alkaline sulphates and phosphates will however be found in far smaller quantities in the aqueous extract of the charred mass than was to be expected from the ash analyses that have been hitherto published. Frequently the two, and especially,



the latter, are present only when too high a temperature has been employed in the charring. From this however it is evident that the nature of the salts in the aqueous extract may vary somewhat, according to the temperature employed and the longer or shorter digestion of the mass with water. When the charring is effected at too high a temperature, the greater portion of the carbonated alkalis are decomposed by the earthy phosphates.

The accurate examination of the salts in the aqueous extract is not accompanied with any great difficulties. One circumstance however renders it somewhat less easy; carbonate and phosphate of lime and magnesia frequently dissolve to a considerable extent in neutral solutions of alkaline salts, particularly of alkaline carbonates and phosphates; in the course of time they are deposited from the solutions, especially after the application of heat. When therefore the aqueous extract is evaporated, it frequently becomes somewhat turbid, and deposits small quantities of earthy salts. It should consequently be evaporated nearly to dryness, diluted with water, and the solution set aside for some time; when the earthy salts have subsided, it is filtered, the filtered solution evaporated to dryness, and its weight determined. When there is no alkaline phosphate or sulphate present, the examination is very easy. The quantity of carbonic acid is determined in a suitable apparatus by decomposition with nitric acid; and upon this that of the chlorine by a solution of silver; upon which, after removing the oxide of silver by hydrochloric acid and concentrating the liquid, the potash may be separated from the soda by chloride of platinum. With the presence of alkaline sulphate or phosphate, it is advisable to divide the quantity of the alkaline salts, and in the one half to determine the quantity of the chlorine and the alkalis, and in the other that of the carbonic acid by decomposition with hydrochloric acid; that of the sulphuric acid by a salt of barytes; and after removing the baryta by means of sulphuric acid, and supersaturating with ammonia, to ascertain the quantity of phosphoric acid by means of a solution of a salt of magnesia to which chloride of ammonium has been added.

The charred mass, exhausted with water, is now digested with hot hydrochloric acid for some length of time, and then washed with water. This operation requires considerably more time and water than in the treatment of the charred mass with water; and if the washing were to be continued until some drops of the filtered liquid no longer produced any opalescence in a solution of silver, an enormous length of time, several months, would be required, especially in operating upon large quantities. Theedulcoration therefore is only continued until a considerable quantity of the wash-water does not exhibit a trace of precipitate when treated with ammonia; it will then also be seen that a large quantity of the wash water, when evaporated upon platinum, no longer leaves any perceptible residue: this does not require much time, especially when hot water is used.

The acid solution contains the earthy phosphates which existed as such in the organic substance and the peroxide of iron. It is precipitated by ammonia, and after having determined the weight

of the precipitate, the bases are separated from the phosphoric acid. I shall subsequently describe in these pages a method by which this may be accomplished without any difficulty. On separating the earthy phosphates by means of ammonia, a small quantity remains dissolved in the filtered liquid, owing to the presence of chloride of ammonium; consequently upon adding oxalate of ammonia, a precipitate of oxalate of lime is obtained, but its quantity is usually larger than corresponds to the phosphate of lime dissolved by the chloride of ammonium; consequently a portion of the lime existed as carbonate of lime in the charred mass, or was formed by the decomposition of the sulphate and phosphate of lime by the alkaline carbonates. The liquid filtered from the oxalate of lime indicates, on the addition of a solution of phosphate of soda, the presence of some magnesia. The insoluble earthy salts which separated from the aqueous extract may be examined conjointly with those in the acid extract.

With respect to the charred mass, which has been exhausted with water and hydrochloric acid, it would be imagined that it could contain only silica or silicates undecomposable by dilute hydrochloric acid; but it yields a very large amount of ash on complete combustion, even when the organic substances contain no silica or mere traces.

I formerly effected the perfect combustion of the cinder in hard glass tubes, in which the mass was heated to redness while a current of oxygen was passed over it; this plan however has its inconveniences. If the quantity of organic substance employed is moderately large, so much cinder is obtained, that a glass tube, even of large diameter, would require to be filled with it several times to burn it entirely; moreover, the tube is very much acted upon, and rarely stands a second heating. The combustion is in most cases very imperfect, and requires much time if but a slow current of oxygen be passed; a rapid current is requisite for the combustion to succeed well; but then a considerable quantity of ash may readily be carried away, especially if it is of a very light nature; this, it is true, may be partially prevented by passing the gas on its exit through a stratum of water, by varying the height of which a different pressure may be obtained; but this does not entirely prevent the removal of the ash, and if the pressure is too great, the glass tube is liable to bulge when strongly heated\*. But the greatest disadvantage attending the use of glass tubes is, that it is impossible to collect the ashes so as to determine their weight accurately. Frequently the ash has undergone slight fusion; it can then only be imperfectly separated from the glass by mechanical means or the use of solvents. The use of thin platinum or silver foil, which is introduced into the glass tube previous to the coal being placed in it, does not entirely remove these serious disadvantages, and gives rise to others. I therefore employ for the combustion of the charred mass a different method, with the results of which I am perfectly

\* The water employed for this purpose contains cyanogen compounds when the charcoal is derived from nitrogenous bodies.

satisfied. For some time past I have no longer employed bulb tubes in reductions by means of hydrogen gas, but place the substance to be reduced in a platinum or porcelain crucible, provided with a platinum lid, which is perforated in the centre, into which passes a curved silver tube about 8 inches long, through which the dried hydrogen is conveyed into the crucible. During ignition the gas escapes between the lid and crucible. Ebelmen\* has recently employed a similar apparatus for the reduction of the sesquioxide of manganese to the state of protoxide. This apparatus can be used with considerable advantage for burning the charred mass. The crucible is half-filled with the substance, and heated over a spirit-lamp, while a current of oxygen is passed into it; with proper care not a particle of ash is carried away, and the combustion proceeds very rapidly; a further quantity of the substance is conveyed from time to time into the crucible. By this means the ash obtained may be weighed with such accuracy as would not be easily accomplished in any other way.

When the combustion is effected in a platinum crucible, this is sometimes acted upon; a silver crucible is liable to partial fusion from the heat during the combustion; it is therefore advisable to employ a porcelain crucible, which is acted upon far less than glass; and if very thin and somewhat transparent, the progress of the combustion may be distinctly observed by the incandescence.

The weight of the ash obtained, added to that of the evaporated aqueous extract of the charred mass and to that of the insoluble earthy salts dissolved by the hydrochloric acid, gives the correct quantity of fixed constituents in the organic substance employed.

The ash obtained, especially when derived from vegetable substances, consists of the same constituents as were found in the aqueous and acid extracts; if alkalies were present in them, we likewise find them in the ash of the exhausted charred substance; otherwise it consists principally of earthy phosphates. I have already mentioned that nearly the whole of the iron of the blood is met with in this ash. Only about the tenth part of it is found in the acid extract of the charred mass, and indeed the less the more carefully the charring was effected with exclusion of the air. When the organic substance contains no silica, various views may be entertained respecting the origin of the ash from the charred mass which has been exhausted with water and acid. The most probable is perhaps to derive it from an imperfect exhaustion with the two solvents. When an organic substance is destroyed by heat, the charcoal formed may contain such cavities that the inorganic salts surrounded by them are protected from the action of the solvents. The globules of the blood, those of yeast, the cells of plants, form perhaps after charring extremely minute vesicles, with such small apertures that no liquid can penetrate into them. That the vessels of wood are capable of forming extremely thin filaments with minute apertures by charring is known from the investigations of Degen†. The charred mass of an organic substance (yeast), after it had been most carefully ex-

\* Chem. Gaz., vol. i. p. 685.

† Poggendorff's Annalen, vol. xxxv. p. 466.



hausted by water and hydrochloric acid, was ground to the very finest powder upon a plate of agate; the two solvents now extracted only imperceptible traces of fixed constituents, and after burning the exhausted charred mass I obtained the same large amount of ash as from the non-pulverized charcoal. It may nevertheless be supposed that the extremely minute vesicles were not destroyed and torn by the friction upon the agate plate. The microscope threw no light upon the subject.

It is known that charcoal is capable, by a weak kind of affinity, of removing certain salts from their solutions; I have also mentioned above, that when the charred substance has been treated with hydrochloric acid, it is almost impossible to remove the acid by washing with water; but the quantity of ash is too considerable for us to ascribe this origin to it, since it is known that the salts which the charcoal has combined with may be entirely separated by long treatment with water at different temperatures. I mixed sulphate of potash and phosphate of lime with sugar, and charred the mixture. Water, and after this hydrochloric acid, extracted the two salts so completely from the charred mass, that the latter, after combustion in an atmosphere of oxygen, left not a trace of ash.

Several acid phosphates (metaphosphates) are insoluble in hydrochloric acid after ignition; to this cause might be owing the residuary ash of the charred mass; but although phosphoric acid is almost constantly met with in this ash, it is not always combined with the bases in the form of acid salts; moreover, the ash itself, when it does not contain silica, is soluble in hydrochloric acid after the salts have been exposed, in the combustion of the carbon in oxygen, to a far higher temperature than that employed in charring the organic substance.

Ignited peroxide of iron is, it is true, not insoluble in hydrochloric acid, although very sparingly soluble; its non-extraction by acid from the charred blood might be attributed to this cause; but supposing it to be contained in the state of peroxide of iron, not only must it be more readily soluble in hydrochloric acid, owing to its finely divided state, but the oxide of iron which is found in the ash after the combustion of the coal, and which has been exposed to a very high temperature, is soluble in hydrochloric acid.

The alkaline and earthy phosphates which are found in the ash of the exhausted charred mass cannot have been contained in the form of metallic phosphurets of a peculiar kind mixed with coal, like the phosphuret of iron, which is perfectly insoluble in hydrochloric acid. It might be assumed that they were formed by the reduction of the alkaline and earthy phosphates by the carbon. It is scarcely necessary to notice this assumption, for it is well known that the neutral and basic alkaline and earthy phosphates are not reduced by carbon even at a high temperature, at least not in the presence of silica or any similar fixed acid. But even the excess of phosphoric acid cannot be reduced by carbon from the acid phosphates at the temperature which I employ in charring organic substances. In the case of the blood, however, phosphuret of iron



might be formed by the reduction of the phosphate of iron by the carbon. Although I was convinced that this could not happen at the temperature which I employed, I mixed phosphate of iron intimately with sugar, and charred the mixture at a higher temperature; hydrochloric acid, however, extracted the salt so completely from the charred mass, that this burnt without any residue in an atmosphere of oxygen.

It might be supposed that the salts found in the ash of the exhausted charred substances are so intimately combined with organic substances that they can only be detected by reagents after the complete destruction of the latter. But the organic substances are so destroyed by the charring, that if the inorganic salts found in the ash pre-existed as such in them, they should have been extracted by those agents in which they are soluble.

There still remains one view respecting the origin of these ashes. The salts found in them may perhaps not have pre-existed as such in the organic substances, but were first formed by oxidation after the burning of the coal. It has long been known that the proteine compounds, of both animal and vegetable origin, contain sulphur and phosphorus in an unoxidized state; but, as far as I am aware, the supposition has never been advanced, that the radicals of the earths and alkalies may likewise be contained in organic substances in an unoxidized state, perhaps combined with those elements. These would certainly constitute a very peculiar class of combinations, such as we are at present not acquainted with. If they are really combined with organic substances in the living body, they cannot have been essentially altered on destroying the organic body by charring, or they have entered into combinations with carbon and nitrogen, which are insoluble in water and in hydrochloric acid.

I have already observed that the salts found in the ash of the charred mass exhausted with water and acid, especially when derived from vegetable substances, are similar to those which occur in the aqueous and acid extract. This view can only be confirmed by a long series of investigations; but if it should be confirmed, then those salts which we find in the ash after the destruction of the living plants were probably contained in them only in part as such, and in part in an unoxidized state. The inorganic salts, therefore, which are taken up from the soil by the living plant, are partially deoxidized by it, and in this state form combinations with organic substances contained in the plant.

This view is far more probable with respect to several animal substances, especially the blood, than in reference to plants. It has long been suspected that the iron in the blood was contained in it in an unoxidized state; and, according to the recent investigations of Mulder, the iron is actually extracted by acids from hæmatine with evolution of hydrogen gas. This view acquires still greater probability from the experiments which I have related. On the other hand, it is very remarkable that the iron cannot be extracted from the charred blood by hydrochloric acid. I think it would be

extremely interesting to pursue this subject further. From what has been stated, it results that the view which I have advanced can neither be confirmed or refuted by any number whatever of analyses of the ashes of plants, which have been prepared according to the methods hitherto in use, but only by a process similar to that I have described, and which I have therefore pronounced a far more rational method of investigation.

I have already shown that frequently very considerable quantities of alkaline carbonates are extracted by water from several organic substances after charring, in the ashes of which no carbonic acid was found by former investigators. But all organic substances do not yield alkaline carbonates when treated in this manner, even though considerable quantities of alkali are contained in their ash. Highly remarkable in this respect is yeast, the ash of which, according to Mitscherlich \*, contains no carbonic acid and no metallic chlorides; and my experiments show that they are likewise not to be found in the aqueous extract of the charred yeast. Yeast diffuses, on being charred, an odour similar to that of the proteine compounds; the aqueous extract did not turn litmus-paper blue, became turbid on evaporation, and deposited a large quantity of earthy phosphates. The mass, evaporated to dryness, yielded on filtration a clear solution, which faintly reddened litmus-paper, and contained therefore not a trace of alkaline carbonates; I could only find in it alkaline phosphates, with very minute traces of alkaline sulphates and chlorides. The charred mass gave, on treatment with hydrochloric acid, a solution, from which ammonia threw down a considerable precipitate of earthy phosphates. The cinder, exhausted with water and acid, furnished on combustion a very large quantity of ash, which contained the same constituents which had been extracted from the charred mass. These experiments, the results of which entirely agree with those obtained by Mitscherlich, were made with top-yeast, which had been perfectly purified by washing. Other results may perhaps be obtained by using unwashed yeast for the experiments; for the beery liquid, separated from the yeast by filtration and evaporated, did not diffuse on charring the same disagreeable odour as the yeast itself, or as nitrogenous substances generally at a high temperature. The evaporated solution, extracted with water from the charred mass, contained a large amount of carbonated alkali, and effervesced therefore strongly with acids, with much chloride of potassium, but only a little phosphate of potash. The charred mass, exhausted with water and hydrochloric acid, yielded a tolerable quantity of a light ash containing phosphates and a large quantity of silica, which latter element Mitscherlich likewise found in the ash of beer †.

\* Chem. Gaz., vol. iv. p. 69.

† After this paper had been read before the Royal Academy of Berlin, I received a letter from Berzelius, to whom I had communicated the principal results, in which he states that he advanced a view similar to that which I have proposed on the ashes of the blood, more than 40 years ago, in his work on animal chemistry. It occurs also in Schweigger's '*Journal für Chemie und Physik*,' vol. ix. p. 391, 1813. It has however never been expressed in the various editions of Berzelius's '*Manual of Chemistry*.'

## SUGAR IN HEALTHY BLOOD.

In the *Phil. Mag.* for May 1845, p. 422, are detailed some experiments by Dr. R. D. Thomson, which show that when starch has been digested in considerable quantities by animals, it passes into the condition of soluble starch, or dextrine and sugar, and being absorbed in the latter form into the blood, can be detected in that fluid during the period of digestion. The experiments detailed in the paper referred to were made in 1844. Magendie has lately, in a paper communicated to the French Academy (*Comptes Rendus*, xxiii. p. 189), obtained similar results. He found that when a dog was fed on cooked potatoes, the blood contained dextrine and grape-sugar. He observed also, that if starch be mixed with fresh serum, it is so transformed in a few seconds that it cannot be detected by reagents, and in a quarter of an hour sugar makes its appearance. This exactly corresponds with the previous results obtained by Dr. Thomson in 1844, who "was unable to detect any traces of starch in the serum of the blood" (*Phil. Mag.*, May 1845, p. 420), but easily obtained evidence of the presence of sugar in the same blood.

## CHEMICAL ACTION OF LIGHT.

*To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,

The chemical action of light has of late attracted so much attention, that any fact connected with the subject will be received with interest by those engaged in researches on this branch of science, which induces me to ask the favour of your publication of the following.

I have been successful in obtaining well-defined photographic impressions on highly sensitive Daguerreotype plates, on which the object, when illuminated by a common *dip* candle, was impressed in ten minutes; with the smallest fish-tail burner of coal gas in three minutes; and by the oil lamp, viz. a solar lamp, in the same time. I included each flame in the picture by which they have recorded their size, and to some extent their illuminating power.

I at present confine myself to the recital of these facts, as I am following up my researches on the subject. I beg to forward the plates for your inspection\*.

I am, Gentlemen,

234 Regent Street,  
April 20.

Your most obedient Servant,  
W. E. KILBURN.

## ON THE COMPOUNDS OF CYANOGEN. BY M. AD. WURTZ.

*Formation of Cyanuric Acid.*—When a current of dry chlorine gas is passed over fused urea, very energetic reaction occurs; the urea is decomposed, swelling up and emitting abundant white vapours. The products of this action are cyanuric acid, hydrochlorate of am-

\* These we have received. They appear to us very perfect, and the effect highly remarkable.—Ed.

monia, hydrochloric acid and azote. To separate the cyanuric acid, it is sufficient to treat the cooled mass, which remains in the vessel in which the decomposition was performed, with a little cold water. The hydrochlorate of ammonia dissolves in the water, and the cyanuric acid remains in the form of a white powder, which, if requisite, may be purified by solution in boiling water. The following equation explains the reaction of the chlorine upon the urea :

$3 (C^2 H^4 Az^3 O^2) + 3 Cl = Az + H Cl + 2 H^4 Az Cl + C^6 Az^3 H^3 O^6$ .  
The author considers this as the most convenient process for preparing cyanuric acid.

*Chlorohydruret [hydrochlorate?] of Cyanogen.*—When a current of chlorine gas is passed into a solution of hydrocyanic acid, prepared by Trautwein's process, a slight increase of temperature occurs after the operation has been some time continued. The liquid emits a very perceptible odour of chloride of cyanogen, and a vapour is formed which condenses in small drops in the cold parts of the apparatus, and these constitute the principal product of the reaction: to collect it, a tube with chloride of calcium is to be adapted to the retort which contains the hydrocyanic acid, bent at a right angle, and passed into a long-necked receiver cooled by ice.

When the operation is finished, the receiver contains a liquid which is limpid, very volatile, fumes on exposure to the air, and emits a very irritating odour of chloride of cyanogen: this is impure chlorohydruret of cyanogen. It contains hydrochloric and hydrocyanic acids, from which it is freed by agitating it with two or three times its volume of cold water. The stratum of liquid which separates from the water is to be poured off, and submitted to a fresh distillation, its vapour being passed through a tube containing chloride of calcium.

The chlorohydruret of cyanogen thus prepared is a colourless, very fluid and corrosive liquid. It emits an odour which strongly irritates the bronchiæ, and especially the eyes. It boils at 68° F. Its vapour burns with a violet flame. It dissolves sensibly in water, and the solution precipitates nitrate of silver white.

When brought into contact with dry chlorine, it is entirely converted into solid chloride of cyanogen and hydrochloric acid,  $C^6 Az^3 Cl^2 H + Cl = H Cl + C^6 Az^3 Cl^3$ .

If this reaction takes place with some grammes of the substance, the sides of the vessel will be covered the next day with fine radiating needles, and there remains at the bottom a viscid liquid, which eventually becomes a mass of large crystals of chloride of cyanogen.

M. Wurtz states that he had some difficulty in analysing the chlorohydruret of cyanogen. This will readily be conceived, when the extreme volatility of this liquid is considered. The experiments performed by the author led to the formula  $C^6 Az^3 Cl^2 H$ , and this appears to be confirmed by the reactions which the substance undergoes.

This substance may therefore be considered as a compound of hydrocyanic acid with chloride of cyanogen,  $C^4 Az^2 Cl^2$ , the description of which will presently be given; or it may be regarded as solid



chloride of cyanogen,  $C^6 Az^3 Cl^3$ , in which one equivalent of chlorine is replaced by one equivalent of hydrogen.

*Liquid Chloride of Cyanogen* is prepared by treating the chlorohydruret of cyanogen with binoxide of mercury. In order to avoid too violent reaction, it is proper to mix this oxide with fused and powdered chloride of calcium, and to render the mixture very cold. After remaining for some hours, the mixture is to be distilled by means of a water-bath, and in the receiver, properly cooled, a colourless liquid condenses, which is a new modification of chloride of cyanogen.

It is a limpid liquid, which strongly irritates the mucous membrane of the bronchiæ, and occasions the eyes to water. It is more dense than water. It boils at  $61^\circ F.$ , and at  $20^\circ F.$  becomes a solid mass of long crystalline laminæ. Its vapour is incombustible. It falls to the bottom of a vessel of water, but is sensibly soluble in it. The solution does not precipitate nitrate of silver. The reactions described seem to indicate that the alkalies decompose the chloride of cyanogen into alkaline chloride, ammonia and carbonic acid. It is well-known that the two latter bodies are products of the decomposition of cyanic acid. Analyses performed by the author led him to conclude that the composition of liquid chloride of cyanogen is expressed by the formula  $C^4 Az^3 Cl^2$ . It appears therefore to be a new isomeric of chloride of cyanogen.—*Comptes Rendus*, Mars 11, 1847.

#### RESEARCHES ON HYDRATES, STANNIC AND METASTANNIC ACIDS. BY M. E. FREMY.

The author inquires whether it is to be generally admitted, that all acids which, by becoming anhydrous, have lost their property of combining with bases, are no longer to be considered as acids. In order to determine the acidity of anhydrous acids, almost all such as are known were made to act upon anhydrous bases, or on salts containing acids less fixed than those employed; the acids used were the carbonic, sulphurous, sulphuric, phosphoric, silicic, boracic, stannic, &c. These were found to possess all the characters of acids, without the intervention of water: thus, to cite one example, it was found that perfectly dried sulphurous acid completely decomposed carbonate of soda, when gently heated, forming sulphite of soda, which was decomposable by an anhydrous and more fixed acid.

It being established that a certain number of acids preserve their acid reaction when rendered anhydrous, the author examined whether certain salts did not exist in which water was indispensable; and he is of opinion that several classes of salts exist, the molecules of which are unquestionably ternary, and always formed by the combination of an acid, a base and water.

Supposing that if energetic acids, such as the sulphuric and nitric, form with equally energetic bases, salts which are almost always anhydrous, it appeared questionable whether the same would occur with acids whose affinity for basic water is but slight. The author's attention was directed to those hydrated metallic oxides which are soluble

in the alkalies, and which may be considered as weak acids. It was found that these bodies possess the property of combining with bases in the state of hydrates only. Thus, hydrated binoxide of copper dried in the air, the formula of which is  $\text{Cu O}^2\text{HO}$ , is perfectly soluble in excess of potash and soda; the solution is of a fine blue colour, and when evaporated, even *in vacuo*, it is decomposed, and deposits oxide of copper, which is insoluble in the alkalies. The hydrates of oxides of tin, antimony and chromium,  $\text{SnO}$ ,  $\text{HO}$ ;  $\text{Sb O}^3$ ,  $\text{HO}$ ;  $\text{Cr}^2\text{ O}^3\text{ 10HO}$ , dissolve in the alkalies, and all become insoluble by losing water. The author satisfied himself that the insolubility was owing to the separation of the water, and not to the isomeric modifications which certain oxides undergo by calcination.

It was found impossible to obtain crystalline compounds of the above-mentioned hydrates with the alkalies; for by evaporation, even *in vacuo*, the alkali which it is requisite to employ in excess combines with the water of the hydrates, or in acting perhaps only by its presence occasions the precipitation of the anhydrous oxide.

The preceding compounds yield, however, incontestable proof of the existence of saline groups, which are not possible without the presence of water: they demonstrate that certain hydrates owe their solubility in the alkalies entirely to water of hydration.

The new facts stated establish marked differences between stannic acid and the acid which M. Fremy calls *metastannic acid*; and this he has found forms three different hydrates with water: the first is insoluble in nitric acid and soluble in ammonia; it is obtained by precipitating a metastannate by an acid; the second is produced by the reaction of nitric acid upon tin; its formula is  $\text{Sn}^5\text{ O}^{10}$ ,  $10\text{HO}$ ; it is insoluble in ammonia and nitric acid; the third is prepared by drying the preceding hydrate at  $266^\circ\text{ F.}$ ; its formula is  $\text{Sn}^5\text{ O}^{10}$ ,  $4\text{HO}$ .

Neutral metastannates, formed in the presence of great excess of alkali, have for their formula  $\text{Sn}^5\text{ O}^{10}$ ,  $\text{MO}$ ,  $4\text{HO}$ , whereas stannates are represented by  $\text{Sn O}^2\text{ MO}$ . The equivalent of metastannic acid is then five times greater than that of stannic acid. The metastannates are always hydrated, are necessarily ternary, and decompose when dehydrated: in this case the metastannic acid loses its acidity, whereas stannates, like most other salts, may be rendered anhydrous without decomposing.

The essential action of water on the constitution of the metastannates, may be readily shown by the following experiments. If metastannate of potash be gently heated, so as to take away its water of combination, the separation of the acid and base is immediately effected: on treating the dehydrated salt with water, potash only is dissolved, which does not contain a trace of metastannic acid, and this acid remains in an insoluble state. In this case, therefore, a decomposition is effected upon a definite salt, similar to that which the binoxide of copper undergoes in potash when heated: these two phenomena are precisely of the same description.

The dehydration of metastannate of soda is perhaps still more remarkable, for it takes place below  $212^\circ\text{ F.}$  This salt is soluble in

cold water; but if it be added to boiling water, it decomposes instantaneously, and the water contains pure soda only.

The author is of opinion that he has stated an important fact, which is, that the same oxide, by combining with different proportions of water, can form two acids which are distinguished by their properties, their equivalent, and their mode of combining with bases. —*Journ. de Pharm. et de Ch.*, Mars 1847.

#### ON SULPHOXIPHOSVINIC ACID AND ITS COMPOUNDS.

BY M. CLOEZ.

The author remarks that he and M. Bouyet had described a new kind of salts, the acid of which they designated sulphoxiarsenic acid, consisting of arsenic, oxygen and sulphur, water being deducted. The sulphoxiarsenate of potash, which was obtained by passing sulphuretted hydrogen into a solution of arseniate of potash, has for its formula  $\text{As O}^3 \text{S}^2, \text{KO} + 2\text{HO}$ .

The great analogy existing between the corresponding compounds of arsenic and phosphorus, rendered it probable that sulphoxiphosphates might also be formed. After many trials, M. Cloez succeeded in producing these salts by decomposing the chlorosulphuret of phosphorus of Serullas by an alkaline solution.

The chlorosulphuret of phosphorus, treated with common alcohol, yields an acid analogous to the phosphovinic acid of M. Pelouze. According to M. Cloez the formula of this acid is  $\text{Ph O}^3 \text{S}^2, \text{C}^4 \text{H}^5 \text{O}, 2(\text{HO})$ ; it is the sulphoxiphosphovinic acid. With pyroxylic spirit sulphoxiphosphomethylic acid is formed. Amylic alcohol should produce the corresponding compound, sulphoxiphosphamylic acid.

By saturating the sulphoxiphosphovinic acid with the carbonate of barytes, lime, strontia, &c., well-defined crystalline salts are produced: the formula of the barytic salt is  $\text{Ph O}^3 \text{S}^2, \text{C}^4 \text{H}^6 \text{O}, 2\text{BaO} + \text{Aq}$ .

The potash and soda salts are readily obtained by decomposing the chlorosulphuret of phosphorus with an alcoholic solution of potash or soda. These salts are soluble in alcohol. The following equation explains the reaction which occurs with potash:  $\text{Ph Cl}^3 \text{S}^2 + \text{C}^4 \text{H}^6 \text{O}^3 + 5(\text{KO}) = \text{Ph O}^3 \text{S}^2, \text{C}^4 \text{H}^5 \text{O} 2(\text{KO}) + 3(\text{KCl}) + \text{HO}$ . — *Comptes Rendus*, Mars 8, 1847.

#### ON ANHYDROUS HYDROFLUORIC ACID.

M. Louyet states that in 1846 he mentioned to the Academy that, in his opinion, anhydrous hydrofluoric acid was as yet unknown; in proof of its correctness, he showed that the acid supposed to be so, saturated a smaller quantity of a base than it ought to do, if it had been anhydrous.

This proof not having satisfied M. Dumas, the author was advised by him to pass hydrofluoric acid, hitherto regarded as anhydrous, over anhydrous phosphoric acid, and to collect the product in a platina vessel immersed in a freezing mixture. According to M. Dumas, if by this process a liquid hydrofluoric acid was always ob-

tained, it would be necessary to admit that the hydrofluoric acid, supposed by MM. Gay-Lussac and Thenard to be anhydrous, was really so.

M. Louyet performed this experiment by the aid of an apparatus which he describes, and the result of it is, that anhydrous hydrofluoric acid is gaseous at  $10^{\circ}$  F. at a medium pressure. In this respect it does not differ from other anhydrous hydracids, nor from the fluosilicic nor fluoboric acids. Hydrofluoric acid gas is extremely fuming, on account of its great affinity for water. It acts feebly on glass, and might probably be received in glass vessels over mercury.—*Ibid.* Mars 11, 1847.

### METEOROLOGICAL OBSERVATIONS FOR MARCH 1847.

*Chiswick.*—March 1.—3. Cloudy. 4. Fine: cloudy. 5, 6. Cloudy. 7. Slight showers: uniformly overcast: clear. 8. Small rain: cloudy. 9. Cloudy. 10. Hazy: severe frost at night. 11. Slight haze: cloudy: frosty. 12. Clear. 13. Cloudy. 14—18. Clear, with bright sun. 19. Clear: cloudy. 20, 21. Cloudy: clear. 22. Hazy. 23, 24. Cloudy. 25. Foggy: clear: cloudy. 26. Slight haze: fine. 27. Foggy: fine. 28. Uniformly overcast: rain. 29. Very clear: cloudy: frosty. 30. Clear: cloudy: clear and frosty. 31. Frosty: cloudy.

Mean temperature of the month .....  $40^{\circ}\cdot 14$

Mean temperature of March 1846 .....  $43^{\circ}\cdot 43$

Mean temperature of March for the last twenty years...  $42^{\circ}\cdot 91$

Average amount of rain in March .....  $1\cdot 36$  inch.

*Boston.*—March 1—5. Cloudy. 6. Fine. 7. Fine: rain early A.M.: rain P.M. 8. Cloudy. 9. Fine: snow A.M. and P.M. 10. Cloudy: hail and snow P.M. 11—13. Fine. 14. Cloudy. 15—18. Fine. 19. Fine: a luminous appearance of an extraordinary nature seen in the sky 9 P.M. 20, 21. Cloudy. 22. Fine. 23. Fine: rain P.M. 24, 25. Fine. 26, 27. Cloudy. 28. Cloudy: rain A.M. and P.M. 29. Fine: thick ice this morning: hail A.M.: rain P.M. 30. Fine: hail P.M. 31. Fine.

*Sandwich Manse, Orkney.*—March 1. Cloudy: clear. 2. Bright: clear. 3. Showers: clear. 4, 5. Cloudy. 6. Showers: cloudy. 7. Damp: cloudy. 8. Showers: snow-showers. 9. Snow-showers. 10. Snow: cloudy. 11. Showers: sleet-showers. 12. Showers. 13. Cloudy: showers. 14. Bright: cloudy. 15, 16. Cloudy. 17, 18. Bright: cloudy. 19, 20. Bright: clear: aurora. 21. Bright: clear. 22. Bright: large halo. 23. Damp: cloudy. 24. Damp. 25. Cloudy. 26. Damp: cloudy. 27. Cloudy. 28. Hail-showers: sleet-showers. 29. Shower: clear. 30, 31. Snow-shower.

*Applegarth Manse, Dumfriesshire.*—March 1. Thaw: slight rain. 2. Slight frost: very mild. 3. No frost: gray day. 4. Frost, slight. 5. No frost: clear and fine. 6. No frost: dull and cloudy. 7. No frost. 8. Slight frost: clear. 9. Frost, hard. 10. Frost, hard: sprinkling of snow. 11. Frost: sprinkling of rain P.M. 12. Frost again. 13. Fair and moderate weather. 14. Fair, but threatening: change. 15. Fair still, but cloudy. 16. Fair: rain P.M. 17. Rain all day. 18. Very fine: fair. 19. Very fine. 20. Rain: cleared P.M. 21. Heavy showers. 22. Fine: a few drops of rain. 23. Rain A.M.: thunder P.M. 24. Frosty: fine. 25. Slight frost. 26. Rain. 27. Very fine: rain A.M. 28. Clear and cold: slight snow. 29. Fair: cold: slight frost. 30. Fair: slight frost. 31. Slight snow: hail: frost.

Mean temperature of the month .....  $42^{\circ}\cdot 5$

Mean temperature of March 1846 .....  $42^{\circ}\cdot 2$

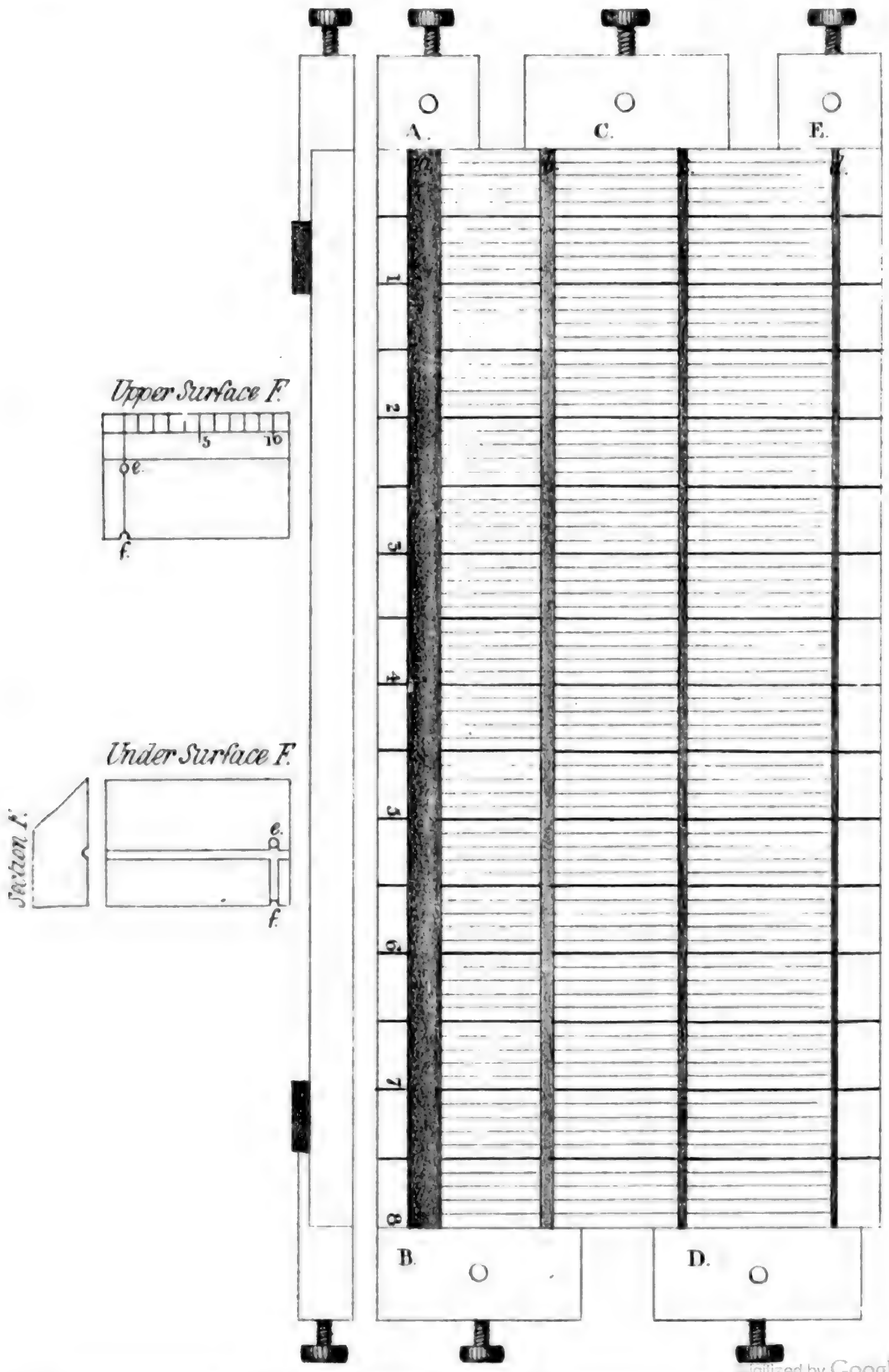
Mean temperature of March for 25 years .....  $39^{\circ}\cdot 1$

Mean rain in March for 20 years .....  $2\cdot 35$  inches.



*Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at Chiswick, near London; by Mr. Veall, at Boston; by the Rev. W. Dunbar, at Applegarth Manse, Dumfries-shire; and by the Rev. C. Clouston, at Sandwick Manse, Orkney.*

Days of Month.	Barometer.						Thermometer.						Wind.				Rain.			
	Chiswick.			Dumfries-shire.			Orkney, Sandwick.			Chiswick.		Dumfries-shire.		Boston.	Dumfries-shire.	Orkney, Sandwick.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.
	Max.	Min.	8½ a.m.	9 a.m.	9 p.m.	94 a.m.	84 p.m.	Max.	Min.	8½ a.m.	Max.									
												1847.	March.							
1.	30.384	30.275	30.05	30.35	30.40	30.52	30.56	37	24	45	36	40	38	calm	e.	calm	.....	.....	.....	.....
2.	30.508	30.417	30.16	30.49	30.50	30.57	30.60	37	37	45	35	43	42	calm	ne.	calm	.....	.....	.....	.....
3.	30.491	30.488	30.19	30.51	30.48	30.59	30.57	41	24	45½	37½	43½	41	calm	ne.	calm	.....	.....	.....	.....
4.	30.474	30.375	30.12	30.45	30.40	30.56	30.55	37	37	45	33	43	42½	calm	ne.	calm	.....	.....	.....	.....
5.	30.265	30.250	29.93	30.36	30.33	30.53	30.50	43	34	48	37½	43½	40	n.	ne.	calm	.....	.....	.....	.....
6.	30.170	30.022	29.85	30.23	30.12	30.36	30.30	43	34	46½	35½	45	42½	calm	nne.	calm	.....	.....	.....	.....
7.	30.113	29.968	29.69	30.09	30.13	30.25	30.22	42	32	49½	36½	46	44	ne.	n.	calm	.....	.....	.....	.....
8.	30.113	30.029	29.74	30.08	29.98	30.12	30.08	42	35	50	35	41	32½	n.	n.	calm	.....	.....	.....	.....
9.	30.027	29.986	29.65	29.97	29.92	30.06	30.05	37	22	43	34	33	30	nw.	n.	calm	.....	.....	.....	.....
10.	30.127	29.931	29.66	29.90	30.10	30.16	30.08	31	27	40	28	30	30	calm	ne.	calm	.....	.....	.....	.....
11.	30.218	30.140	29.95	30.00	29.75	29.62	29.83	31	26	41½	21½	39	36	calm	w.	calm	.....	.....	.....	.....
12.	30.234	30.041	29.72	30.00	30.02	30.05	29.91	37	25	45	30½	38	41	calm	nw.	calm	.....	.....	.....	.....
13.	30.315	30.027	29.84	30.00	30.08	29.99	29.97	40	25	49	37	40½	45	w.	w.	calm	.....	.....	.....	.....
14.	30.345	30.290	29.94	30.05	29.95	29.93	29.71	42½	24	46½	40	47	45	calm	e.	calm	.....	.....	.....	.....
15.	30.206	30.005	29.76	29.83	29.63	29.63	29.48	37	34	57½	39½	48	49	sw.	s.-se.	calm	.....	.....	.....	.....
16.	29.904	29.795	29.53	29.53	29.40	29.45	29.37	45	42	60	38	49	45	sw.	se.	sw.	.....	.....	.....	.....
17.	29.832	29.794	29.44	29.50	29.60	29.53	29.64	48	26	52½	46½	47½	49	sw.	se.	sw.	.....	.....	.....	.....
18.	29.848	29.825	29.49	29.65	29.63	29.74	29.66	46	27	61	43	50	47	calm	ssw.	calm	.....	.....	.....	.....
19.	29.729	29.489	29.38	29.50	29.30	29.60	29.46	47	37	60½	44	49½	43½	calm	se.	calm	.....	.....	.....	.....
20.	29.542	29.493	29.10	29.17	29.22	29.27	29.34	50	41	54	45	45½	43	sw.	se.	sw.	.....	.....	.....	.....
21.	29.659	29.524	29.16	29.25	29.50	29.43	29.48	48	26	52	44	47	43	sw.	se.-s.	calm	.....	.....	.....	.....
22.	29.843	29.715	29.42	29.70	29.70	29.77	29.85	42½	34	57	37½	49	42	e.	ssw.	calm	.....	.....	.....	.....
23.	29.810	29.729	29.42	29.58	29.50	29.81	29.79	48	30	52	43	43	43	s.	sse.	calm	.....	.....	.....	.....
24.	29.919	29.771	29.38	29.62	29.79	29.84	29.96	46	23	51	33	45	43	s.	nne.	calm	.....	.....	.....	.....
25.	30.011	29.990	29.36	29.88	29.88	30.04	30.06	45	39	52	34½	42½	41½	calm	e.	calm	.....	.....	.....	.....
26.	30.017	29.969	29.64	29.85	29.83	30.07	30.04	43	31	49	40	41½	41½	se.	e.	calm	.....	.....	.....	.....
27.	29.996	29.808	29.60	29.84	29.74	30.00	29.91	47	39	56½	43½	43½	42	calm	ne.	calm	.....	.....	.....	.....
28.	29.604	29.483	29.27	29.60	29.64	29.84	29.77	48	30	47½	41½	37	40	s.	ne.	calm	.....	.....	.....	.....
29.	29.760	29.679	29.37	29.67	29.63	29.84	29.82	38	26	49	31	40	36	nw.	n.	nw.	.....	.....	.....	.....
30.	29.712	29.621	29.34	29.62	29.50	29.71	29.68	38	20	45½	31	38½	37	w.	n.	nw.	.....	.....	.....	.....
31.	29.563	29.446	29.20	29.45	29.30	29.53	29.37	36	32	45	30	34	33½	ne.	n.	nw.	.....	.....	.....	.....
Mean.	30.023	29.915	29.00	29.868	29.837	29.948	29.922	41.7	29.77	49.7	36.8	42.67	40.91				0.41	0.94	1.27	1.56



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LIX. *On the Use of a double Circuit in the measurement of Electrical Forces.* By ISAAC B. COOKE, Esq.\*

[With a Plate.]

ACCORDING to the theory of Ohm, the electric force circulating through each part of a galvanic circuit is equal to the *quotient* of the *sum* of the affinities in action divided by the *sum* of the resistances to be overcome. Thus

$$F = \frac{A}{R + r},$$

where  $F$  represents the force,  $A$  the sum of the affinities generating the current,  $R$  the resistance of the electromotor and the wires attached, and  $r$  the resistance of the wire of a galvanometer included in the circuit.

If a secondary wire, whose resistance is represented by  $w$ , have its ends attached to the two points of connexion between the wires of the electromotor and those of the galvanometer, so as to form with the galvanometer-helix a divided course for the circulation of the current, the equation for the force becomes

$$F = \frac{A}{R + L};$$

where

$$L = \frac{rw}{r + w}.$$

The magnitude of that part of the current passing through the galvanometer wire is expressed by

$$G = \frac{A}{R + L} \cdot \frac{L}{r};$$

\* Communicated by the Author.

386 Mr. I. B. Cooke on the Use of a double Circuit in  
or, on substitution,

$$G = F \cdot \frac{w}{r+w}.$$

Let one of the wires of the electromotor be detached from its connexion with the other wires, and drawn along in contact with the secondary wire, thus modifying the relative length, and of course, in the same proportion, the relative resistances of the two courses open to the current, but leaving the sum of the resistances constant. The above equation will become

$$G' = F' \cdot \frac{w'}{r+w}.$$

If the electromotor be not very weak, and the galvanometer be sensitive, in order to reduce the current passing through the galvanometer to a magnitude within the compass of its indications, the secondary wire will require to be short and stout, making the resistance  $w$  very small, and consequently  $L$  very small in comparison with  $R$ ;  $F$  will then vary very slightly by the relative changes thus caused in  $r$  and  $w$ , and for practical purposes  $F$ , as well as  $r+w$ , may be considered constant; so that we may take with little error

$$G' = F \frac{w'}{r+w}, \text{ and } \therefore \frac{G'}{G} = \frac{w'}{w},$$

or the quantity of electricity passing through the galvanometer wire will vary sensibly as the length of that part of the secondary wire included between the wires of the electromotor.

We have thus, differing slightly from methods previously proposed, a means of graduating the galvanometer. By the use of two cells of Daniell's constant battery in series, this may be accomplished with sufficient accuracy by the above method. One battery wire, being in fixed connexion with one point of the double circuit, the other wire must be approximated to, or elongated from, that point, in contact with the secondary wire, until the needle of the galvanometer points to  $1^\circ$ . If the distance of the battery wires be doubled, so as to include a double resistance, the needle ought to point to  $2^\circ$ , if trebled to  $3^\circ$ , and so on, the variations in  $F$ , whatever be those in  $G$  and  $w$ , being wholly inappreciable.

$F = G \cdot \frac{r+w}{w}$ . If the section and length of the secondary wire be properly proportioned to those of the galvanometer wire, it will always be possible, by moving one battery wire backwards and forwards in contact with the secondary wire, while the other battery wire is in fixed connexion, as before



described, to make the value of  $G$  uniformly constant, the needle for example always pointing to  $45^\circ$ , whatever power of electromotor be applied; and as the sum  $r + w$  continues constant,  $F$  in the above equation will then vary inversely as  $w$ . The relative force of different electromotors, however powerful or however weak, may thus be accurately measured by the same delicate galvanometer; being inversely proportional to the length of that part of the secondary wire contained between the wires of the electromotor; or

$$\frac{F'}{F} = \frac{w}{w'}.$$

Moreover, if a compound constant battery of three or four pairs be used, and a voltameter be introduced between the battery and the double circuit, it will be easy to find the value of  $w$  answering to a certain  $F$ , which is also known in terms of the chemical equivalents decomposed in the voltameter in a unit of time. Thus, the wires being adjusted so as to cause the galvanometer needle to point to  $45^\circ$ , note the value of  $w$ , viz. the length of the secondary wire contained between the wires of the battery; note also the length of time required, other things being kept constant, to effect a certain amount of decomposition in the voltameter. The quantity decomposed in one minute, deduced from the above observation by simple proportion, is the  $F$ . The product of the  $F$  and  $w$  thus obtained may then be used in the formula

$$F' = \frac{Fw}{w'},$$

to ascertain the value,—also in terms of chemical equivalents,—of any other force  $F'$ , without the further use of the voltameter, and whether the force have energy sufficient for chemical decomposition or not; for the *quantity* necessary to deflect the galvanometer to a certain fixed point is always the same, whatever be the *intensity* of the current.

Thus the same rigorous measurement, in terms of the weight of matter naturally associated with fixed quantities of electricity, may be applied to all electrical forces, whatever their intensity, which the voltameter has applied to those capable of producing chemical decomposition, and this by an immediate observation, without the loss of time requisite for a chemical quantitative experiment.

Since the needle of the galvanometer is liable to slight changes in the intensity of its magnetism, the above-determined values of  $F$  and  $w$  will from time to time require verification.

To find the resistance of an electromotor. Let a certain

length of wire, of such section and conductivity as are best fitted for a standard of resistance, and whose known resistance may be represented by  $l$ , the units being linear inches, be introduced between the electromotor and the double circuit, and let the wires be again adjusted so as to deflect the needle to  $45^\circ$ , noting the new length of secondary wire included  $= w'$ . The equation representing the force, which, previously to the

introduction of the wire  $l$ , was  $F = \frac{A}{R+L}$ , now becomes

$$F' = \frac{A}{R+l+L'}$$

and since

$$\frac{F'}{F} = \frac{w}{w'}$$

therefore

$$\frac{R+L}{R+l+L'} = \frac{w}{w'};$$

from which, substituting for  $L$  and  $L'$  their values  $\frac{rw}{r+w}$  and  $\frac{r'w'}{r'+w'}$ , and remarking that  $r+w=r'+w'$ , the value of  $R$  is deduced

$$= \frac{lw}{w'-w} - \frac{ww'}{r+w}.$$

Excepting in the case of very weak forces, if the galvanometer be sensitive, having a long helix of fine wire, both  $w$  and  $w'$  will be very small, while  $r+w$  will be very large. Therefore the term  $\frac{ww'}{r+w}$  will have no practical value; and unless the value of  $F$  be very small,  $R$  may be taken without sensible error  $= \frac{lw}{w'-w}$ .

The resistance  $R$  of any electromotor which is constant in its action being thus discovered, any other resistance may be measured by the converse process. If it be introduced between the electromotor and the double circuit in the place of the standard wire of the last paragraph, and the value of  $w'$  be noted as before, the unknown resistance may be deduced from the equation

$$l = \frac{R \cdot (w'-w)}{w} + \frac{w'(w'-w)}{r+w}.$$

The second term of the latter branch of the equation can only be required when  $F$  is very small or  $l$  very large, and consequently  $w'-w$  considerable.

With the help of an additional galvanometer, the resistance of the helix of the one in ordinary use may be measured by the above process; and also the resistance of the secondary wire of the double circuit may be estimated, by introducing as the  $l$  a piece of wire having the same section and conductivity as the secondary wire,—cut off for instance from the same piece. Thus  $r + w$  becomes known, and may then be used for the determination of the resistances opposed to weaker forces. If the helix of the galvanometer be skilfully formed and well-insulated, the resistance  $r + w$  once determined will not be liable to change.

The sum of all the tensions of the circuit, positive and negative, *i. e.* of the affinities producing the current, is known from the equation

$$A = F(R + L).$$

In the majority of cases  $A = FR$  will be a sufficient approximation; or at least  $A = F(R + w)$ , as except in very weak forces,  $w$  will be so small that it will not materially differ from

$$L \text{ or } \frac{r}{r + w} \times w.$$

The practical form which I have given to an instrument for the above purposes, and which I call the “Detached Galvanometer Scale,” is represented in Plate VII., and may be thus described.

It consists essentially of four wires,  $a, b, c, d$ ; the first three, formed of copper, are one-fourth, one-eighth, and one-sixteenth of an inch in diameter respectively; the last,  $d$ , is also one-sixteenth of an inch in diameter, but is formed of platinum. Each of them is exactly eight inches long, and they are all partially sunk in grooves, parallel to, and about one inch distance from, each other, in a board eight inches long of hard well-dried wood, varnished and graduated into tenths of an inch, the lines being drawn through from side to side.

The four wires are joined into one continuous wire, commencing at A and terminating at E, by being soldered to the connecting pieces A, B, C, D, E, which are bored, and furnished with screws to clasp a wire from any electromotor whose force is to be measured.

The connecting pieces are of solid brass, and at least half an inch thick, in order to offer no appreciable resistance to a current passing through them from the wire of the electromotor to the wire of the scale.

When in operation, the wires from the galvanometer are always to be attached to the terminating connecting pieces A and E.

One wire from the electromotor must be attached to A, B, C or D, according to the magnitude of the force to be measured, and the other wire from the electromotor must be moved along in contact with the wire *a*, *b*, *c* or *d*, according as the first wire is connected with A, B, C or D, until the galvanometer needle points to  $45^\circ$ .

F is a vernier which is applicable for this purpose: the moveable wire of the electromotor, being passed through the small hole *e* and brought up again by the notch *f*, will lie in the groove *ef*, and the vernier sliding on the wire of the scale by the groove which is at right angles to *ef*, will enable the operator to read off the position of the moveable wire to  $\frac{1}{1000}$ th of an inch. It is requisite to keep the wires of the scale and vernier bright, and to rub them well with wash-leather before each time of using, and the vernier will require little pressure to obtain a correct indication.

The diameter of wire *c* being half that of *b* and one-fourth that of *a*, its section is one-fourth and one-sixteenth of these respectively. Consequently, it will offer to an electric current four times the resistance which is offered by an equal length of *b* and sixteen times that offered by an equal length of *a*. The wire *d* is of the same diameter as *c*, but being platinum has a less conducting power, say one-fifth, and will therefore offer five times the resistance of an equal length of *c*. The ratio of the resistances of the wires *a*, *b*, *c*, *d*, however, since different pieces of the same material are apt to differ in their conducting power, and very slight differences of diameter become multiplied into importance in the section, must be determined by actual experiment, which is easily done with the help of a constant battery; and the product of F and *w* being also ascertained by experiment for one of the wires, may then be known by computation for the others. As the length of *d* is graduated on the board into eighty divisions, and indeed with the assistance of the vernier into 800, this instrument is calculated to measure a scale of electric forces, extending from its lowest or unity to  $80 \times 5 \times 4 \times 4 = 6400$ , or with the vernier through 64,000°.

With a galvanometer of moderate sensitiveness, having a helix of about 1000 inches of copper wire  $\frac{1}{1000}$ th of an inch in diameter, a force capable of decomposing water sufficient to furnish (F)  $\frac{6}{1000}$ th of a cubic inch of hydrogen gas in one minute, required (*w*) six inches of wire *d* to be interposed between its conducting wires to raise the index of the galvanometer to  $45^\circ$ , making the product of F and *w* for this wire = 36.

For smaller or larger electric forces, additional instruments with finer or coarser wires will be requisite.



The resistances of wires are to each other directly as their lengths, and inversely as the products of their sections and conductibilities. If the wire of the galvanometer helix be as above, 1000 inches in length and  $\frac{1}{100}$ th of an inch in diameter, then eight inches of wire  $d$ , being of platinum, one-sixteenth of an inch in diameter, and having, suppose, one-fifth of the conducting power of copper, will equal in resistance about an inch and a quarter of the helix wire; therefore since  $r=1000$  and the greatest value of  $w=1\frac{1}{4}$ , the quantity  $\frac{rw'}{r+w}$  cannot in this instrument be of any importance in the determination of resistances, and may always be safely neglected. For the accurate measurement, with an instrument having finer wires, of resistances offered to weaker forces, the fraction may be useful.

LX. *Physical and Geological Observations on the principal Geysirs of Iceland.* By A. DESCLOIZEAUX\*.

IN Iceland the name of *Geysir* is generally given to a jetting thermal spring, and the names of *hver* (cauldron) and *laug* (baths) are applied to springs simply bubbling up, or wholly tranquil, contained in basins almost always regular, circular or elliptic.

The jetting springs most celebrated on account of their volume, the beauty of their eruptions, and the importance of the deposits which they have formed and which they still form at the present day, are those known by the names of the *Great Geysir* and *Strokkur*. Although most of the travellers who have visited Iceland have described the eruptions of these two springs which they witnessed, and of which some have published pretty accurate representations, I will here relate the manner in which this phænomenon takes place.

In the Great Geysir, whose waters, as I shall observe hereafter, have formed for themselves a very regular basin in the form of a truncated cone hollowed at the summit, the eruptions are preceded by subterraneous detonations, which have always been justly compared to a distant sound of artillery, and which powerfully shake the base and lateral parts of the cone. After each detonation, the column of water which occupies the central channel is upraised, in the form of a hemisphere, some metres† above the surface of the basin; then all becomes calm again.

These detonations and these upliftings of the liquid mass are produced pretty regularly every two hours, as we have

\* Translated from the *Ann. de Chim. et de Phys.* for April 1847.—The Icelandic orthography has been preserved in this memoir.

† The metre is equivalent to about 3 feet 3½ inches.—Tr.

ascertained by a series of observations carried on for several days, and consequently they are far from announcing a great eruption; but when this is approaching, detonations stronger and more frequent than the first are heard, the uprisings of the central column become more and more considerable, and all at once an immense column of water, three metres in diameter at its base, is projected into the air to a height varying from thirty to fifty metres, spreads like a fountain at its summit, and falls again in part into the large reservoir, eighteen metres in diameter, which forms the basin of the spring; the rest of the mass of water flows over the external sides of the cone, and spreads out into the little rills which furrow the plateau on which this cone rests. The column of water does not generally attain its greatest height until several consecutive jets at very short intervals have prepared the observer for the magnificent spectacle which the Geysir reserves for the close of the eruption, like a skilful firework-maker who terminates his exhibition by a splendid bouquet. These eruptions last from five to seven minutes.

In the Strokkur, the circumstances attending the eruption are a little different, and the form of the channel which discharges the emitted waters is doubtless the principal cause which gives the phænomenon a peculiar character. In this channel, which represents a well even with the ground, the water is at about three metres below the surface, and it boils up with incessant oscillation. When it is attempted to produce an artificial eruption by covering the surface of the water with clods of earth or turf, or when a natural eruption is about to take place, this boiling ceases for some instants, and a pointed, lanceolate jet issues suddenly to a certain height; it is soon followed by another, which is in its turn succeeded by a series of slender columns, very pointed at the summit, with serrated outlines, which cannot be better compared than to immense yew-trees. These columns, which succeed one another sometimes for twelve to fifteen minutes, attain various heights, which reach, as in the Geysir, to fifty metres. When the eruptions of the Strokkur take place naturally, the jetting water is quite as limpid as that of the Geysir; and, if the weather is calm and the sun bright, nothing can exceed the magnificence of these immense columns of clear water, perfectly vertical, and reflecting the brilliant colours of the rainbow. If however the Strokkur is excited by clods of earth or turf, the water is coloured black, often of a very deep tinge, and the phænomenon loses much of its splendour.

Before passing to the results of the thermometric experiments and the various observations made at the Geysir and

the Strokkur, I shall describe in a summary manner the topographical and geological position of these two springs, for the geological portion of the *Voyage en Islande* of the Scientific Commission of the North contains extensive details upon this subject.

The two principal geysirs of Iceland are situated in the south-east part of the island, at eighty-seven kilometres, in a straight line, to the east-north-east of Reykjavík, and at forty-six kilometres to the north-west of Hecla. These fountains, accompanied by a great number of *hver*s and *laugs*, occupy a space of 500 metres from north to south, by 100 metres from east to west, towards the northern part of a large plateau of volcanic tufa, bordered on three sides by large glaciers or *Jökulls*, the principal of which are,—on the north, the Láng-Jökull; on the north-east, the Hofs-Jökull and the Arnarfells-Jökull; on the east, the Skaptár-Jökull; on the south, Hecla and the Torfa-Jökull.

This vast plateau is traversed, from north-east to south-west, by two of the largest rivers in Iceland, the Hvítá and the Thjórsá, and by their numerous tributaries: these two principal rivers, which take their sources, the first in the snows of the Láng-Jökull, and the second at the foot of the Arnarfells-Jökull, flow into the sea at the south-west part of the plateau, which is terminated on this side by low sandy shores. At numerous points of the plateau containing the geysirs, rise mountains which have in many instances the form of isolated cones, composed of a conglomerate, the chief constituent of which is basalt and a brown tufa, the principal element of which appears to be the mineral which Sartorius von Waltershausen found in the ancient tufas of Etna, and which he has named *palagonite*. The height of these mountains, several of which have preserved the appearance of craters of eruption, and still present, on their slopes or at their base, debris of streams of lava, does not exceed 500 to 600 metres.

Beside these cones, we meet with some hillocks of a gray phonolite, inclosing numerous crystals of felspar, and very small crystals of pyroxene: some strips of ancient trap or compact basalt, often divided into columns, appear also at the surface of the plateau; and a sort of vitreous pitchstone, both green and reddish, which appears to have resulted from the fusion of the phonolite by the basalt, is ordinarily seen in contact with these two rocks.

The hill nearest to the geysirs, from which it is distant only about 360 metres, to the west, consists of upheaved and almost vertical strata of a bluish gray phonolite, much

disintegrated on the lower part, where it presents numerous traces of having been acted upon and undergone a slight alteration by the thermal waters, but it is tenacious and tolerably homogeneous towards the summit. The height above the plain of this hill, which has the name of Laugafjall, may be estimated at seventy-five metres. From the foot of its eastern escarpment, an undulating district extends for a length of about 350 metres, entirely composed of ancient siliceous concretions, slightly altered and friable. This district, the highest hills of which rise to nearly twenty-five to thirty metres' elevation above the plain, is pierced with a multitude of holes, from which steam and the vapour of sulphuretted hydrogen still escape; small quantities of alum and sulphur are deposited on the edges of some of these holes, one of which is situated at the very foot of the trachytic hill.

The existence of an ancient siliceous district, so extensive as that which I have just mentioned, certainly appears attributable to numerous extinct geysirs, the abundant deposits of which have at length completely stopped the orifices from which their eruptions took place. The lower part of this ancient district is separated from the deposits of the present Great Geysir by a small ravine of two metres in depth, situated at ten metres from the basin of the geysir, and directed nearly from south to north. The sides of this ravine, from whence acid vapours are also disengaged at several points, discloses both in the ancient and modern formation, numerous layers of siliceous concretions of a very varied aspect, distributed in the midst of a bolary clay in which the red colour predominates.

On the right side which bounds the deposits of the present geysir, and in a very plastic clay, we meet with small stems of birch-trees, of the size of those which still grow in the country, entirely silicified and converted into chalcedony; the clay itself is full of small crystals of white pyrites, formed from the iron which it contains and of the sulphur of the vapour springs. Above the chalcedonous stems, and approaching the most recent strata of concretions, we now only find birch-leaves and a network of herbaceous plants, forming a sort of siliceous travertine. In this same part, and always in the middle of a reddish clay, I have observed a thin, very regular, and extensive layer of zoned chalcedony; this substance, which is translucent as long as it remains immersed in the moist clay, becomes opaque and of an enamel white by a simple desiccation in the open air, without being able to re-acquire its transparency, even on a prolonged immersion.

The banks of the little river the Beíná, which flows at 300



metres to the east of the Geysir, and into which a part of the water that overflows after each eruption run, present a fact of the same kind: in the midst of a network of silicified plants forming several layers, the total thickness of which exceeds three metres, we found small portions of silex, which possessed all the properties of precious opal, as long as they are strongly hydrated, but which only preserve their vivid colours while they remain immersed in water, or are kept from drying. May we not hence conclude that the opals and chalcedonies of some ancient volcanic districts owe their origin to phenomena similar to those of the present geysirs of Iceland?

The left side of the small ravine spoken of above, which bounds the deposits of the ancient extinct geysirs, did not present chalcedonous wood, but we extracted from it birch stems transformed into very friable white silica, which appeared to have been derived from individuals larger than those we meet with at the present day in Iceland. Some specimens of these woods, in which the cells and vessels may still be observed under the microscope, are penetrated in various directions with small roots which appear to belong to the same species; we might without doubt thence conclude that the large stems were already in a state of advanced decomposition when the silica came and penetrated into their tissue,—a circumstance which would perhaps explain the reason why these woods have not been converted into chalcedony, but into pulverulent silica.

It is at all events certain that a birch-wood once existed between the ancient and recent geysirs; and, as these trees cannot live where the soil is covered with siliceous concretions, it must be admitted that these two classes of fountains did not exist simultaneously, and that the recent geysirs succeeded the primitive geysirs, when the orifices of these last became insufficient to give a passage to the masses of water which had to be cast forth\*.

With respect to the duration of the ancient phenomena, the depth of twenty-five metres at least of the soil which they have formed, shows that it must have been very considerable. In fact, the concretions deposited by the present geysirs have not yet exceeded a thickness of four to five metres; and all seems to prove that, within the historical period of Iceland, the increase of this thickness, which is distributed over an extended surface, has been very small, so that we must attribute a very high antiquity to the recent geysirs themselves†.

\* A very detailed description of the districts formed by the ancient and recent geysirs has been given by M. Robert in the geological part of the Voyage of the Northern Scientific Commission to Iceland.

† As it is almost impossible, in order to prove the thickness of deposits

The two jetting springs, the temperatures of which I have principally sought to determine during my stay in that locality with M. Bunsen of Marburg, are the Great Geysir and the Strokkur. I shall first direct my remarks to the Great Geysir.

The only observations of any accuracy which we hitherto possessed on the temperature of this spring are those of M. Lottin, a member of the Northern Scientific Commission, who visited Iceland in 1836; but these observations left much still to be desired, both from the method itself which M. Lottin followed, and from the experiments not having been sufficiently repeated to embrace the various phases of the moments of calm and of eruptions of the geysir. The determinations which M. Bunsen and I have made were taken immediately before a great eruption, immediately after that eruption, and in the interval of two great consecutive eruptions. The thermometers which I employed were double-bulbed, and constructed by Bunsen after the directions of M. Regnault\*: they were composed of a small cylindrical reservoir, surmounted by a stem of accurate calibre, and divided into parts of equal capacity; this stem ended in a finely drawn-out point terminating almost at the summit of a small cap of thin glass, soldered to the top of the stem, at the part where its diameter began to diminish, and containing a small excess of mercury.

In taking observations with these thermometers, the stem was entirely filled with mercury, at the ambient temperature; it was then immersed, by the side of a good standard, in a bath, the temperature of which was nearly constant, and lower than that which was to be observed: a portion of the mercury of the stem was raised into the small upper bulb. When the mercury was stationary and occupied exactly the extremity of the small drawn-out point, the temperature of the standard formed in a given time, to establish at the Geysir itself a fixed mark capable of resisting the ignorant curiosity of certain visitors, I endeavoured to construct one easy to be recognised only by the forewarned and attentive eye. For this purpose, I chose two contiguous *laugs*, situated at nearly an equal distance from the Strokkur and the Geysir, proceeding towards the Laugafjall: the most northern *laug*, without apparent overflow, is contained in a nearly elliptical basin, the major axis of which is seven metres, the minor one 3<sup>m</sup>·40, and the depth 2<sup>m</sup>·55; the temperature at the surface was found to be 98°·5. The second *laug*, separated only from the first by a small natural wall, has a more irregular basin, of a depth of 9<sup>m</sup>·30, and the water of which, indicating a temperature of 96°·5, flows off incessantly by a small rivulet which passes near the Strokkur, depositing on its way abundant incrustations. In the narrowest part of the wall which separates these two *laugs* I made an aperture, the successive obliteration of which it will always be easy to ascertain. The following were the dimensions of this aperture on July 12th, 1846:—Depth 0<sup>m</sup>·065; length, from east to west, 0<sup>m</sup>·260; breadth, from north to south, 0<sup>m</sup>·110.

\* See Scientific Memoirs, Part XVI. p. 588.

was noted; the thermometer, inclosed in a copper or iron case, the lid of which was hermetically closed by means of a screw and red-lead cement, was let down into the spring; after remaining there thirty minutes, it was raised and compared again with the standard; the mercury stopped at a certain division of the stem, and the quantity of mercury which had escaped from the stem during the observation, converted into centigrade degrees and added to the first temperature indicated by the standard, gave the temperature of the spring. The thermometers employed by M. Bunsen were constructed upon the same principle; only that, instead of a capsule soldered to the upper part of the stem, the capillary opening of the latter was closed by a small point of iron, kept vertical by means of a spring, which the mercury could raise, so as to fall over into the case.

In each of the five experiments which we made at the Great Geysir, we employed five thermometers arranged on a single line, and separated by nearly equal intervals.

It is known that the Great Geysir has formed by successive incrustations a very regular basin, having the shape of a truncated cone, the exterior slopes of which have a mean inclination of  $8^{\circ}$ , whose summit is occupied by a cup or vase\*, presenting only on the edges some small clefts through which is carried the overflow of the water, when its level rises. The mean inclination of the sides of this cup is  $13^{\circ}$ .

In the centre of the basin is a circular well, which appears perfectly regular as far as the eye can discern. Between two consecutive eruptions, this well and the cup which surrounds it are completely filled with water, a very small portion only of which overflows after the upheavings, which take place about every hour and half: but immediately after the great eruptions, the cup is completely empty, and in the well the water descends below the bottom of the cup from one metre to 2<sup>m</sup>.50, according as the projected column had attained a greater or less height; the former level is not restored until after six or seven hours.

The following are the principal dimensions which we have found for the different parts of the Geysir:—

Diameter of the basin or cup, on the edges, from	m
north to south . . . . .	16.00
Diameter of the basin or cup, in a perpendicular	
direction . . . . .	18.00
Diameter of the central well . . . . .	3.00

\* Sir W. J. Hooker thus describes it:—"To compare great things with small, the shape of this basin resembles that of a saucer with a circular hole in its middle." *Tour in Iceland*, p. 117.—Tr.

- Depth of the well, including that of the basin :—  
 1. In the direction of the first diameter . . . . m 23·50  
 2. In the direction of the second diameter . . . . 21·50  
 Depth of the cup in the centre . . . . . 1·57

As we took care, in each experiment, to alter the order of our thermometers, that their indications might be checked, I shall indicate by numbers and by the letter B those of M. Bunsen, and by the letter D those which belonged to me.

It will be conceived that the temperature at the surface of the water which fills the basin must be excessively variable ; for that temperature depends at the same time on the temperature of the sides of the basin, on that of the air, on the force and direction of the wind, and on the hygrometric state of the atmosphere, which allows of a more or less speedy evaporation. This is the reason why the numbers given by different travellers, for the temperature of the water on the edge of the basin, agree so little ; we ourselves made a series of observations, the terms of which differ considerably among themselves. They are as follow :—

1846.	h m	Temp. at the margin.
3rd July, at 10 30 A.M. ; basin filled .		86°·0 centigrade.
3rd July, at 10 58 A.M. ; <i>Id.</i> . . . .		89·0
3rd July, at 11 30 A.M. ; <i>Id.</i> . . . .		88·0
3rd July, at 3 25 P.M. ; <i>Id.</i> . . . .		89·7
4th July, at 12 35 P.M. ; <i>Id.</i> . . . .		76·0
4th July, at 1 17 P.M. ; <i>Id.</i> . . . .		85·0
9th July, at 1 50 P.M. ; basin half-full		82·5 in the wind.
9th July, at 1 50 P.M. ; <i>Id.</i> . . . .		86·0 under the wind.

We will admit that the mean temperature of the water at the surface and in the centre of the basin is 85° C.

The following table indicates the temperatures found in different points of the liquid column, immediately before a great eruption :—

<i>Third Experiment.</i>			<i>Fourth Experiment.</i>		
The 7th July, at 2 <sup>h</sup> 55 <sup>m</sup> in the afternoon : four hours before a great eruption. Basin filled : total depth, 23 <sup>m</sup> ·50 ; length of the line, 22 <sup>m</sup> ·85.			The 7th July, at 6 <sup>h</sup> 58 <sup>m</sup> in the afternoon : ten minutes before a great eruption. Basin filled : total depth, 23 <sup>m</sup> ·50 ; length of the line, 22 <sup>m</sup> ·85.		
	Heights.			Heights.	
	m			m	
	85·0	22·85		85·0	22·85
Therm. No. 1, D . . . . .	85·2	19·55	Therm. No. 5, D . . . . .	84·7	19·55
Therm. No. 2, B . . . . .	106·4	14·75	Therm. No. 1, B . . . . .	110·0	14·75
Therm. No. 5, D . . . . .	120·4	9·85	Therm. No. 6, D . . . . .	121·8	9·85
Therm. No. 6, D . . . . .	123·0	5·00	Therm. No. 1, D without result.		5·00
Therm. No. 1, B . . . . .	127·5	0·30	Therm. No. 2, B . . . . .	126·5	0·30
	Bottom.			Bottom.	
Mean temperature of the column of water, 108°·33.			Mean temperature of the column of water, 109°·19.		



The mean temperature of the column of water was obtained by multiplying half the sum of the indications of two consecutive thermometers, by the interval which separates them, and dividing the sum of all these products by the sum of the intervals, or the length of the line.

Twelve minutes after the fourth experiment, at 7<sup>h</sup> 40<sup>m</sup> in the evening, the great eruption took place.

The following are the results of the fifth experiment commenced the 7th July, at 9<sup>h</sup> 45<sup>m</sup> in the evening, two hours after the great eruption; basin half-filled: height of the liquid column, 22<sup>m</sup>·75; length of the line, 22<sup>m</sup>·50.

		Heights. m
	85·0	22·50
Thermometer No. 1, D . .	103·0	13·50
Thermometer No. 3, D . .	121·0	9·70
Thermometer No. 5, D . .	lost	4·90
Thermometer No. 6, D . .	122·5	0·30
		Bottom.

Mean temperature of the column of water 108°·83.

The three preceding tables show, in an evident manner, that the mean temperature of the column of water which fills the central well of the Great Geysir, remaining in other respects constant, the distribution of the temperature, in different points of that column, varies with the moment when the observation was made. We see, on examining these tables, that there is at the bottom of the well a maximum, immediately before the eruptions, which may be estimated at 127°; and a minimum, immediately after, which may be estimated at 123°.

The results given in the two following tables, obtained at different periods between two consecutive eruptions, show that the phænomenon always passes in the same manner, and with tolerable regularity:—

<i>First Experiment.</i>			<i>Second Experiment.</i>		
The 6th July, at 0 <sup>h</sup> 30 <sup>m</sup> in the morning: three hours after a great eruption, and eleven hours before the following eruption. Basin half-full: height of the liquid column, 22 <sup>m</sup> ·75.			The 6th July, at 8 <sup>h</sup> 20 <sup>m</sup> in the morning: nine hours after a great eruption, and twenty-three hours before the following eruption. Basin filled: height of the column, 23 <sup>m</sup> ·50; length of the line, 22 <sup>m</sup> ·85.		
	Heights. m			Heights. m	
	85·0	22·50		85·0	22·85
Therm. No. 5, D . . . . .	95·0	19·70	Therm. No. 6, D . . . . .	82·6	19·20
Therm. No. 3, D . . . . .	109·0	16·30	Therm. No. 5, D . . . . .	85·8	14·40
No result.		12·90	Therm. No. 2, B . . . . .	113·0	9·60
Therm. No. 2, B . . . . .	121·1	9·50	Therm. No. 3, D . . . . .	122·7	4·80
Therm. No. 1, B . . . . .	121·6	6·00	Therm. No. 1, B . . . . .	123·6	0·30
Corresponding number of the fifth exp. . . . .	122·5	0·30			Bottom.
		Bottom.			
Mean temperature of the column of water, 112°·68.			Mean temperature of the column of water, 102°·30.		

Thus, notwithstanding the relative lightness of the most heated water, which tends to rise to the surface, we see that, even after the eruptions, the temperature of the different strata does not become equalized; still further, in proportion as the inferior strata become heated, to reach their maximum, the upper strata become cool, undoubtedly in consequence of evaporation, and of the heat communicated to the sides of the well and the basin; so that the greatest difference of temperature at the two extreme levels corresponds precisely to the moment of the eruptions.

We can now ascertain what would be the boiling-point of water submitted to a pressure equal to that of the atmosphere, augmented by the liquid column the height and mean temperature of which we have determined in our five experiments. For this purpose, we must first express the pressure of this liquid column in millimetres of mercury at  $0^{\circ}$ . The density of the water of the Geysir, found at from  $1^{\circ}$  to  $10^{\circ}$  centigrade; the curve which represents the results obtained by M. Isidore Pierre for the dilatation of distilled water, and which I admit as being applicable to the water of the Geysir, extending it up to  $113^{\circ}$ ; lastly, the density of the mercury at  $0^{\circ}$ , compared to that of distilled water at  $4^{\circ}$ , and found at 13.596 by M. Regnault, enables us to make this reduction. We thus find that the heights of the columns of water in our different experiments correspond, in millimetres of mercury at  $0^{\circ}$ , to the following numbers:—

Third and fourth experiment . . . . .	<sup>m</sup> 1.645
Fifth experiment . . . . .	1.593
First experiment . . . . .	1.587
Second experiment . . . . .	1.654

From the equations

$$\log e = a - b\alpha^x, \quad x = t - 100^{\circ},$$

and from the data

$$\log a = 1.9977641, \quad \log b = 0.4692291, \quad a = 5.8267890,$$

laid down by M. Regnault, in which  $e$  represents the tension of steam at saturation, expressed in millimetres of mercury, and  $x$  the temperature to which that pressure corresponds, we can derive the value of  $x$  and of  $t$ ; and this last quantity will precisely represent the boiling-point of water submitted to the pressures

$$\begin{array}{ll} 0.760 + 1^{\text{m}}.645, & 0.760 + 1^{\text{m}}.593, \\ 0.760 + 1^{\text{m}}.587, & 0.760 + 1^{\text{m}}.654. \end{array}$$

The corresponding values of  $t$  are as follow :—

Third and fourth experiment . . .	$t = 136^{\circ} \cdot 151$
Maximum temperature at the bottom	127
Difference . .	$9^{\circ} \cdot 151$

Fifth experiment . . . . .	$t = 135^{\circ} \cdot 398$
Minimum temperature at the bottom	$122^{\circ} \cdot 5$
Difference . .	$12^{\circ} \cdot 898$

First experiment . . . . .	$t = 135^{\circ} \cdot 31$
Temperature at the bottom . . .	$122^{\circ} \cdot 5$
Difference . .	$12^{\circ} \cdot 81$

Second experiment . . . . .	$t = 136^{\circ} \cdot 28$
Temperature at the bottom . . .	$123^{\circ} \cdot 60$
Difference . .	$12^{\circ} \cdot 68$

Thus, at the point to which the lower thermometer can penetrate, the water does not attain the temperature of ebullition which it should have under the pressure to which it is subjected ; but it approaches that temperature in proportion as a great eruption is near. We may thence conclude that the centre of heat which raises the temperature of the water of the Geysir is situated at a certain distance below the presumed bottom of the central well, and we may deduce from this a plausible explanation of the manner in which the eruptions are determined.

Let us in fact suppose that the column of water in the central basin communicates, by a long and sinuous channel, with the space, be it what it may, which receives the direct action of the subterranean heat : after an eruption during which a projection of a great quantity of water and vapour has taken place, the lower parts of the liquid mass are cooled, and the steam which is formed in the reservoir submitted to the action of the heat has a less tension than that at which the weight of the central column and that of the atmosphere are in equilibrium ; this vapour, as it forms, becomes condensed in contact with the water which fills the sinuous channel, and it imparts to that water its latent heat. The increase of temperature of the water of the channel is transmitted by degrees to the lower part of the central column where the thermometer can reach ; but this increase is retarded by the atmospheric air and the other gases which accompany the vapour ; however, at the lapse of a shorter or longer time, the water of the channel must boil, and the steam which continues to form cannot longer condense there ; this vapour must therefore accumulate, and acquire a gradually increasing tension, until this

tension is able to overcome the resistance of the column of water which fills the basin, and to project it into the air.

A thermometer placed at four metres above the bottom of the central well during an eruption which took place July 14, 1846, at 3<sup>h</sup> 15<sup>m</sup> in the morning, and which raised the column of water to 49<sup>m</sup>·37, according to a measurement taken by M. Waltershausen, indicated a temperature of 124°·24, which corresponds perfectly to what would have been found at the same point in the third and fourth experiments.

In the intervals of the great eruptions it often happens that the column of water is raised to one metre, or 1<sup>m</sup>·50 above the usual level, and it is easy to conceive that it must be thus; for, if the vapour formed in the subterranean reservoir is not condensed entirely at its contact with the water of the sinuous channel of which I have spoken, and if it is accompanied with a sufficient quantity of atmospheric air, or of other gases, it must in the end divide the liquid column, and escape at the surface, producing those subterranean detonations and those jets which all travellers have remarked. A fact worthy of attention is, that these detonations appear to be produced periodically with sufficient regularity; whereas the great eruptions only take place at quite unequal intervals, as is shown by the following table of a series of observations which we continued for several days. This difference might be explained by admitting that the production of vapour is not perfectly regular. In the following table, the three columns indicate the date of the observations, the hours when the detonations or jets took place, and those when great eruptions were produced:—

Dates of the Observations.	Explosions and great Eruptions.	Dates of the Observations.	Explosions and great Eruptions.	Dates of the Observations.	Explosions and great Eruptions.
3rd July ...	h m 9 45 a.m.	4th July ...	h m 10 30 a.m.	5th July ...	h m 6 53 p.m.
" "	10 58 a.m.	" "	11 36 a.m.	" "	8 20 p.m.
" "	12 15 p.m.	" "	1 13 p.m.	.....	9 45 p.m.*
" "	1 5 p.m.	" "	2 4 p.m.	Interruption.	
" "	2 5 p.m.	.....	3 39 p.m.*	6th July ...	7 40 a.m.
" "	3 25 p.m.	4th July ...	7 45 p.m.	" "	9 25 a.m.
" "	4 0 p.m.	" "	9 0 p.m.	.....	11 0 a.m.*
" "	8 0 p.m.	" "	11 15 p.m.	Interruption.	
" "	8 25 p.m.	" "	11 23 p.m.	6th July ...	3 0 p.m.
" "	9 55 p.m.	Interruption.		" "	5 45 p.m.
" "	10 48 p.m.	5th July ...	5 0 a.m.	" "	6 45 p.m.
4th July ...	12 14 a.m.	" "	8 45 a.m.	" "	8 0 p.m.
Interruption.		" "	9 44 a.m.	" "	9 30 p.m.
4th July ...	6 0 a.m.	" "	11 15 a.m.	" "	10 50 p.m.
" "	7 50 a.m.	" "	2 5 p.m.	Interruption.	
" "	9 7 a.m.	" "	4 34 p.m.	7th July ...	9 15 a.m.

\* The great eruptions are denoted by an \*.



Table (continued).

Dates of the Observations.	Explosions and great Eruptions.	Dates of the Observations.	Explosions and great Eruptions.	Dates of the Observations.	Explosions and great Eruptions.
	h m		h m		h m
7th July ...	10 52 a.m.	10th July...	12 45 p.m.	Interruption.	
" "	1 30 p.m.	" "	2 10 p.m.	12th July...	5 57 p.m.
" "	2 53 p.m.	" "	3 30 p.m.	" "	7 46 p.m.
" "	3 30 p.m.	" "	4 24 p.m.	" "	8 55 p.m.
" "	5 40 p.m.	" "	5 30 p.m.	Interruption.	
" "	7 40 p.m.*	" "	7 15 p.m.	13th July...	5 22 a.m.
" "	10 50 p.m.	" "	9 33 p.m.	" "	6 48 a.m.
" "	11 10 p.m.	Interruption.		" "	7 25 a.m.
" "	11 35 p.m.	11th July...	3 30 a.m.	" "	8 40 a.m.
Interruption.		" "	4 52 a.m.	" "	9 52 a.m.
8th July ...	3 0 a.m.	Interruption.		" "	11 7 a.m.
" "	4 20 a.m.	11th July...	8 30 a.m.	" "	11 28 a.m.
" "	6 10 a.m.	" "	9 55 a.m.	" "	12 32 p.m.
" "	8 27 a.m.	" "	11 30 a.m.	Interruption.	
" "	10 16 a.m.	Interruption.		13th July...	3 47 p.m.
" "	12 35 p.m.	11th July...	1 46 p.m.	Interruption.	
Interruption.		" "	3 35 p.m.	13th July...	6 45 p.m.
9th July ...	10 45 a.m.*	" "	4 35 p.m.	" "	7 53 p.m.
Interruption.		" "	6 10 p.m.*	" "	9 12 p.m.
9th July ...	3 25 p.m.	Interruption.		" "	9 47 p.m.
" "	5 45 p.m.	12th July...	5 5 a.m.*	" "	10 55 p.m.
" "	7 25 p.m.	Interruption.		" "	11 43 p.m.
" "	9 25 p.m.	12th July...	9 6 a.m.	Interruption.	
Interruption.		" "	10 45 a.m.	14th July...	3 15 a.m.*
10th July ...	8 0 a.m.*	" "	12 55 p.m.	Interruption.	
Interruption.		" "	2 50 p.m.	15th July...	9 0 a.m.*

The heights of the two most remarkable eruptions during our visit to the Geysir, measured by M. Sartorius von Waltershausen with a theodolite, gave—

The 6th of July, at 11<sup>h</sup> 00<sup>m</sup> A.M., 145 French ft.=47<sup>m</sup>·101.

The 14th of July, at 3<sup>h</sup> 15<sup>m</sup> A.M., 152 French ft.=49<sup>m</sup>·375.

I have said, at the commencement of this memoir, that the water, when it had completely left the basin, after the great eruptions, descended into the central well, at about 2<sup>m</sup>·50 below its orifice. The following are two numbers I observed:—

The 11th of July, at 6 o'clock in the evening, a depth  
of water below the orifice of the well . . . . 2<sup>m</sup>·00

The 14th of July, at 3<sup>h</sup> 15<sup>m</sup> in the morning † . . . 2<sup>m</sup>·60

\* The great eruptions are denoted by an \*.

† If we calculate the quantity of water which, to produce this lowering, escaped from the basin, during and after the eruption, to flow almost wholly into the Beiná, we find that this quantity was, in the first case, 157<sup>mc</sup>·585, and, in the second case, 161<sup>mc</sup>·825: now, admitting that at 135°·8 and under the pressure of three atmospheres, the steam has a volume equal to 600 times that of the water which produced it, and a density of 0·0016145, a quantity of vapour represented by 94,551 cubic metres = 152,652 kilogrammes in the first case, and by 97,095 cubic

I also availed myself of my visit to the Geysir to make some chemical experiments upon the water of this remarkable spring, which it would have been difficult to render so conclusive with water carried to Paris. I paid particular attention to the estimation of the sulphuretted hydrogen, the odour of which is very perceptible in the vapour which incessantly escapes from the basin, and to ascertain whether the soda contained in that water is in the caustic or carbonated state.

The sulphydrometer of M. Dupasquier served me for the first determination. On operating with necessary caution, I found that one litre of water of the Geysir, perfectly limpid and cooled, protected from contact with the air, to about  $40^{\circ}$ , contained  $2^{\text{cc}}\cdot448$  of sulphuretted hydrogen.

To ascertain the state of the soda, we collected some water in a well-stoppered bottle, acidulated it with some drops of hydrochloric acid, and boiled it; the vapour, collected in some lime-water, yielded a very perceptible precipitate. Mixed with some ammoniacal chloride of calcium, the water was not rendered perceptibly turbid; it therefore does not contain free carbonic acid.

The presence of chlorine or of alkaline chlorides was likewise ascertained by means of nitrate of silver.

To obtain a complete analysis of this water, I collected a quantity in a flask with a drawn-out neck and closed it over the lamp, and in glass bottles closed with a good cork stopper covered with caoutchouc.

The water of the first flask was employed to estimate the carbonic acid combined with the soda; this determination was made in the laboratory of the Val-de-Grâce, where M. Millon was kind enough to place at our disposal one of the apparatus which he employs to estimate the carbonic acid in organic analysis. A second operation, made comparatively on water of the bottles closed with a caoutchouc stopper, gave a result nearly identical with that of the first experiment.

metres = 156,760 kilogrammes in the second case, on condensing upon the lower part of the Geysir, might re-establish its primitive level.

I have stated above that this level was re-established in about seven hours, and that from that moment only a small quantity of water flowed over after each of the detonations which took place every two hours.

The deficit in the Geysir, after a great eruption, might therefore be made good by a subterraneous disengagement of vapour, varying from 1807 kilogrammes to 2394 kilogrammes an hour. Now an ordinary steam-engine, without coating or covering and working without pressure (*sans enveloppe et sans détente*), consuming five kilogrammes of pit-coal an hour, expends about thirty kilogrammes of vapour by horse-power. We see therefore that the present phænomenon of the Geysir requires for its production, only a development of vapour corresponding to nearly that of an engine of 700 horse-power, which gives an approximate idea of the order of this phænomenon.

M. Damour kindly undertook the analysis of the residue which this water leaves on evaporation, and I here transcribe only the results which he has given in a separate notice. A litre of this water, evaporated at 65° centigrade, gives a residue weighing 1·3900 gr., and effervescing with acids. Heated to 300°, the weight of this residue was reduced to 1·1830; and at a cherry-red heat the residue agglutinates, and now amounts to only 1·0540.

The same quantity of water contains,—

	gr.
Chloride of sodium . . . . .	0·2638
Sulphate of magnesia . . . . .	0·0091
Sulphate of potass . . . . .	0·0180
Sulphate of soda . . . . .	0·1343
Soda . . . . .	0·1227
Silica . . . . .	0·5190
Carbonic acid . . . . .	0·1520
Sulphur . . . . .	0·0036
	<hr/> 1·2225

The quantities of oxygen of the silica and the bases are in the following relation:—

	gr.		Oxygen.	Relations.
Silica . . . . .	0·5190		0·2696	3
Soda . . . . .	0·3427	0·0876 }	0·0892	1
Potass . . . . .	0·0097	0·0016 }		

On subtracting from the weight of the alkalies the quantity necessary to saturate the chlorine and the sulphuric acid, there remains,—

	gr.	Oxygen.	Relations.
Silica . . . . .	0·5190	0·2696	9
Soda . . . . .	0·1227	0·0314	1

In several analyses, M. Damour found that the relation 3 : 1 between the oxygen of the silica and that of the bases was constant, and he concluded therefrom that the silica is probably dissolved first in the water of the Geysir, in the state of alkaline silicates,  $\text{NaO}$ ,  $\text{SiO}^3$ ,  $3\text{NaO}$ ,  $2\text{SiO}^3$ , which can be produced artificially, and which are very soluble in water; then, by the action of the sulphurous and hydrochloric vapours, sulphates and chlorides are formed: the quantity of alkali combined with the silica undergoes a successive reduction, and the primitive relation of 3 : 1 becomes 9 : 1. In this state of saturation, a portion of the silica ceases to be soluble, and is deposited, without doubt, in a quantity corresponding to the amount of alkali saturated every day by the action of the vapour springs, and consequently of the

oxidation of the alkaline sulphurets in contact with the atmosphere.

We can conceive that the water of the Geysir, passing into the state of vapour at a very high temperature in subterranean conduits formed by felspathic rocks, attacks their sides, and is thus charged with the salts which the analysis discovers in it.

Beside the analysis of the water of the Geysir, the memoir of M. Damour contains those of several of the springs of Reykir, on the south coast of the island, and that of the water of Laugarnes, situated at a short distance from Reykjavík. All these waters are alkaline and siliciferous; and, although the proportions of the substances which they contain are different in each, they offer, like that of the Geysir, a constant relation between the oxygen of the silica and that of the bases: only, the thermal springs of Reykir deposit, like the Geysir, numerous siliceous concretions, whilst that of Laugarnes forms none at all. The experiments of M. Damour explain this difference very well; for in the waters of Reykir, the constant relation between the oxygen of the silica and that of the bases is as 2 : 1; and after subtracting the quantity of alkalies necessary to saturate the chlorine and sulphuric acid, this relation becomes as 8 : 1. In this state of saturation, a part of the silica ceases to be soluble and is deposited.

In the water of Laugarnes, on the contrary, the primitive relation of the quantities of oxygen is as 3 : 1, as for the Geysir, and this relation, after the saturation of the chlorine and the sulphuric acid, becomes as 6 : 1, numbers which represent an alkaline silicate entirely soluble in water.

### *Strokkur.*

The Strokkur, the Icelandic name of which, signifying *churn*, well indicates the continual movement of the water in the channel which contains it, is situated at about sixty metres to the south of the Great Geysir. It has not, like the latter, formed itself a regular and conical basin; the waters occupy a sort of level well, the edges of which are only encircled by a rounded mound, and worn by the frequent passage of the waters.

The dimensions of this channel are as follows:—

Total depth below the surface . . . . .	13 <sup>m</sup> ·55
Diameter of the opening . . . . .	2 <sup>m</sup> ·40
Diameter at 8 <sup>m</sup> ·30 below the surface . . . . .	0 <sup>m</sup> ·26

The channel of this second jetting spring is therefore irregular, and its diameter diminishes rapidly from the surface to



the bottom. The level of the water, which generally stands at rather above three metres below the ground, falls principally after great eruptions, but it sometimes rises suddenly; and the surface of the water is incessantly boiling.

The following are some observations I made upon these oscillations:—

8th July, at 4 <sup>h</sup> 30 <sup>m</sup> P.M.: depth below the surface .	<sup>m</sup> 3·40
9th July, at 11 o'clock A.M.: after a great eruption .	4·22
9th July, after a second eruption, immediately after the first . . . . .	4·55
9th July, at 3 o'clock P.M.: Strokkur calm; depth .	3·35
9th July, at 4 <sup>h</sup> 15 <sup>m</sup> P.M.: after a great eruption . .	4·40
9th July, at 4 <sup>h</sup> 59 <sup>m</sup> P.M.: Strokkur calm . . . .	4·40
9th July, at 5 <sup>h</sup> 32 <sup>m</sup> P.M.: <i>Id.</i> . . . . .	3·05
9th July, at 7 <sup>h</sup> 15 <sup>m</sup> P.M.: <i>Id.</i> . . . . .	3·50
10th July, at 6 <sup>h</sup> 57 <sup>m</sup> P.M.: <i>Id.</i> . . . . .	3·55

To determine the temperature of this column of water, we employed the same thermometers as at the Great Geysir; but we only arranged three on a line. The following are the results obtained in these three experiments:—

<i>First Experiment.</i>			<i>Second Experiment.</i>		
8th July, at 4 <sup>h</sup> 38 <sup>m</sup> P.M.: height of the column of water above the bottom, 10 <sup>m</sup> ·15.			9th July, at 5 <sup>h</sup> 32 <sup>m</sup> P.M., an hour after a great eruption: height of the column of water above the bottom, 10 <sup>m</sup> ·50.		
		Heights. <sup>m</sup>			Heights. <sup>m</sup>
	100·0	10·15		100·0	10·50
Therm. No. 1, D.....	108·0	6·00	Therm. No. 3, D.....	100·5	9·20
Therm. No. 3, B.....	111·4	3·00	Therm. No. 3, B.....	109·3	6·20
Therm. No. 8, B.....	112·9	0·30	Therm. No. 6, D.....	114·2	2·95
		Bottom.			Bottom.
Mean temperature of the column of water, 104°·77.			Mean temperature of the column of water, 105°·79.		

<i>Third Experiment.</i>		
10th July, at 6 <sup>h</sup> 57 <sup>m</sup> P.M., six hours after a great eruption: height of the column of water above the bottom, 10 metres.		
		Heights. <sup>m</sup>
	99·9	10·00
Thermometer No. 3, D .....	99·9	8·85
Thermometer No. 3, B .....	113·7	4·65
Thermometer No. 6, D .....	113·9	0·35
		Bottom.
Mean temperature of the column of water, 105°·278.		

From the water bubbling incessantly at the surface, we

always supposed that its temperature there was that of boiling water; the second and third experiments show, in fact, that at one metre below the surface it is so. Lastly, a thermometer placed at the bottom of the channel during an eruption, indicated a temperature of  $115^{\circ}$  C.

We see that the column of the water of the Strokkur possesses, at different heights, much more constant temperatures than those of the Geysir, and that the influence of the eruptions is very little felt; this depends evidently on the continual boiling of the water, which determines incessantly ascending currents in the column and a sort of intermixture. The mean height of the column of water of the Strokkur is equal to a column of mercury at  $0^{\circ}$ , of  $0^{\text{m}}\cdot 7179$ .

The temperature at which the water, submitted to this pressure and to that of the atmosphere, would begin to boil, is  $120^{\circ}\cdot 043$ .

We have seen that the maximum temperature at the bottom of the Strokkur was  $115^{\circ}$ ; the difference between this maximum and the point of ebullition of the water is therefore  $5^{\circ}\cdot 043$ . This number is sensibly smaller than the corresponding number found at the Great Geysir, which may be owing to the point attained by the thermometer in the Strokkur being nearer to the subterranean source of heat, or to the channels which form the communication between the liquid column and that central point being narrower; the heat parted with by the vapour condensed to the water of this channel is transmitted more rapidly to the base of the column.

The eruptions of the Strokkur, which are quite as high as those of the Great Geysir, instead of presenting, like the latter, the appearance of a beautiful fountain with a base three metres in diameter, and a crown of about eighteen metres, present only an *ensemble* of sharp, jagged points, resembling very tall and slender yew-trees. In the Geysir, the eruptions always take place naturally; at the Strokkur, on the contrary, they are either natural or artificial. All travellers in fact have remarked that, if the orifice of the Strokkur is filled with light stones or pieces of earth and grass, the oscillations of the water cease for some instants; then, at the end of five or ten minutes, a jet of a blackish water charged with earth is shot out to a great height, and is followed by other intermitting jets during eight to ten minutes. This experiment, which can be repeated several times a day with success, seems to prove that the small excess of pressure resulting from the accumulation of clods of earth upon the water suffices to hinder the normal disengagement of the vapour through the column of water, and to augment the tension of that which

is formed in the subterranean reservoir, to the extent of producing a sudden detonation.

Be this as it may, as we desired to know whether the natural eruptions of the Strokkur were produced more regularly than those of the Geysir, and had any connexion with them, we only forced the eruptions twice at our arrival, and we noted, in the following table, all the great natural eruptions:—

Dates.	Forced Eruptions. Heights.	Natural Eruptions. Hours.
5th July, at 3 <sup>h</sup> 57 <sup>m</sup> P.M. ....	30 <sup>m</sup> ·210	
9th July, at 4 <sup>h</sup> 15 <sup>m</sup> P.M. ....		49 <sup>m</sup> ·375
9th July, at 9 <sup>h</sup> 25 <sup>m</sup> P.M. ....		43 <sup>m</sup> ·528
10th July, at 12 <sup>h</sup> 15 <sup>m</sup> A.M. ....		not measured.
12th July, at 8 <sup>h</sup> 2 <sup>m</sup> A.M. ....		not measured.
13th July, at 3 <sup>h</sup> 30 <sup>m</sup> P.M. ....		47 <sup>m</sup> ·426

It will be seen from this table that the natural eruptions of the Strokkur do not present more regularity than those of the Geysir, and that their coincidence with the latter, if it sometimes happens, is not general.

Some observers have admitted that there was a direct communication between the Strokkur and the Geysir: unfortunately I was not able, during my visit, to collect water from the Strokkur sufficiently freed from the earthy matters coming from the clods of earth which we threw upon it during the first days of our arrival, to bring it back and submit it to an accurate analysis; but the quantity of sulphuretted hydrogen, which I found to be 1<sup>cc</sup>·748 per litre, differs sufficiently from that which the water of the Geysir contains, to allow us to conceive that these two springs, subjected perhaps to the same cause of subterranean heat, are not, as has been thought, in immediate connexion.

LXI. *On the existence of a New Vegeto-Alkali in Gun-Cotton.* By ROBERT PORRETT, Esq.\*

AT the last meeting of this Society on the 7th instant, a joint communication from Mr. Teschemacher and myself “On the Chemical Composition of Gun-Cotton” was read†, and in the last paragraph but one thereof I suggested that a view

\* Communicated by the Chemical Society; having been read Dec. 21, 1846.

† This communication will be found at p. 273 of the present volume of this Journal.

might be taken of the arrangement of its elementary particles which would account for the singular fact of its non-acidity, by supposing it a compound of nitrous acid and oxide of lignin, the said new oxide being also supposed to possess alkaline properties.

It was before mentioned that the action of a small voltaic battery on gun-cotton gave but very slight indications of its decomposition, but that slight as they were they were rather favourable to the suggestion; the indifference of this compound to the electrical action of the battery I found was owing to its highly non-conducting nature, but I succeeded rather better by moistening the gun-cotton with acetic acid, and placing it in a thin layer between a plate of silver and one of zinc, completing the circuit with a copper wire and leaving them for forty-eight hours so arranged; at the end of that time, on dismounting this little voltaic arrangement, the inner surface of the silver plate was found to be encrusted with a small quantity of matter, which when dry looked white like starch, which powerfully restored the blue colour of reddened litmus paper, and which when heated to redness left a black carbonaceous residue; the minute quantity obtained made further experiments with it impossible, but it increased my desire to be able to procure the same substance by chemical means in any quantity that might be wanted.

After several trials I succeeded by the following process.

I took 2 ounces by measure of nitric acid of the specific gravity 1.45, to which I added 50 grains of gun-cotton; the mixture being gradually heated to  $100^{\circ}$  of Fahrenheit's scale and kept below  $180^{\circ}$ , became quite transparent and fluid, and all the cotton disappeared, being quietly dissolved without any evolution of gas; when a portion of the solution in this state was dropped into water a white precipitate was formed, having all the properties, structure excepted, of the original gun-cotton, the liquid was therefore a simple solution in acid, without decomposition; but on heating the liquid further up to  $240^{\circ}$ , deep red acid vapours were given off abundantly for a long time, and these collected in a receiver surrounded with ice proved to be hyponitrous acid. The same effect exactly took place when sulphuric acid was used instead of nitric at the same temperature, so that the hyponitrous acid was merely eliminated in both cases, and not formed from the excess of nitric acid used in the former instance; the cold and very concentrated nitric solution containing a great excess of acid was now brought nearly, but not quite, towards a neutral state by pouring into it a strong solution of subcarbonate of potash, after which a solution of bicarbonate of potash was used



until perfect neutrality was obtained: the liquid in this state became thickened, not only by the quantity of minute crystals of nitrate of potash formed in it beyond what it could hold in solution, but also by an abundant precipitate of a whitish-gray colour, consisting of the new alkali probably in a state of carbonate. The liquid was heated nearly to ebullition, during which its colour became darker, and it was set aside in order that the crystals of nitrate of potash formed on cooling might be separated and the liquid disembarassed of them as much as possible; after this it was evaporated to dryness by the heat of a water-bath, and the brownish residue was acted upon first by sulphuric æther, which however would not dissolve any portion of it; next by alcohol, specific gravity 0.813, which took up a very small quantity of the new alkali; then spirit of wine was tried; the solvent power was greater than that of the alcohol, but it had the disadvantage of taking up with the alkaline matter a very minute quantity of the nitrate of potash which accompanied it, so that I could not get an exclusive solvent for the former; the principal quantity by far of the carbonated alkaline oxide of lignin still remained, and I was obliged to use a small quantity of cold, distilled water for its solution, notwithstanding that the water took up at the same time some of the nitrate of potash and bicarbonate of potash remaining in the mass. This aqueous solution contains the new alkali in abundance, but mixed as stated with the other two salts, it acts very powerfully upon reddened litmus paper.

When a portion of the hyponitrous acid is neutralized by an aqueous solution of the new alkali, hyponitrite of oxide of lignin, probably identical with gun-cotton or with xyloidine, is reproduced and precipitated as an insoluble compound.

Thus I have verified the accuracy of my suggestion, excepting that instead of nitrous acid neutralized by a new alkali existing in gun-cotton, it is the hyponitrous acid so neutralized that constitutes that substance. For this new alkali I have proposed the name *Lignia*, and believe that it opens a wide field for scientific research.

I suspect that in the natural decay of woody fibre from leaves and rotting plants this alkali is produced in combination with acetic acid, and that the acetate of lignia so formed exists in the sap of all vegetables; further, that it is decomposed and deoxidated by light acting on the leaves and bark of trees, reproducing lignin for the assimilation and growth of the plant: its action also on the animal system deserves inquiry. I lay no stress however on these speculations, excepting so far as they may stimulate others to enter into in-

vestigations which it is impossible for me, consistently with my other duties, to find time for carrying on, and I leave the subject without reserve to those who have more leisure.

I would remark, that the composition of lignia must be that of lignin plus 2 atoms of oxygen given up by the nitric acid in becoming hyponitrous acid; and I would further observe, that the latter acid and lignia forming an insoluble compound, may probably be employed as mutual tests of each other's presence in combinations.

**LXII.** *On some new Researches in Animal Chemistry. Extracted from a Letter from Professor LIEBIG to Dr. A. W. HOFMANN\*.*

**I** AM at present occupied with the investigation of the constituents of the animal fluids which are found without the blood and lymphatic vessels. The fluid from flesh, for example, reacts strongly acid, and the question was, whence arose this acidity? After overcoming more difficulties than I have ever experienced in any investigation, I have for the first time indisputably proved that free lactic and phosphoric acid exist in the whole organism wherever muscle is found. How curious, that in the absence of all proofs on the part of the opponents of lactic acid, I should now demonstrate to them its existence in the flesh of oxen, fowls, calves, and sheep, by preparing and analysing the most beautifully crystallized zinc and lime salts! How wonderful, that in the animal organism acids and alkalies are found separated by a membrane, constituting myriads of little galvanic circles, which, as such, must produce chemical and electrical effects! To the latter class I refer all the observations of Matteucci, which can now be easily explained.

I have further found that the flesh of the muscles of oxen, fowls, sheep, calves, and the carnivorous pike contain creatin, prepared by Chevreul eleven years ago, and which, from Berzelius's not being able to reproduce it, has since then, in a measure, disappeared from the field of science. Creatin is a beautiful substance, having the formula  $C_8N_3H_{11}O_6$ . At the temperature of  $100^\circ C$ . it loses 2 equivs. of water, and becomes  $C_8N_3H_9O_4 = \text{glycocoll} + \text{ammonia}$  or  $\text{cafein} + \text{amidogen}$  and water. Heated in a stream of hydrochloric acid, creatin loses 4 equivs. of water and takes up 1 of hydrochloric acid. By this treatment, however, its nature is entirely altered, being now converted into a beautiful organic base, the properties of which are totally different from those

\* Communicated by the Chemical Society; having been read Dec. 21, 1846.

of creatin. It becomes now soluble in water, and forms with bichloride of platinum a fine crystallized double salt.

I have, finally, discovered two other new bodies in the same fluids, of which the one crystallizes in needles, the other in plates of the lustre of mother-of-pearl. Unfortunately I have obtained scarcely sufficient for two analyses from 40 lbs. of the flesh of oxen and 20 of that of fowls.

I see a boundless field before me, and doubt not that for every *quality* of the animal body, something which can be estimated *quantitatively*, will also be discovered to which it is indebted for its properties.

I have also satisfied myself as to the part which common salt plays in the bodies of animals. I have found that the fluids without the blood and lymphatic vessels contain only potash-salts, viz. chloride of potassium and phosphate of potash, with phosphate of magnesia, whilst the blood and lymph contain merely those of soda (phosphate of soda). If, therefore, the latter are indispensable to the formation of blood and the processes of life, it is evident that an animal on the continent, which finds in plants only potash-salts, should have chloride of sodium given to it, by means of which the phosphate of potash of the seeds and of the rest of the plant is transformed into chloride of potassium and phosphate of soda. I found further that the salt brine which flows from salted meat contained certainly alkaline phosphates, and that scurvy is hence easily explained by the deficiency in the salted meat of the alkaline phosphates necessary to the formation of blood. The soup from boiled meat contains the soluble phosphates of the flesh, and the meat itself the insoluble. Neither the soup nor the flesh alone can maintain the processes of life, but both must be taken together. The English have in this respect hit upon the proper practice. In a theoretical point of view their food is more correctly combined than that of the Germans.

Still more wonderful results have been obtained by the oxidation of casein by means of peroxide of manganese and sulphuric acid, by M. Gugelberger. Three products are obtained: the first of which is aldehyde, the second oil of bitter almonds, and the third a fluid æthereal body with a composition similar to metacetone. The aldehyde was analysed as aldehydite of ammonia, of which a considerable quantity was obtained. From oil of bitter almonds the most beautiful benzoic acid was produced by the action of chlorine.

From these results a sort of conception may be obtained how and wherefore many medicines have a certain deleterious or useful action.

Urea, creatin, glycocoll, leucin, cystin, &c. are organic bases, and only products of the animal body or its elements, and organic bases are partly poisonous, partly beneficial in their action. I have caused the new experiments of Mulder on his protein to be repeated. The substance prepared by Fleitmann in this laboratory, according to his new method, and supposed to be free from sulphur, still contains 1.5 per cent., as does likewise a similar preparation by Laskowski.

I beg of you to communicate this short notice to the Chemical Society, of which I have the honour to be a member.

### LXIII. On the Salts of Sulphurous Acid.

By J. SHERIDAN MUSPRATT, Esq., Ph.D.\*

I HAVE been induced to return to this subject by a paper lately published by Dr. Rammelsberg in Poggendorff's *Annalen* †. In his treatise he differs slightly from me in the quantities of water contained in some of the sulphites, but as he invariably took his water as loss and as mine was generally determined by combustion with chromate of lead, I did not think it worth while going over all the analyses that I had performed in Giessen, being so thoroughly convinced of the accuracy of those results. The only point where we materially disagree is regarding the constitution of the red sulphite of copper, and which will be subsequently discussed under that head. When I first undertook, in Baron Liebig's laboratory, the investigation of the salts of sulphurous acid, very little was known of their constitution; but since that time they have occupied the attention of numerous chemists, and are now invested with as much interest as the compounds of any other acid. They are very readily decomposed either by moisture or heat, and on this account may have led different chemists to the assumption of various formulæ for the same salt. For example, there is scarcely any salt of this acid which does not contain traces of sulphate, which are always overlooked in the analysis; and moreover, unless great care is exercised in oxidizing the sulphurous acid when its quantity is to be determined, serious errors may arise. I shall now proceed to the description of the sulphites under their respective heads.

*Sulphite of Soda.*—This salt is obtained by transmitting sulphurous acid through a solution of carbonate of soda until the liquid becomes acid, and then allowing the solution to

\* Communicated by the Chemical Society; having been read Dec. 21, 1846.

† lxvii. pp. 245, 391; or No. 89 of the Chemical Gazette, July 1, 1846, p. 254.



repose for some days under a bell-jar over sulphuric acid. The crystals possess a cooling and sulphurous taste, and are slightly alkaline to test-paper.

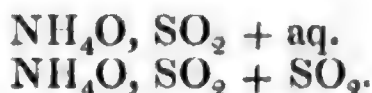
*Analysis*:—7.10 grs. burned with chromate of lead gave 4.19 grs. of water = 59.01 per cent.

Formula,  $\text{NaO}, \text{SO}_2 + 10 \text{ aq.}$

Vauquelin has described a sulphite of soda of the following constitution:— $\text{NaO}, \text{SO}_2 + 8 \text{ aq.}$ ; and Rammelsberg analysed one which agreed with the annexed formula:  $\text{NaO}, \text{SO}_2 + 7 \text{ aq.}$  The varying proportions of water can only be accounted for by the different temperatures at which the salts have been formed.

*Bisulphite of Soda*.—Dr. Rammelsberg obtained an acid salt agreeing with the formula  $2(\text{NaO}, 2\text{SO}_2) + \text{aq.}$  I find however on referring to his results that the formula  $\text{Na SO}_2 + \text{HO SO}_2$ , which I gave in my former paper, corresponds quite as well with his numbers as the one given by him.

*Sulphite of Ammonia*.—I have not yet succeeded in procuring from an aqueous solution an ammoniacal sulphite of sufficient stability for analysis. The salts formed in alcohol and æther, and heretofore described\*, possessed the subjoined formulæ:—



I have produced another ammonia sulphite by passing sulphurous acid into a very strong solution of caustic ammonia until there was no smell either of the acid or alkali, and then adding cautiously absolute alcohol. After a short time a brilliant white saline mass of crystals subsided, which were allowed to remain undisturbed for twenty-four hours. I threw the whole on to a filter and dried the crystals over sulphuric acid. This salt is alkaline, and evolves traces of ammonia.

*Analysis*:—23.09 grs. of the salt gave 33.88 grs. sulphite of barytes = 9.32 sulphurous acid or 40.36 per cent.

The formula  $2(\text{NH}_4\text{O}, \text{SO}_2) + \text{NH}_3 + 3 \text{ aq.}$  requires 40.09 per cent. sulphurous acid.

*Sulphites of the Protoxide of Iron*.—Finely divided metallic iron dissolves pretty readily in sulphurous acid, imparting to the liquid a slight green tinge. When this liquid is evaporated *in vacuo*, very small and almost colourless crystals are obtained, which are but slightly soluble in water. Exposed to the air in a dry state they are not readily altered, but if moist are rapidly converted into sulphate. I found my sulphurous acid, on analysing this compound, to agree so closely

\* *Ann. Chem. und Pharm.*, Bd. 1. p. 285.

with what Fordos and Gelis had previously given, that I neglected determining the iron and water. 5.65 grs. gave 7.00 grs. sulphate of barytes = 1.92 grs. sulphurous acid or 33.98 per cent. Centesimally represented,—

	Theory.	F. & G.	Mean.
1 eq. protoxide of iron	439.21	37.28	37.23
1 ... sulphurous acid .	401.17	34.06	34.04
3 ... water . . . .	337.44	28.76	28.73
	<u>1177.82</u>	<u>100.00</u>	<u>100.00</u>

Formula,  $\text{FeO}, \text{SO}_2 + 3 \text{ aq.}$

*Sulphites of the Peroxide of Iron.*—When sulphurous acid is passed through water holding in suspension freshly-precipitated peroxide of iron, the whole readily dissolves, and the liquid acquires a blood-red colour, which disappears on exposure to the air. If this solution be boiled so as to expel the uncombined acid, and then left to the atmospheric action for some days, it acquires a light brownish colour, and deposits a reddish brown compound containing sulphurous acid and peroxide of iron. Dr. Kœne of Brussels expresses the constitution of this salt by the formula



I found that when caustic potash was added very gradually to the blood-red solution above-mentioned, a yellowish crystalline precipitate formed, the liquid remaining still acid. This compound, after filtration, edulcoration and desiccation, gave the following results :—

9.01 grs. gave 2.18 grs. peroxide of iron.

6.21 grs. gave 6.80 grs. sulphate of barytes = 1.87 grs. sulphuric acid.

10.11 grs. gave 1.50 gr. water.

Composition per cent. :—

	Theory.	Found.
3 eqs. Sulphurous acid.	1203.51	30.66
2 ... Potash . . . .	1179.84	30.06
1 ... Peroxide of iron.	978.42	24.93
5 ... Water . . . .	562.50	14.35
	<u>3924.27</u>	<u>100.00</u>

These numbers agree with the formula

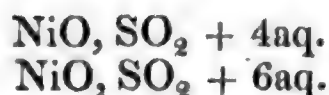


This double sulphite of iron and potash appears to be Kœne's salt, in which 2 equivalents of water are replaced by 2 equivalents of sulphite of potash.

The sulphite of the peroxide of iron forms a salt with sulphite of ammonia, but it is very difficult to get a sufficient

quantity of it for analyses. It contains 33·68 per cent. of peroxide of iron.

*Sulphites of Nickel.*—The two salts which I described in my first paper on the sulphites possessed the subjoined formulæ :—



The latter agrees with the results of Fordos, Gélis and Rammelsberg. I have again prepared the former by transmitting a stream of sulphurous acid gas into water holding in suspension carbonate of nickel. When the whole had dissolved, the solution was filtered and boiled, which liberated a mass of small greenish crystals, almost insoluble in water.

6·02 grs. salt burned with chromate of lead gave 2·08 grs. water = 34·55 per cent.

The formula  $\text{NiO, SO}_2 + 4\text{aq.}$  affords 34·06 per cent. of water.

Dr. Böttinger has described a double sulphite of nickel of the following constitution :—



*Sulphites of Cadmium.*—Carbonate of cadmium in water is readily dissolved by sulphurous acid, and when the resulting solution is treated with absolute alcohol an aluminous-looking precipitate appears, which when dried and heated in a test-tube affords no water—it must be the anhydrous sulphite of cadmium. If however the gelatinous precipitate is allowed to remain in the alcoholic menstruum for some days it disappears, and there deposit in its stead beautiful silvery-looking crystals, which turn out to be the same salt as that obtained by Fordos and Gélis when dissolving the metal in the acid.

*Analysis* :—6·79 grs. of salt gave 6·94 grs. sulphate of barites = 1·90 sulphurous acid.

8·11 grs. salt gave 1·29 grs. water.

Centesimally represented,—

	Theory.	Found.	
1 eq. Sulphurous acid . .	401·17	28·19	27·98
1 ... Oxide of cadmium .	796·77	55·99	
2 ... Water . . . . .	224·96	15·82	15·90
	<hr/> 1422·90	<hr/> 100·00	

Formula,  $\text{CdO, SO}_2 + 2\text{aq.}$

It is oxidized very slowly when exposed to the air in a dry state; is sparingly soluble in water, and readily dissolved by dilute acids. I have not formed any double salts with the sul-

*Phil. Mag.* S. 3. Vol. 30. No. 203. June 1847. 2 F

phite of cadmium. Dr. Rammelsberg analysed a salt which gave him the annexed formula,—



*Sulphites of Copper.*—The brilliant red salt which is formed either by boiling together solutions of sulphate of copper and sulphite of ammonia, or passing sulphurous acid into water, holding in suspension oxide of copper, was first analysed by Chevreul, whose numbers agree with the formula—



I gave the annexed formula for this salt,\*—



Böttinger† analysed the compound after me, and his results favour the formula—



Rammelsberg however has stated that the salt is a double one, and constituted as follows:—



I have reprepared the red salt by passing a stream of sulphurous acid into water containing pure oxide of copper. When the whole had dissolved, the liquid was boiled. The red compound deposited in pretty large crystals, which were thrown upon a filter and effused with cold water *until the filtrate did not afford the slightest milkiness with a solution of chloride of barium*. They were then dried *in vacuo*. I tested the salt for *sulphuric acid* before submitting it to analysis, and found traces present, even after taking every precaution.

#### Analyses.

				Per cent.
I.	8.84 grs. gave	9.30 BaO, SO <sub>3</sub> =2.55	SO <sub>2</sub> =28.84	
II.	7.41 ...	8.32 BaO, SO <sub>3</sub> =2.28	SO <sub>2</sub> =30.76	
III.	6.52 ...	7.11 BaO, SO <sub>3</sub> =1.95	SO <sub>2</sub> =29.90	
IV.	9.62 ...	10.03 BaO, SO <sub>3</sub> =2.759	SO <sub>2</sub> =28.68	
V.	18.47 ...	20.71 BaO, SO <sub>3</sub> =5.69	SO <sub>2</sub> =30.80	
I.	13.41 grs. salt with PbO, CrO <sub>3</sub> gave	1.38 HO=	10.29	
II.	11.13 ...	... PbO, CrO <sub>3</sub> ...	0.97 HO=	8.71
III.	10.74 ...	... PbO, CrO <sub>3</sub> ...	1.02 HO=	9.49

Mean of the above per cent.

Sulphurous acid	. . .	29.796
Water	. . .	9.496
Base or loss	. . .	60.708
		<hr/> 100.000

\* *Ann. Chem. und Pharm.*, bd. l. p. 285.

† *Ibid.* bd. li. p. 412.



The copper has also been determined in the salt by my brother Frederick and myself.

					Per cent.
F. M.	6.94	grs. salt	gave	4.70	CuO=4.22 Cu <sub>2</sub> O=60.80
Dr. M.	11.41	...	...	7.48	CuO=6.72 Cu <sub>2</sub> O=58.88

It is evident from the preceding results, *that if the base of this salt be only suboxide of copper*, its constitution may be assumed as the following,—



Dr. Rammelsberg found the annexed quantities of sulphurous acid,—

35.12      35.06      34.47,

which I can only account for by his salt not being so well washed as mine. My salt dissolves in hydrochloric acid, affording a colourless liquid, and when a weak solution of potash is added to this, a white precipitate subsides. Yellow prussiate of potash gives a whitish precipitate in the acid solution, soluble in an excess of muriatic acid. When the red salt is placed on a filter and affused with boiling water until the filtrate affords no milkiness with barytes, half of its sulphurous acid is removed.

7.37 grs. gave 4.41 grs. BaO, SO<sub>3</sub>=1.21 SO<sub>2</sub>=16.41 per cent.

This quantity favours the formula—



Böttiger states in his paper, “That by long boiling with the liquid this salt loses sulphurous acid and becomes lighter in colour, which confirms my own observation. The analysis of such a precipitate gave me 66.60 per cent. of Cu<sub>2</sub>O, which will account for the varying results obtained by different chemists when analysing this red salt.”

*Sulphite of Copper and Soda.*—When rather strong solutions of sulphate of copper and sulphite of soda are mixed together and treated with absolute alcohol, a dark reddish-looking liquid, which greatly resembles nitrobenzide, subsides, and if this be collected and left *in vacuo* for some time, fine yellow crystals appear, which after washing and desiccation afforded the subjoined results:—

						Per cent.
16.40	grs. salt	gave	7.27	grs. HO		=44.32
15.17	...	...	1.81	...	CuO	=1.62 Cu <sub>2</sub> O=10.67
6.13	...	...	5.44	...	BaO, SO <sub>3</sub> =1.49 SO <sub>2</sub>	=24.30

These numbers agree very closely with the formula—



if there was no uncombined sulphite of soda present.

2 F 2

Represented per cent.—

		Theory.	Found.
1 eq.	Suboxide of copper	891.39	9.36
6 ...	Sulphurous acid .	2407.02	25.62
5 ...	Soda . . . . .	1954.50	20.52
38 ...	Water. . . . .	4274.24	44.32
		<u>9527.15</u>	<u>100.00</u>

This salt, when heated in a test-tube, first gives off large quantities of water, then sulphurous acid, and the residue consists principally of sulphates of copper and soda, with traces of sulphur, &c.

*Sulphite of Copper and Ammonia.*—I mixed a solution of sulphate of copper with a large excess of sulphite of ammonia, then put the mixture into a long wide tube containing a thermometer, and applied heat. I removed the solution from the sand-bath at 136° F., because at this temperature silvery-looking flakes began to deposit, which powerfully refracted the light. In a short time a large quantity of a shining compound had separated, which when dried over sulphuric acid and submitted to analysis gave the annexed results:—

8.690 grs. salt gave 10.73 sulphate of barytes = 2.952 grs. sulphurous acid = 33.97 per cent.

As these numbers agreed so closely with Böttinger's formula\*,—



I thought further determinations unnecessary. When this compound is heated with water, it is decomposed and the red gritty salt separates. Great care is required in the preparation of this sulphite, for it is difficult to prevent some of the red salt depositing with it. The best way to obtain it in large quantities, is to heat slightly a strong solution of sulphate of copper with an excess of bisulphite of ammonia, then cork the vessel and allow the mixture to remain undisturbed for some days.

All the sulphurous acid for my experiments was obtained by the deoxidation of sulphuric acid by means of metallic copper,—



\* Liebig's *Annalen*, bd. li. p. 411.

LXIV. *Analysis of the Bohemian Glass as found in the Combustion Tubes employed in Organic Analysis.* By Mr. THOMAS ROWNEY\*.

THE difficultly fusible variety of Bohemian glass has been repeatedly the subject of chemical investigation. We are indebted to Berthier†, Dumas‡, Gras§, and lately Peligot¶ for an analysis of this substance. The glass analysed by these chemists was of the kind usually employed in the manufacture of Bohemian goblets, an art in which Bohemia has excelled for centuries.

It appeared desirable to compare with the results of these analyses the composition of the glass which is used for the combustion of organic substances, and which likewise is chiefly manufactured in Bohemia. The properties most valued in this variety of glass are essentially different from those sought for in other kinds, and it was to be expected that this material, which has so greatly contributed to the progress of organic chemistry, and upon which the German manufacturers have of late bestowed so much attention, would also show some marked peculiarity of composition.

The following analysis was made in the laboratory of the Royal College of Chemistry. The glass was selected from the stock of the laboratory, recently imported from Germany, and which numerous trials had proved to be of first-rate quality. The tubes, though perfectly tractable in an energetic blow-pipe flame, scarcely altered their form during the longest combustion in a strong charcoal fire; they never cracked during the process, even on suddenly increasing the temperature, and not uncommonly the same tube could be employed repeatedly in analysis.

A careful qualitative examination of the specimen having proved the presence of silicic acid, lime, sesquioxide of iron, alumina, oxide of manganese, magnesia, soda and potassa, the quantitative determination was proceeded with.

*a. Determination of Silicic Acid.*

The finely-powdered glass was ignited with carbonate of soda, treated with hydrochloric acid in excess, evaporated to dryness, once more ignited and digested with hydrochloric acid, after which all the silicic acid remained insoluble; it was collected on a filter, washed, ignited and weighed.

I. 2.1890 grms. of glass gave 1.5946 grm. silicic acid.

II. 2.1284 grms. of glass gave 1.5628 grm. silicic acid.

\* Communicated by the Chemical Society; having been read Dec. 21, 1846.

† Gmelin's *Handbuch*, vol. ii. 366.

‡ Dumas' *Traité de Chimie*, vol. ii. 528.

§ *Ibid.*

*b. Determination of Lime.*

The acid filtrate from the silicic acid was neutralized by ammonia, then slightly acidulated with acetic acid and precipitated by oxalate of ammonia, and the oxalate of lime converted into carbonate in the usual manner.

I. 2.1890 grms. of glass gave 0.4203 grm. of carbonate of lime.

II. 2.1284 grms. of glass gave 0.3843 grm. of carbonate of lime.

*c. Determination of Alumina and Sesquioxide of Iron.*

To the liquid filtered from the precipitate of oxalate of lime (*b.*), containing a large quantity of chloride of ammonium, ammonia was added in excess, by which alumina and sesquioxide of iron were precipitated and the oxide of manganese and magnesia were retained in solution. The joint precipitate of alumina and sesquioxide of iron was collected, washed, ignited and weighed; then dissolved in hydrochloric acid and thrown down with potassa, which kept the alumina in solution. The precipitate of sesquioxide of iron (thus obtained), once more dissolved in hydrochloric acid and reprecipitated by ammonia, was now collected, ignited and weighed; the difference in the amount of the two weighings gave the amount of the alumina thus:—

2.1890 grms. of glass gave 0.0096 grm. of mixed oxides and 0.0029 grm. of sesquioxide of iron; the difference, 0.0067 grm., is the amount of alumina.

*d. Determination of Oxide of Manganese.*

This oxide was thrown down by carbonate of soda as carbonate of manganese, from the filtrate, separated from the oxide of iron and alumina (*c.*); the precipitate was washed, dried, ignited, and calculated as the compound oxide and sesquioxide of manganese ( $\text{Mn}_3\text{O}_4$ ).

I. 2.1890 grms. of glass gave 0.0114 grm. of the above-named double oxide.

II. 2.1284 grms. of glass gave 0.0160 grm. of the double oxide.

*e. Determination of the Magnesia.*

The filtrate from the carbonate of manganese (*d.*) was precipitated by phosphate of soda; the phosphate of magnesia and ammonia was washed, dried, ignited and weighed as pyrophosphate of magnesia.

I. 2.1890 grms. of glass gave 0.0127 grm. of pyrophosphate of magnesia.



II. 2.1284 grms of glass gave 0.0177 grm. of pyrophosphate of magnesia.

*f. Determination of the Alkalies.*

To determine the amount of the alkalies, a portion of the glass was fused with hydrate of baryta; the fused mass was digested in hydrochloric acid, evaporated to dryness and ignited, the residue again digested in hydrochloric acid, and then filtered from the insoluble silicic acid. Carbonate of ammonia was then added to the filtrate, to separate the alumina, sesquioxide of iron, baryta and lime; the solution filtered from the precipitate was evaporated to dryness and ignited, to drive off the ammoniacal salts, and the magnesia separated by means of baryta; the excess of baryta was separated by means of carbonate of ammonia, and filtered. The filtrate, evaporated to dryness and ignited, yielded the mixed chlorides of the alkaline metals; they were dissolved in water, bichloride of platinum added, and the solution evaporated to dryness on a water-bath; the residue was digested in strong alcohol, when the potassio-chloride of platinum remained undissolved; it was collected on a weighed filter, washed with alcohol, and dried in a water-bath until it ceased to lose weight. The filtrate from the potassio-chloride of platinum was evaporated to dryness, and, after the addition of some oxalic acid to the residue, ignited till it was completely decomposed; the residue was digested in water, filtered, and the filtrate evaporated to dryness in a weighed capsule; its increase in weight gave the quantity of chloride of sodium.

1.5645 grm. of glass gave 0.9330 grm. of potassio-chloride of platinum, and 0.0905 grm. of chloride of sodium.

From the results enumerated, the calculated per-centage is as follows:—

	I.	II.	Mean.
Silica . . . . .	72.84	73.42	73.13
Lime . . . . .	10.75	10.11	10.43
Alumina . . . . .	0.30	...	0.30
Peroxide of iron . . .	0.13	...	0.13
Magnesia . . . . .	0.21	0.30	0.26
Protoxide of manganese	0.37	0.54	0.46
Soda . . . . .	3.07	...	3.07
Potash . . . . .	11.49	...	11.49
	99.16		99.27

The amount of oxygen in the bases stands to the amount of oxygen in the silicic acid nearly as 1 : 6.

From the preceding analysis, it results that the composition of the glass for combustion-tubes does not differ very much

from the material employed in goblets, as may be seen from the following comparison with the results of the chemists formerly named:—

	Berthier.	Dumas.	Gras.	Peligot.
Silicic acid . . . .	71·7	69·4	71·6	76
Lime . . . .	10·3	9·2	10·0	8
Alumina . . . .	0·4	9·6	2·2	1
Sesquioxide of iron .	0·3	...	3·9	
Oxide of manganese	0·2	...	0·2	
Magnesia . . . .	...	...	2·3	
Soda . . . .	2·5	...	...	
Potassa . . . .	12·7	11·8	11·0	15
	<u>98·1</u>	<u>100·0</u>	<u>101·2</u>	<u>100</u>

In one of the last numbers of Dr. Otto's German translation of Professor Graham's Elements of Chemistry, recently published, I find the analysis of a combustion-tube enumerated, performed in the laboratory of Dr. Otto, which has not yet been published in the Journals. He obtained the following results:—

Silicic acid . . . .	74·0
Lime . . . .	7·2
Alumina . . . .	} 0·1
Sesquioxide of iron . .	
Potassa . . . .	18·5
	<u>100·0</u>

The oxygen in the bases of this glass stands to the oxygen in the silicic acid likewise as 1 : 6. Soda is entirely absent, and completely replaced by potassa. The presence however of a small amount of soda does not seem to be injurious to the glass.

**LXV. On a Generalization of a Theorem of Euler in reference to the Products of the Sums of Squares. By J. R. YOUNG, Professor of Mathematics in Belfast College\*.**

THE recent researches of Sir W. R. Hamilton, of Professors Graves and De Morgan, and of Messrs. J. T. Graves and Cayley, in reference to the new analytical theory of imaginary quantities, have revived attention to Euler's theorem, that the sum of four squares multiplied by the sum of four squares produces the sum of four squares; to which theorem an extension was given by Lagrange, by the introduction of coefficients into the component squares.

These recent researches have suggested the inquiry as to

\* Communicated by the Author.

whether or not the theorem admits of generalization; and in the *Philosophical Magazine* for April 1845, Mr. J. T. Graves announced that he had arrived at the truth that "the product of two sums of eight squares is a sum of eight squares;" but adds, that "the full statement and proof of the theorem must be reserved for another time." I have anxiously inquired at every likely source of information to which I have access, for the publication of this proof; and am disposed to conclude, from the result, that such publication has not yet been furnished.

I have been thus led to enter into an independent investigation of the subject; and find that the theorem holds not only for four and eight, but also for sixteen, and indeed for any number of squares expressed by an integral power of 2. This investigation I propose to forward to the British Association, at its meeting in June, provided I be assured that I have not been anticipated in the generalization here announced. By thus alluding to the results at which I have arrived, in the pages of this Journal, I shall afford timely opportunity for the information being communicated to me, if the above-mentioned extension of Euler's theorem has ever as yet been published.

The notation in which the subordinate theorems and the general theorem is announced is this, viz.

$\Sigma_2(\square) \times \Sigma_2(\square') = \Sigma_2(\square'')$ ,  $\Sigma_4(\square) \times \Sigma_4(\square') = \Sigma_4(\square'')$ ,  
and generally

$$\Sigma_n(\square) \times \Sigma_n(\square') = \Sigma_n(\square''),$$

where  $n$  is any integral positive power of 2.

The theorem holds too when certain coefficients are introduced: thus, taking eight squares, it is true that

$$\begin{aligned} & (s^2 + bt^2 + cu^2 + bcv^2 + bcw^2 + cx^2 + by^2 + z^2) \\ & \times (s'^2 + bt'^2 + ct'^2 + bcv'^2 + bcw'^2 + cx'^2 + by'^2 + z'^2) \\ & = s''^2 + bt''^2 + ct''^2 + bcv''^2 + bcw''^2 + cx''^2 + by''^2 + z''^2. \end{aligned}$$

And also that

$$\begin{aligned} & (s^2 + \alpha t^2 + \alpha^2 u^2 + \alpha^3 v^2 + \alpha^4 w^2 + \alpha^5 x^2 + \alpha^6 y^2 + \alpha^7 z^2) \\ & \times (s'^2 + \alpha t'^2 + \alpha^2 u'^2 + \alpha^3 v'^2 + \alpha^4 w'^2 + \alpha^5 x'^2 + \alpha^6 y'^2 + \alpha^7 z'^2) \\ & = s''^2 + \alpha t''^2 + \alpha^2 u''^2 + \alpha^3 v''^2 + \alpha^4 w''^2 + \alpha^5 x''^2 + \alpha^6 y''^2 + \alpha^7 z''^2. \end{aligned}$$

And the same has place for any number of squares expressed by a power of 2.

Belfast, May 8, 1847.

LXVI. *On a Method for the Analysis of Bodies containing Nitric Acid, and its application to Explosive Cotton.* By WALTER CRUM, F.R.S.\*

AT the first meeting of the present Session of the Philosophical Society, I gave an account of some experimental inquiries into the nature of gun-cotton, a body whose composition was then little known. I had at that time chiefly occupied myself with its nitrous contents, and described a method by which some approximation could be made to a quantitative result for nitric acid. On resuming the subject, I found that much was wanting to render the method a rigorously accurate one; and I shall now relate what I have since done to simplify and complete it. I shall first, however, give an account of its application to nitrate of potash,—a body of known composition, and easily obtained in a state of purity,—to which I had recourse as a means of proving the accuracy of the method, and detecting any fallacy to which it might be liable.

*Nitric Acid in Nitrate of Potash.*—The salt I employed was purified by repeated crystallization, and fused at little more than its melting heat. A glass jar, eight inches long and an inch and a quarter in diameter, is filled with, and inverted over mercury. A single lump of the fused nitrate, weighing about six grains, is let up into it, and afterwards fifty grains of water. As soon as the nitrate is dissolved, 125 grains of sulphuric acid, ascertained to be free from nitric acid, are added. By the action of the mercury upon the liberated nitric acid, deutoxide of nitrogen soon begins to be evolved, and usually in about two hours, without the application of heat, the whole of the nitric acid is converted into that gas. Occasional agitation is necessary, and it is easily performed by giving a jerking horizontal motion to the upper part of the jar. The surface of the sulphuric acid is then marked, and three-fourths of a cubic inch of solution of sulphate of iron, recently boiled, let up into the jar. The gas is rapidly absorbed, except a small portion at last, which must be left several hours to the action of the solution, or be well agitated in a smaller tube with a fresh portion of it. No correction of the nitric oxide has to be made for moisture; for the mixture of acid and water which I employed, as I ascertained by direct experiment, has no perceptible force of vapour. In one experiment,

5.40 grains nitrate of potash yielded

4.975 cubic inches of gas, at 60° Fahr., and bar. 30 inches.

The residue not absorbable by sulphate of iron, was

\* Read before the Philosophical Society of Glasgow, April 14, 1847, and communicated by the Author.



0.015 cubic inch; leaving  
 4.96 cubic inches of nitric oxide = 1.594 grains  $\text{NO}_2$ , and  
 which correspond to  
 2.869 grs. nitric acid, or 53.13 per cent. of the nitrate of potash.  
 Four consecutive experiments made in this manner yielded—

53.13  
 53.14  
 53.73  
 53.29

Mean . . . . 53.32

or leaving out the third experiment, 53.19.

The calculated per-centage of nitric acid in nitrate of potash, the acid being represented by 6.75, and the potash by 5.8992, is 53.36\*.

In order further to determine whether the presence of organic matter would interfere with the liberation of the nitric oxide, the experiment was repeated with the addition of three grains of cotton wool, which was first dissolved in the sulphuric acid; the result was 53.24.

Other nitrates are analysed in the same manner. For salts in powder, which it is difficult to pass through mercury without loss, I cut a quarter-inch glass tube into little cylinders for them, of half an inch long, and close up the ends with thin paper fastened with gum. In the analysis of numerous samples of crude nitrates, the residue, which is azote, may be taken as a constant quantity, and the jar graduated in such a manner that the volume of gas may be read off at once as the per-centage of nitric acid.

*Preparation of Gun-Cotton.*—The cotton I employed was fine Sea Island. It was first thoroughly carded, and then bleached, by boiling in caustic soda, and steeping in solution of bleaching powder; then caustic soda again, and afterwards weak nitric acid. It was well washed and beaten in a bag with water after each operation. When burnt, 10,000 parts left 9 of ashes. It was considered to be lignine, nearly pure.

The cotton, dried and carded after bleaching, was exposed in parcels of 10 grains each, for several hours, to the heat of a steam-bath, and each parcel was immersed, while hot, in an ounce measure of the following mixture:—

One measure sulphuric acid, spec. grav., 1.840.

Three measures of pale lemon-coloured nitric acid, of 1.517. After one hour it was washed in successive portions of water, till no trace of acid remained, and dried in the open air.

\* By Thomson's numbers the per-centage of nitric acid in nitre is 52.94; by Berzelius, 53.44.

30 grs. of bleached cotton wool, dried at 65° F., became, after being some hours in a steam bath,

28.32 grs., and lost, therefore, 5.6 per cent. of water. It increased to

51.08 grs. when made into gun-cotton, and dried in the open air.

Dried further *in vacuo*, over sulphuric acid, it was reduced to 50.40 grs., and lost therefore 1.33 per cent. of water.

100 of dry cotton produced 177.9 of dry gun-cotton.

The gun-cotton thus prepared is whiter, but less transparent, than the original bleached wool. It appears to be little liable to change, but a slight elevation of temperature causes a commencement of decomposition, and the colour becomes more or less brown. It is much less tenacious than cotton wool. Dissolved in nitric acid, and tested with chloride of barium, it gives no indication of sulphuric acid.

The increase of weight above stated is the greatest I have been able to obtain; and I had completed its analysis in the manner I shall describe, when I found reason to believe that it still contained a portion of unaltered cotton. With a view to saturate that portion, it was immersed a second time, and for twenty-four hours, in the same mixture of acids, but without yielding any greater quantity of nitric acid.

An immersion of one hour in nitric acid alone gave a better result. It lost in weight by this second process 0.47 per cent. It was little altered in appearance, but after being dried in the open air, it lost in the air-pump only 0.69 per cent., instead of 1.33, as in the former case. It is this substance of which I shall now relate the analysis.

*Ashes in Gun-Cotton.*—Sixteen grains of gun-cotton were dissolved in nitric acid. The solution being evaporated by degrees, and burnt to ashes, left 0.035 gr. of a reddish ash, or 0.22 per cent.

*Nitric Acid in Gun-Cotton.*—In this process the same apparatus is employed as for nitrate of potash. About 6 grains of the gun-cotton, containing a known quantity of water, is collected into a ball—squeezed between the finger and thumb to free it as much as possible from air—and let up into the jar, over the mercurial trough. 125 grains of sulphuric acid are added to it. Nitric acid is liberated, and, being acted upon by the mercury, produces nitric oxide. After one hour, when about three-fourths of the whole gas has been evolved, and the gun-cotton is entirely dissolved, 50 grains of water are added. In another hour the increase of gas ceases; in a few hours more its boundary is noted, then treated with sulphate of iron, and the residue measured. It consists of azote

from the common air introduced with the gun-cotton, and a minute portion also, which is always accidentally entangled between the mercury and the glass. Its oxygen is absorbed by the mercury, when in the state of nitrous acid.

In one experiment—

6.02 grs. of gun-cotton = 5.978, after being dried over sulphuric acid *in vacuo*, and =

5.964 grs., after deducting ashes, produced

5.513 cubic inches of gas, bar. 30 in., therm. 60°, of which 0.08 was left by sulphate of iron.

5.433 cubic inches, therefore, were deutoxide of nitrogen, =

1.746 grs.  $\text{NO}_2$ , which represent

3.143 grs. of nitric acid, or 52.70 per cent.

Another experiment gave 52.68 per cent.

The gun-cotton prepared by a single immersion gave only 51.42 per cent. of nitric acid.

*Carbon in Gun-Cotton.*—Having failed to obtain good results by burning this substance with oxide of copper, I used chromate of lead, precipitated from the nitrate, and heated to redness. I employed for the combustion an apparatus which I used many years ago for the analysis of indigo, and I still find it very convenient for substances which do not require a strong red heat. It consists of a tube of hard glass, eight inches long and three-eighths of an inch in diameter; the gases from which are led by a small bent tube under the receiver in a mercurial trough.

1 inch at the closed end of the tube is filled with 8 grains chlorate of potash, ground with chromate of lead.

$4\frac{1}{2}$  inches are filled with chromate of lead, among which is ground to powder 3 grains of the gun-cotton.

$1\frac{1}{2}$  inch contains chromate of lead that has been used to wash out the mortar.

A glass plug separates these materials from the perforated cork which joins the two tubes. The materials are gradually heated with broad-wicked spirit-lamps. Carbonic acid comes over, mixed, when in the receiver, with nitric oxide and the azote of the apparatus; and when all the gun-cotton is consumed, the lamps are extended to the chlorate of potash. The oxygen gas thus liberated, which in other cases is useful to consume carbonaceous matter that may have escaped the chromate, expels in this case all remains of carbonic acid, and passing itself into the receiver, mixes there with the nitric oxide, and causes its entire absorption by the mercury. Oxygen and azote are then the only gases left along with the carbonic acid, and as those are not absorbable, an addition of half a cubic inch of solution of caustic soda indicates exactly the quantity of carbonic acid present.

In one experiment, 2.993 grs. of gun-cotton (after deducting water and ashes) yielded 7.952 cubic inches of gas, of which 5.733 was carbonic acid, = 0.739 grs. carbon, or

24.69 per cent.

A second experiment gave 25.16

Mean . . . 24.92

*Elements of Water in Gun-Cotton.*—To burn gun-cotton for the purpose of collecting its oxygen and hydrogen in the state of water, I ground up 10 grains of it with pounded flint, and used the combustion tube already described, having attached to it a chloride of calcium tube, and afterwards a tube with asbestos moistened with sulphuric acid. But along with water, ammonia and other matters were obtained, which destroyed the result. I next used a thin glass tube of a foot and a half long, bent so that a foot in the middle of it could dip into cold water. Such water as would condense at 65° Fahr. was collected. The gas was led through it into a mercurial trough, and measured. A trace of cyanogen appeared in the last portions of gas, while the oxygen from the chlorate of potash was burning a quantity of charcoal that had escaped the nitric acid.

After the experiment, the refrigerating tube was found studded with large crystals of bicarbonate of ammonia. It contained very little water in the liquid state. The crystals and the liquid were washed out with more water, converted into muriate of ammonia, and found to contain 0.675 gr.  $\text{NH}_3$ ,  $2\text{CO}_2$ , the hydrogen of which represents 0.299 gr. of water. There were besides 2.025 grs. water in the tube. In the 22 inches of gas which were obtained, assuming it to be saturated with moisture, which is doubtful, there was 0.088 gr. of water—making in all

2.412, from which must be deducted

0.160 gr. hygrometric water in the gun-cotton and in the flint, leaving

2.252 for the water in 9.92 grs. of dry gun-cotton or 22.70 per cent.

In a second experiment, where the only difference was in having moistened cotton for the gas to pass through before entering the mercurial trough, the water obtained only amounted to 20.61 per cent. I did not proceed further. These were the two last of a number of experiments, and the determinations of nitric acid and carbon are so much more satisfactory, that I prefer resting the water contents upon their results.

Purified cotton wool (lignine) is composed of  $\text{C}_{12} \text{H}_{10} \text{O}_{10}$ .



During its transformation into gun-cotton, there is no indication of change in the proportions of its oxygen and hydrogen. The difference, therefore, between the weight of the substance employed and that of the nitric acid and carbon found by experiment, is oxygen and hydrogen in the proportions which form water.

The experiments I have related give the following for the composition of gun-cotton:—

52·69 nitric acid,  
24·92 carbon, and leave  
22·39 for the elements of water.

100·00

These numbers are nearly in the proportions of 12C, 7HO, 3NO<sub>5</sub>.

Found.	Calculated.
52·69	52·69 = 3NO <sub>5</sub> .
24·92	23·41 = 12C.
22·39	20·49 = 7HO. ,
100·00	96·59

Leaving a remainder of 3·41 per cent., consisting of 1·51 carbon, and 1·90 water. These however, are nearly the proportions which form lignine.

Found.	Calculated.	
1·51	1·51 = 12C	} = lignine
1·90	1·88 = 10HO	

Gun-cotton, from the form in which it is produced, is not one of those substances we can expect to obtain in absolute purity. Every previous improvement in its preparation had diminished this excess of unaltered cotton, and I had no reason to suppose the last portion perfect, considering the difficulty with which some of the previous stages of improvement had been attained.

The specimen I have thus examined consists, therefore, of—

96·59 gun-cotton (12C, 7H, 7O, 3NO<sub>5</sub>).  
3·41 lignine (12C, 10H, 10O).

100·00

And pure gun-cotton consists of—

24·24 = 12C.	24·24 = 12C.
21·21 = 7HO.	2·36 = 7H.
54·55 = 3NO <sub>5</sub> .	14·14 = 3N.
100·00	59·26 = 22O.
	100·00

It is lignine in which three atoms of water are replaced by three atoms of nitric acid.

LXVII. *On the Conversion of Relief by Inverted Vision.* By  
 Sir DAVID BREWSTER, K.H., D.C.L., F.R.S., and V.P.R.S.  
 Edin.\*

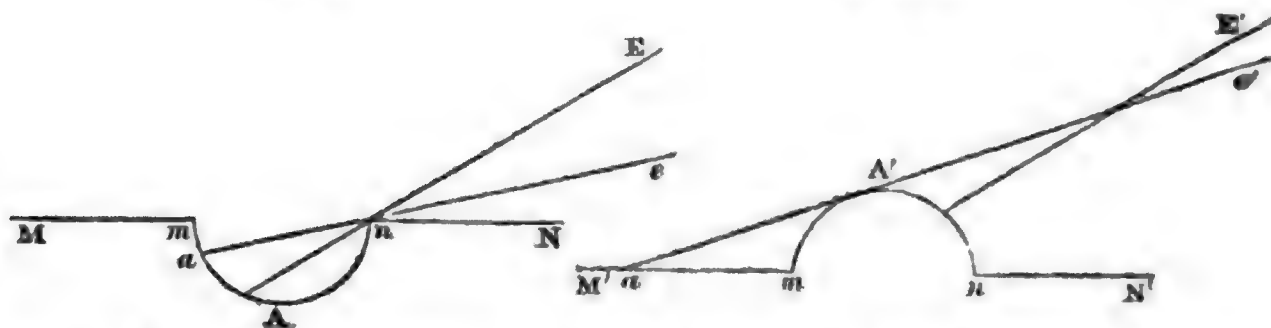
**U**NDER the name *Conversion of Relief*, an expression first used by Mr. Wheatstone, I include all those optical illusions which take place in the vision of cameos and intaglios, of elevations and depressions, whether they are produced with opaque or transparent bodies,—no surfaces with or without shadows,—in reflected or transmitted light,—while using one or both eyes,—or by erect or inverted vision. In these various forms of the phænomenon, the illusion is modified by certain secondary causes, which were regarded both by Mr. Wheatstone† and myself‡ as primary causes; so that we were led away, each in a different direction, from the right path of inquiry.

The phænomenon occurs in its most general and simple form, when it is produced by viewing a shadowless depression, or elevation, made in an extended surface, through an inverting microscope, or the inverting eye-piece of a telescope, and at an angle intermediate between  $0^\circ$  and  $90^\circ$ . In so far as I know, the phænomenon has never been thus limited, and, consequently, no explanation of it has ever been given. That which I shall now submit to the Society is capable of the most rigorous demonstration; and when it is once in our possession, we can have no difficulty in recognising the secondary causes which increase or diminish the influence of the primary one, and which, in its absence, are sometimes the immediate cause of the illusion.

Let A, fig. 1, be a deep spherical concavity, and A', fig. 2,

Fig. 1.

Fig. 2.



a high spherical convexity in an extended horizontal table M N, M' N', and let them be shadowless, or illuminated by a

\* Read at the Royal Society of Edinburgh, May 6, 1844. See their Transactions, vol. xv. p. 657.

† Philosophical Transactions, 1838, pp. 383, 384.

‡ Edinburgh Transactions, vol. xv. p. 365; Edinburgh Journal of Science, vol. iv. p. 97; and Letters on Natural Magic, p. 98.

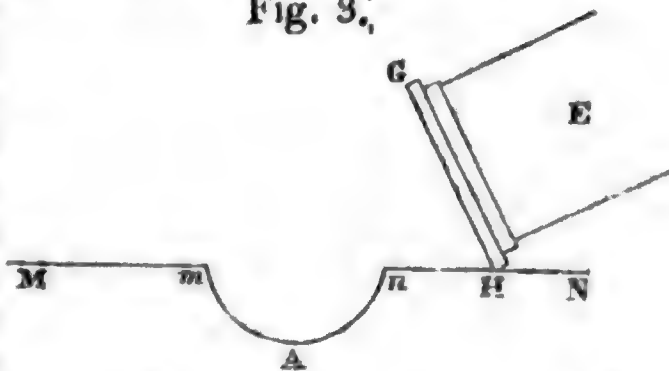
*quaquaversus* light, like that of the sky. If the observer, placed at a moderate distance, view these objects in the directions  $EA$ ,  $E'A'$ , either with one or with both eyes, his accurate appreciation of the distances  $EA$ ,  $E'A'$ , will prove to him that  $A$  is a *concavity*, and  $A'$  a *convexity*; but if  $EA$ ,  $E'A'$  approach to equality, either from the distance of the observer, or from the shallowness of  $A$ , or the slight elevation of  $A'$ , he will cease to recognise any difference in the distances  $EA$ ,  $E'A'$ , and will be unable to tell which is the convexity, and which the concavity. So great, indeed, is this uncertainty, that from causes which he cannot discover, they will sometimes appear convex and sometimes concave. In this indetermination of the judgement, a touch of  $A$ ,  $A'$  by the finger, or the introduction of a shadow, will remove or confirm the illusion, whatever it may be. The same result will be obtained if we view  $A$  and  $A'$  vertically, with an erect or inverting eye-piece. In all these cases, we suppose that the circular, or rather the elliptical, base of the convexity or concavity is distinctly seen.

Let us now look at  $A$ ,  $A'$ , at obliquities varying from  $0^\circ$  to  $90^\circ$ . In fig. 1 the *concavity*  $A$  will have an elliptical section at all obliquities, till, at  $90^\circ$ , it appears a straight line; but in the *convexity* the effect is very different. In passing from  $0^\circ$  to the position  $E'$ , fig. 2, the circular section of  $A'$  will appear an ellipse; but in passing from  $E'$  to  $90^\circ$ , the appearance of  $A'$  will lose all resemblance to  $A$ . When the eye is at  $e'$ , for example, the summit  $A'$  of the convexity will cover the point  $a$  of the table, and  $am$  will be invisible; and near  $90^\circ$ , the convexity  $A$  will eclipse the whole surface of the table  $mM$ , however extended it may be, and will rise above it.

Let us now suppose that the eye at  $E$ , fig. 3, views the concavity  $A$  through the inverting eye-piece  $E'GH$ , the horizontal table  $MN$  must obviously be inverted as well as the hollow  $A$ ; but the *apparent* change, produced by inversion, is very different from the real change. The surface

Fig. 3.

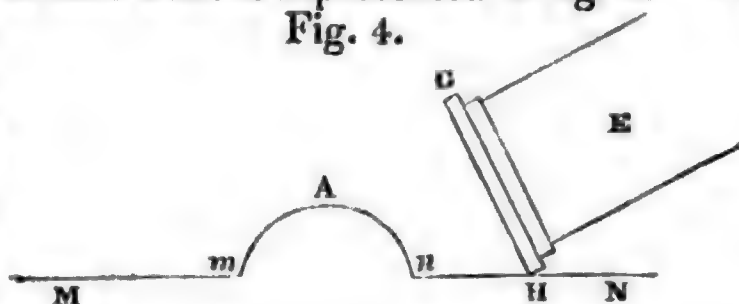
$MN$ , out of which  $A$  is excavated, and upon which the observer leans, and rests the lower end  $H$  of his inverting eye-piece, appears to remain where it was, and still to look upwards, in place of appearing inverted, and looking downwards. When he strikes the table with the end  $H$  of the eye-piece through which he looks, he believes that it is the lower end of



the field of view that strikes the table, and rests upon it. With these convictions, he sees what is represented in fig. 4. The

Fig. 4.

concavity  $m A n$ , fig. 3, appears inverted; and as the visible part of the concavity  $Am$ , fig. 3, is nearest the eye in fig. 4, and the invisible part  $An$ , fig.

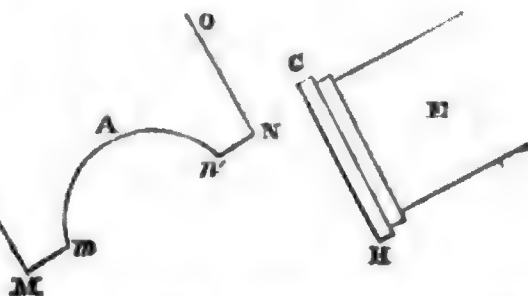


3, furthest from the eye in fig. 4,  $m A n$  must appear a concavity in fig. 4, solely because it seems to rise out of the surface  $M N$ , which looks upward, as if it had not been inverted by the eye-piece.

Now, in this experiment, the conversion of the concavity into a convexity depends on two separate illusions, one of which springs from the other.

Fig. 5.

The *first* illusion is the belief that the surface  $M N$  is looking upwards, whereas it is really inverted, as shown in fig. 5; and the *second* illusion, which arises from the first, is, that the point  $n$  appears *furthest* from the eye, whereas it is



*nearest* to it, as shown in fig. 5. All these observations are equally applicable *mutatis mutandis* to the vision of convexities; and hence it follows, that the conversion of relief, occasioned by the use of an inverting eye-piece, is not produced directly by the inversion, but by an illusion, in virtue of which we conceive the remotest side of the convexity or concavity to be nearest our eye when it is not.

In order to demonstrate the correctness of this explanation, let the concavity  $m A n$  be made in a *narrow stripe* of wood, as in fig. 5, and let it be viewed, as formerly, through the inverting eye-piece. It will now appear, as in fig. 5, really inverted, and free from both the illusions which formerly took place. The narrow surface  $M N$  being now wholly included in the field of view, and the thickness  $N O$  of the stripe of wood distinctly seen, the inversion of the surface  $M N$ , which now looks downward, will be at once recognised. The edge  $n$  of the concavity will appear *nearest* the eye\*, as it really is, and *the concavity, though inverted, will still appear a concavity*. The very same reasoning is applicable to a convexity on a narrow stripe of wood.

\* The inversion of an object never makes the *nearer* part of an object *more remote*, nor the *remote* part *nearer*.



When, as in fig. 4, the concavity is seen as a convexity, let it be viewed more and more obliquely. The *elliptical margin of the convexity will always be visible*, which is impossible in a real convexity; and the elevated apex will gradually sink till the elliptical margin becomes a straight line, and *the imaginary convexity completely leveled*. The struggle between truth and error is here so singular, that while one part of the figure *m A n* has become concave, the other part retains its convexity!

In like manner, when a convexity is seen as a concavity, the concavity loses its true shape, as it is viewed more and more obliquely, till its remote elliptical margin is encroached upon by the apex of the convexity; and, towards an inclination of  $90^\circ$ , the concavity disappears altogether, under circumstances analogous to those already described.

If, in place of using an inverting eye-piece, we invert the concavity *m A n*, by looking at its image in the focus of a convex lens, it will sometimes appear a convexity, and sometimes not. In this form of the experiment the image of the concavity, and consequently its apparent depth, is greatly diminished. Hence any trivial cause, such as a preconception of the mind, or an approximation to a shadow, or a touch of the hollow by the point of the finger, will either produce a conversion, or prevent it.

In the preceding experiments we have supposed the convexity to be high and the concavity deep and circular, and we have supposed them also to be shadowless, or illuminated by a *quaquaversus* light, such as that of the sky in the open fields. This was done to get rid of all secondary causes, which interfere with and modify the normal cause when the concavities and convexities are shallow, and have distinct shadows, or when the concavity has the shape of an animal, or any body which we are accustomed to see convex.

Let us now suppose that a strong shadow is thrown upon the concavity. In this case the normal experiment, already explained and shown in fig. 5, is much more perfect and satisfactory. The illusion is complete, and invariable when the concavity is in an extended surface; and it as invariably disappears when it is in a narrow stripe.

In the secondary forms of the experiment, the inversion of the shadow becomes the principal cause of the illusion; but, in order that the result may be invariable, or nearly so, the concavities must be shallow, and the convexities a little raised. At great obliquities, however, this cause of the conversion of form ceases to produce the illusion, and in varying the inclination from  $0^\circ$  to  $90^\circ$ , the cessation takes place sooner with

deep than with shallow cavities. The reason of this is, that the shadow of a concavity is very different at great obliquities from the shadow of a similar convexity. The shadow never can emerge out of a cavity so as to darken the surface in which the cavity is made; whereas the shadow of a convexity soon extends beyond the outline of its base, and finally throws a long stripe of darkness over the surface on which it rests. Hence it is impossible to mistake a convexity for a concavity, whenever its shadow extends beyond its base.

When the concavity is a horse or a dog upon a seal, it will often rise into a convexity when seen through a single lens which does not invert it; but the illusion disappears at great obliquities. In this case the illusion is favoured, or produced, by two causes: the first is, that the convex form of the horse or dog is the one which the mind is most disposed to seize; and the second is, that we use only one eye, with which we cannot measure depths as well as with two. The illusion, however, still takes place when we employ a lens *three* or more inches wide, so as to admit the use of both eyes, but it is less certain, as the binocular vision enables us to keep in check, to a certain degree, the other causes of illusion.

The influence of these secondary causes is strikingly displayed in the following experiment. In the armorial bearings upon a seal, the shield is often more deeply cut than the surrounding parts. With binocular vision the shallow parts rise into a convexity sooner than the shield, or continue so while the shield remains concave; but if we shut one eye, the shield then becomes convex like the rest. In these experiments with a single lens, a slight variation in the position of the seal, or a slight change in the direction or intensity of the illumination, or particular reflexions from the interior of the stone, will favour or oppose the illusion. In viewing the shield, or the deepest portion, with a single lens, a slight rotation of the seal round the wrist, backwards and forwards, will remove the illusion, in consequence of the eye perceiving that the change in the perspective is different from what it should be.

In a paper in the *Edinburgh Journal of Science*, already referred to, I have described several other examples of the conversion of form, in which inverted vision is not employed. As seen by the naked eye, hollows in *mother-of-pearl*, and other semi-transparent bodies, rise into relief; and the same thing happens on surfaces of agate and woods of various kinds when transparent circular portions are illuminated by refraction, at those parts of their circumference where they would have been illuminated had they been convexities\*. But the

\* In examining, under the microscope, the shallow fluid cavities *within*

most interesting cases of conversion of form are those in which *the mind alone* operates, and receives no aid either from inversion, shadow, or monocular vision. "If we take, as I have elsewhere remarked, one of the Intaglio moulds, used in making the bas-reliefs of that able artist Mr. Henning, and direct the eyes to it steadily, without noticing surrounding objects, we may coax ourselves into the belief that the Intaglio is actually a bas-relief. It is difficult at first to produce the deception, but a little practice never fails to accomplish it. We have succeeded in carrying this deception so far as to be able, by the eye alone, to raise a complete hollow mask of the human face into a projecting head. In order to do this we must exclude the vision of other objects; and also the margin or thickness of the cast. This experiment cannot fail to produce a very great degree of surprise in those who succeed in it; and it will, no doubt, be regarded by the sculptor (who can use it) as a great auxiliary in his art\*."

From these observations it will be seen that the conversion of form, excepting in the normal case, depends upon various causes which are effective only under particular conditions; such as the depth of the hollow or the elevation of the relief, the distance of the object, the sharpness of vision, the use of one or both eyes, the inversion of the shadow, the nature of the object, and the means used by the mind itself to produce the illusion. In the normal case, however, where the cavity or convexity is shadowless, and upon an extended surface, and where inverted vision is used, the conversion of form depends solely on the illusion, which it is impossible to resist, that the side of the cavity or elevation next the eye is actually furthest from it—an illusion not produced by inversion, but by a false judgement respecting the position of the surface on which the form is placed.

St. Leonard's College, St. Andrews,  
May 4, 1844.

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LXVIII. *Experiments proving the common nature of Magnetism, Cohesion, Adhesion and Viscosity.* By Sir GRAVES C. HAUGHTON, K.H., M.A., F.R.S., Foreign Associate of the Institute of France, &c.†

PART I.

VARIOUS methods of experimenting have been employed of late years to extend the knowledge of magnetic phæthe substance of a film of sulphate of lime, described in the *Edinburgh Transactions*, vol. x. p. 35, they frequently appeared as *elevations* on the surface of the plate next the eye.

\* *Edinburgh Journal of Science*, No. VIII. p. 109, Jan. 1826.

† Communicated by the Author.

nomena, and to which we are indebted for many very valuable discoveries. That which I am now about to explain differs from all that have preceded it, and possesses at least the advantage of simplicity united to singular delicacy\*.

It must have been clear to every one, that the phænomena observed by M. Arago in his well-known experiments made in the year 1824, when he caused a needle to oscillate over the surfaces of various substances, were due to the proximity of the needle to the body observed, as much as to the nature of the body itself; and the discovery of this distinguished natural philosopher was confirmed by Dr. Seebeck of Berlin, on making similar observations. Those I have now to detail are entirely due to the same cause, carried to the extent of *actual* and *forced* contact.

I now proceed to explain the mode in which the experiments have been performed.

#### *Mode of operating.*

If the substance to be examined is flat, as a piece of glass, for instance, it should be set up on a *very steady* table, in a vertical position, and a magnetic needle, of which a description will be presently given, is to be placed on the point of a fine sewing-needle, about half an inch in height, which may be run through a slice of cork to answer as a support; but that which I myself employ has a flat brass foot about the size of a shilling, with the fine end of a sewing-needle fixed in the centre, and the brass foot is truncated close to the supporting needle, to enable it and the object to be brought close together for the purpose of observation. If a cork support is used, the sewing-needle should for the same reason be run through it near the edge.

When a minute object, such as a precious stone or any small fragment of metal, is to be examined, it should be placed on a piece of cork or any other suitable body, in such a position to the natural direction of the needle, that on the latter

\* The general principle upon which these observations have been made was discovered so far back as the month of March 1841; it was not however applied to ascertaining whether the various classes of substances were magnetic or not till the spring of 1845, when nearly 140 substances were found to be magnetic, and a few measurements of the intensities were obtained, but it was at the commencement of 1846 that the greatest part of them were made. The remainder have been only recently completed; though these remarks were in such a state of forwardness as to lead to a hope of their being published in the summer of 1845. Want of health has been the cause of these various delays.



being urged to it by a bar-magnet, its point will exactly touch some portion that is smooth and vertical. These conditions being strictly attended to, and the object under examination being placed about a quarter of an inch or so from the point of the magnetic needle, the pole of the bar-magnet that is of a contrary character to the point of the needle intended to touch the object, is to be approached with extreme gentleness and very slowly, so as to cause the least tremulous motion possible in the needle. When the bar-magnet is brought so as to cause the needle to press with a slight force against the substance examined, the former is to be kept in the same steady horizontal position, for at least ten or fifteen seconds after it has been observed that all tremor has ceased in the needle; upon which the bar-magnet may be withdrawn as slowly and as carefully as it was brought near. And this is the place to observe, that one great advantage of having the supporting pivot so short, and consequently the magnetic needle so low down, is that it enables the observer to allow the magnet to lie upon the table, and to advance and draw it back at pleasure, as well as to have its axis in the same horizontal plane as that in which the needle revolves; for should it be higher or lower than the latter, it would cause an inclination, and a consequent displacement of the needle, from gravitation the moment the bar was withdrawn, that might interfere with the success of the experiment. If all the conditions just described have been attended to, the needle will be found to have attached itself firmly to the glass, copper, or other body operated upon, and will in many cases so remain any indefinite time, if not disturbed. But it should be well-remarked that *time* must generally be given to the magnetic needle to settle, and thereby coerce the substance to which it is to attach itself. *This is an essential condition in most cases.* The slightest movement in the room, even the motion of the air from the impulsion given by the breath of the observer, are to be carefully avoided in the nicer experiments. Any vibration in the house also will, in most cases, from the extreme delicacy of the operation, cause a failure. Some time, too, will in all probability elapse before the experimenter will have acquired the dexterity necessary to the success of the difficult cases, and he must be careful that the point of the magnetic needle does not catch itself against the object under examination when it is rough; and there is also great danger of its becoming jammed when it approaches an angle of  $90^{\circ}$  with the magnetic meridian. To prevent such a consequence, the object should be so placed, when the amplitude is large, that only the *side* of the point of the magnetic needle may rest

against it. Either pole of the needle may be used, and the bar-magnet may be made to influence it either by attraction or repulsion, as is most convenient for the observation. The operator will likewise find it an advantage to use a low seat, as he will by that means be able to have his eye in the same level as the object examined as often as it may be necessary, particularly when minute objects are observed.

All that has just been said merely regards the mode of avoiding a failure; but if it is desired to measure the relative magnetic intensities of the substances under examination, then a graduated disc is to be employed, which may be placed under the supporting needle, its N. and S. being in the line of the magnetic meridian; but a very convenient substitute for these two will be found in detaching the bottom of one of the compass-boxes, of which I shall immediately speak, covered with its engraving of the rhombs and degrees, and cutting away so much of it as might interfere with the approximation of the different bodies. The nearer the needle can be brought to  $90^\circ$  by the attraction or repulsion of the bar-magnet, the greater is to be considered the magnetic intensity of the object, which is to be shifted after each measurement so as to increase the angle of amplitude till it is found that the needle will no longer attach itself, which consequently gives the limit of its magnetic intensity. This being an exhausting and tentative process, renders such measurements extremely tedious in many cases, and yet it cannot be remedied; for, owing to the time lost in waiting for the oscillations of the needle to cease, as well as for it to connect itself with the substance, and in the various adjustments of the latter, much time is expended; added to which, the experiment must be often repeated in difficult cases, to be quite sure that a failure has really arisen, from a want of attractive affinity between the needle and substance. Nearly an hour has been consumed in an experiment where much attention and delicacy of manipulation were required.

Great care of course must be taken not to confound effects purely electric with those that are magnetic. To prevent such a possibility of error, it is only necessary to breathe upon a suspected substance, which will effectually remove all free electricity. Thus Iceland spar, which cannot be touched with the fingers without becoming electric and remaining so for days, may in the space of a few seconds be rendered fit for examination, as the humidity of the breath quickly evaporates and carries off the superabundant electricity.

It is well-worthy of remark, as showing how much *time* is necessary, not merely for the exhibition of magnetic attraction

by some substances, but also for the loss of this quality, that in many cases where the needle was with difficulty made to attach itself, on contact being broken a fresh connexion was produced with the greatest ease, if only a short delay had taken place, and no change whatever had been made in the position of the object. Time, too, was still more necessary to the success of the experiments when the angles were to be measured; for it not unfrequently happened that, when I had supposed the greatest amplitude had been attained, on maintaining the forced connexion for a few minutes with an enlarged angle, a firm connexion took place; thus throwing a doubt upon the measures I had previously noted as the highest attainable in other substances. Such measures, therefore, even in any case, can only be considered as approximative, as greater deviations may possibly be yet obtained by myself or other observers. Thus in the case of a spark of diamond, I was able to increase the angular distance from  $42^{\circ}$  to  $65^{\circ}$ . So likewise in the case of quartz, it rose gradually from  $45^{\circ}$  to  $90^{\circ}$ . Now when it is considered that the attachment of the needle takes place generally with the greatest readiness when the angle of deviation is small, say  $10^{\circ}$  or  $15^{\circ}$ , owing to its limited deviation from the meridian in such cases, it can leave but little doubt that this peculiarity arises from the slow effect of magnetic propagation in the substance examined, in which, to use language that has become familiar in describing magnetic phenomena, *saturation* takes place but slowly.

The foregoing facts entirely accord with the remark made by Sir John Herschel and Mr. Babbage, when in 1825 they varied the experiments of M. Arago. A remarkable confirmation of this peculiarity was exhibited by chromium, which was entered, after many trials with a magnetic needle, as a failure, yet on repeating the experiment, and allowing a long time for the needle to operate, the measurements rose successively, according to the notes taken on the occasion, as follows:  $20^{\circ}$ ,  $25^{\circ}$ ,  $30^{\circ}$ ,  $40^{\circ}$ ,  $45^{\circ}$ ,  $49^{\circ}$ ,  $55^{\circ}$ ,  $70^{\circ}$ ,  $90^{\circ}$ . The notes on the subject having been overlooked, a fresh attempt at measurement, after the lapse of more than a year, was recently made, when it was again entered as a failure, as there was not the least tendency to a connexion between the needle and the chromium after the ordinary time given to other difficult substances.

On reflecting on the necessity of time being given to develop the magnetism of some substances, and remembering that the needle, which was the active agent in its production, did not weigh four grains, and that the parts of the needle and the substance in contact were merely two minute points, it struck

me that the magnetism might be much increased by keeping it in close contact for a short time with a more powerful magnet. The result fully justified this anticipation, and the attractive affinity between the needle and the substance was immediately augmented; but generally in those cases where no attraction had been already discovered, none made its appearance by the additional magnetic influence to which it was subjected. Want of health and time prevented me from establishing more than the general character of these last facts, but I shall have to speak more fully of the particular cases tried as I proceed.

Among the few substances placed in contact with the bar-magnet for the purpose of increasing their magnetic energy, the following seem to be totally uninfluenced by its presence, viz. copper, lead, marble, selenite, alum and pure lac.

The following experiment will show how strong the connexion often is that takes place between the needle and non-ferruginous substances. A magnet having been brought vertically over the needle contained in a small box-compass, which was screened by a piece of crown-glass, the needle attached itself instantly, or, to speak more exactly, *per saltum*, so firmly to the glass, that the box might be pushed backwards and forwards from one end of the chimney-piece on which it lay to the other, without contact being broken; and in this state it continued for six days, though the door of the room was often shut with considerable force; and the connexion was at last only broken by the box having inadvertently received a shock from the hand. The contact was occasionally renewed by the same means, sometimes for two or three days, and always continued till broken at pleasure. This experiment will not succeed if the pivot of suspension is either too high or too low; and the needle must be of the pointed kind.

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It is proper to describe here a very useful electrometer which I employed to ascertain whether substances exhibit any *electrical effects*. It is of extreme simplicity, and is not merely *much more* sensitive than that employed by Häüy, but even more so than the most delicate gold-leaf electrometer. It consists of a magnetic needle five-eighths of an inch in length, with an agate cap. Upon each end of the needle a piece of oaten straw, one inch in length and about one-thirtieth of an inch in diameter, is fixed. When this part of the system is placed, as in the former case, on the point of a sewing-needle with a brass stand, the instrument is complete. It has an additional advantage over that of Häüy, that, owing to the



slight magnetism of the needle, the points of which are inserted in the straws, it preserves a definite position, without the slight magnetism of the needle apparently interfering with its sensibility. It has likewise the advantage of being easily set up or dismounted to protect it from injury, and is capable of being contained in a very small box. I ought to remark likewise that it is so susceptible, that if unprotected by a screen it is kept in a sort of perpetual movement, by the otherwise inappreciable currents of air in the room.

*Proper forms of the needles and magnets.*

After the trial of needles of various forms and lengths, I have found none that answer so well as those which are employed in the compasses of commerce, set in mahogany boxes two inches square, and to which I have already alluded. The needles in these boxes are an inch and a half long, very slender and *pointed*, weighing about three grains and a half. Their caps are of brass, which in this case is an advantage; for needles with those of agate are much too tremulous from their great sensibility, and the experiment will be almost sure to fail if the point of the needle slips in the least from the spot where it first settles, the circumference of its action being as limited as the part of the point of the needle actually in contact. Long needles, therefore, owing to their great oscillations, are particularly unfit for these experiments; while, on the contrary, I have succeeded extremely well with a needle not more than five-eighths of an inch long.

The preceding directions simply concern the kind of magnetic needle that is to be employed; but it is, as I have said, by the aid of a bar-magnet that the needle is made to attach itself to non-ferruginous bodies; for by no other means could it be caused to approximate itself by a slow, steady, and gentle movement, and be left without disturbance when the motive agent is withdrawn, a condition upon which the success of the experiment entirely depends in the cases of low intensity; but for those of higher attraction, I shall presently show a simpler mode of attaining the same end without a magnet. The bar-magnet which I use is very convenient; it is six inches and a half long, three-quarters of an inch wide, and rather better than a quarter of an inch thick. If the magnet is long, it is managed with difficulty; and if small, it requires to be brought very near the object, and there is consequently danger of disturbing the position of the needle when the magnet is withdrawn, which had better likewise be wrapt in soft paper, to enable it to be laid down on the table without causing a jar

or shock. I now proceed to an important step in these discoveries.

*On the relative dynamic values of the degrees of the compass, and on the cause of the needle resting in the magnetic meridian.*

Shortly after I had begun to measure the angles of deviation, I saw that it was a matter of the first necessity to ascertain whether the degrees of the compass after  $90^\circ$  increased in strength, or went on diminishing in some given ratio, as it appeared to me that the connexions were made with increased facility in proportion as the N. pole of the needle approached nearer to  $180^\circ$ . For the purpose of settling this question, I made a number of most careful measurements, after drawing proper lines upon a sheet of paper, which all crossed one another in a common centre, by ascertaining the distances at different azimuths at which a bar-magnet, placed with its axis directed to the centre of a magnetic compass-needle (the pivot of suspension of which was exactly over the common centre of the lines of which I have spoken), caused the needle to assume a position in which its axis was in the same line with that of the magnet, I found that these points of distance form a peculiar curve, which was nearest to the axis of the magnet at the east and west azimuths, and receded from it as it advanced to the north and south; and was twice the distance from it at the north that it was at the south azimuth. Such is the sum total of the results which I communicated to the Royal Society in June 1846, and of which a short account has been given in the Abstracts of the Papers read before the Society, and republished in this Journal for last November\*. It since however occurred to me, that the longest distance from the east and west azimuths would be better ascertained by operating by the means of repulsion than by attraction, as I had done; for it is evident that repulsion will sooner discover a minute deviation of the needle when in its natural position at N. than can be effected by attraction, because if the axis of the bar-magnet deviates in the slightest degree from the line, a false value will be obtained; and there always therefore remains a doubt in proceeding by attraction, respecting the point where the reciprocal action of the needle and the magnet ceases, in consequence of the identity of their axis. Now in proceeding by repulsion, the reverse takes place; and the minutest difference, except so far as the friction of the needle on its pivot is concerned, is immediately detected. I was surprised therefore to find that where I had only been able to discover a devia-

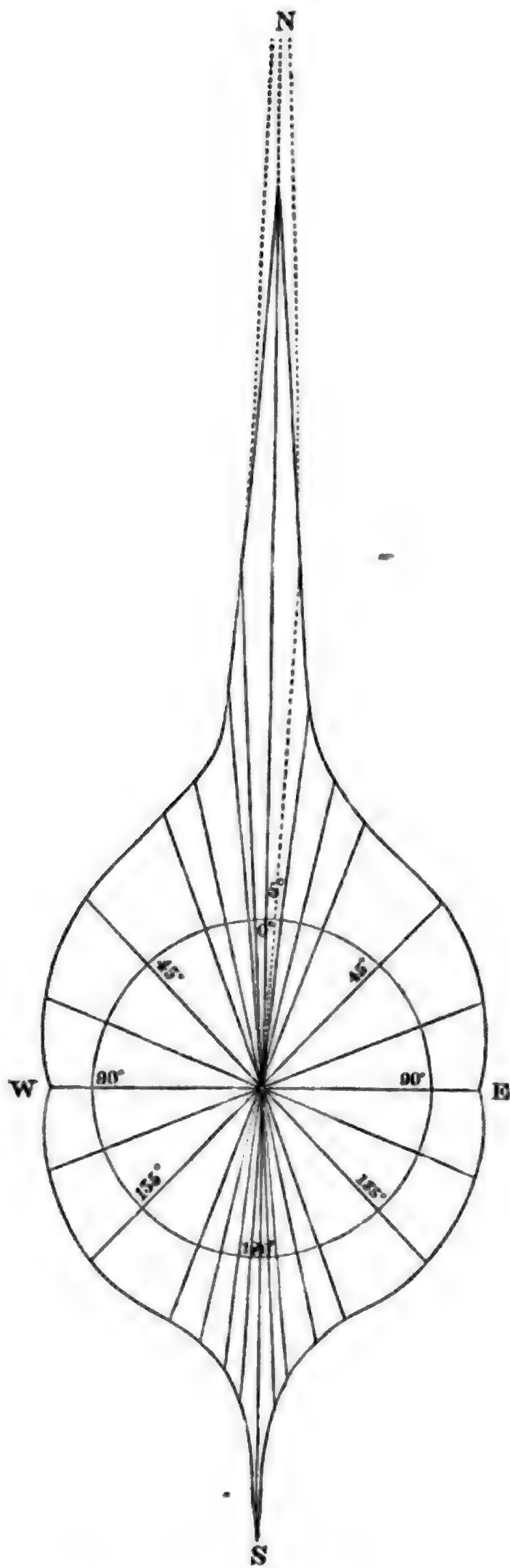
\* Vol. xxix. p. 405.

tion at eight inches distance, owing to the minute quantities concerned, I could obtain a deviation at sixty inches, after which all effect ceased. As the distance was exactly four inches from the centre when the north end of the needle was drawn to  $180^\circ$  by the bar-magnet, and as there must be some definite ratio between the two distances thus obtained, I assume that the true length of the greater azimuth is sixty-four inches, and that the four inches not discovered in the measurement resulted from the friction of the needle on its pivot, though it was an extremely delicate instrument that had been made by M. Pixii. Instead therefore of the greater distance being merely the duplicate length of the shorter one, it is related to it as its cube.

The reason of this disproportion of the needle when in its natural position, from that which is discovered when it is forced round to the opposite pole, may, I think, be found, when it is remembered that, as the earth is either *per se* or by induction a magnet, the latitude in which the experiment has been made being at such a distance from the magnetic equator, there can be no longer an equality in the effects. It would be therefore a question, I think, not devoid of interest, to ascertain whether such a disproportion exists at the magnetic equator.

I will now describe the means by which a long line was obtained, upon the exactness of which the accuracy of the observation necessarily depended. A white thread of fine cotton, weighted at each end with lead, was laid over a long and steady table, and the weights being allowed to hang down, a perfectly right line was instantly obtained, which had only to be adjusted so as to agree with the magnetic meridian.

The length of the azimuths obtained in the lines N., E. and S. being respectively 64, 2, 4, it follows that by the law of the inverse square of the distance they are related to one another in the proportion of 1·1024 and 256, and that consequently every degree rises gradually from N. or  $0^\circ$ , till at E. or  $90^\circ$  its strength is 1024, after which there is a rapid decrease up to  $180^\circ$ ; and it may therefore be practically assumed, to save circumlocution, that at  $175^\circ$  its proportional strength is 256°, or only one-fourth the strength it has at  $90^\circ$ , that being the true proportion as it reaches S. or  $180^\circ$ , where the needle is necessarily in a state of equilibrium by its balanced attraction to the west; but all this will be much more evident by a reference to the curve itself, which is preserved in the form that was laid before the Royal Society, as it would be inconvenient to represent on paper the due proportion from the centre to N. In fact, for all practical purposes, the new correction is



The true proportions of the curve, from the centre to E. or W., is 1, to S. 2, and to N. 16; but as the distance from the centre to N. could not be represented conveniently on paper, the dotted lines on each side of the line N. must be supposed to be elongated to that extent.



of no consequence, as anything lower than the azimuth line that passes through  $5^\circ$ , I have found to be as low as can be ever required for any useful measurement; and it will be seen that by the dotted line I have drawn passing through it, and the dotted lines that are intended to represent the correction of the curve, that azimuth remains unaffected. Measured by a rule, the proportion of the azimuth line passing through  $90^\circ$  is as 1 to 2 with that passing through  $5^\circ$ . Now by the inverse ratio of the distance, it has only one-fourth of the strength at  $5^\circ$  that it has at  $90^\circ$ ; and practically, therefore, is the same in strength at  $5^\circ$  as it is at  $175^\circ$ . Hence  $90^\circ$  may be considered as a middle point, from which the degrees diminish by equal decrements to  $5^\circ$  and  $175^\circ$ ; and it will be seen by a reference to the curve, that the azimuth lines cutting  $45^\circ$  and  $135^\circ$  are essentially the same in length. The ascertainment of the true strength of the degrees from  $90^\circ$  to  $180^\circ$  was a necessary desideratum, which has led to the unexpected discovery that the position of the magnetic needle north and south is the effect of equilibrium arising from equal repulsions from east and west; and that so far from there being any attraction at N. and S., they are just the two points where the needle is in the greatest state of freedom; and that in the case of a needle of low magnetic power, the mere resistance caused by friction on its pivot, will be sufficient to prevent its taking up its true position in the line of the magnetic meridian. These facts prove that the movement of the needle from  $90^\circ$  to zero depends upon repulsion, commencing at  $90^\circ$ , and diminishing gradually till it stands in equilibrium at zero; and that the return of the needle to  $90^\circ$ , when the north end is brought to  $180^\circ$ , is the result of an attraction that increases up to  $90^\circ$ , when the repulsion commences.

The mathematical expression of these results is that—

*The value of every degree of the compass is inversely as the square of the length of the ordinate or co-ordinate passing through it, the abscissa being considered as zero.*

The general principle of the values of the various degrees of the compass obtained by the preceding system of azimuths, was further verified by the following method:—

A bar-magnet  $12\frac{7}{8}$  inches long, was properly suspended by silk, free from torsion, and the N. pole being brought round to the south, was allowed to return back by the east. The following is the mean of eight observations, made with great care, and which only slightly differed from one another. The bar required  $11\frac{1}{2}$  seconds to complete its return from  $180^\circ$  to  $135^\circ$ . When made to start again from  $135^\circ$  to  $90^\circ$  it occupied six seconds; and when allowed to depart again from  $90^\circ$

to  $45^\circ$ , the time occupied was  $5\frac{1}{2}$  seconds; while its return from  $45^\circ$  to  $0^\circ$ , after being again left to itself, took  $6\frac{7}{8}$  seconds.

It might be supposed, *à priori*, that the north end of the bar-magnet when brought to the south and left to itself would be repelled with violence; but so far from this, as *seems* to be the case when small needles are observed, its movement is extremely slow, and there is a loss of a second or two before it acquires the gradually augmenting momentum that is to carry it back: a momentum indeed which increases so rapidly, that though the bar consumed  $11\frac{1}{2}$  seconds to carry it back from  $180^\circ$  to  $135^\circ$ , it only required  $15\frac{1}{8}$  seconds to carry it from  $165^\circ$  to  $0^\circ$ , and of this time  $13\frac{1}{8}$  seconds were occupied in reaching  $45^\circ$ , after which the remaining  $45^\circ$  were completed in 2 seconds; yet the bar required  $6\frac{7}{8}$  seconds to return from  $45^\circ$  to  $0^\circ$ , when it had not the benefit acquired by the preceding momentum. As a further proof how much the slowness of the movement of the bar increases near  $0^\circ$ , when not assisted by previous momentum,  $6\frac{3}{8}$  seconds was the space of time requisite to carry the bar from  $5^\circ$  to  $0^\circ$ , yet when it had the advantage of the momentum acquired between  $10^\circ$  and  $5^\circ$ , it completed the whole in  $\frac{1}{4}$  second less time, that is the space from  $10^\circ$  to  $0^\circ$  in  $6\frac{1}{4}$  seconds, while it has just been seen that it required only 2 seconds to carry it from  $45^\circ$  to  $0^\circ$ , when it had the advantage of the previous momentum acquired by setting off from  $165^\circ$ .

There was a peculiarity in the return movement of the magnetic bar which is worthy of attention. When the north end moved from  $180^\circ$  to  $135^\circ$ , it required, as has just been seen,  $11\frac{1}{2}$  seconds to complete the amplitude, while it only took  $6\frac{7}{8}$  seconds to traverse over the parallel amplitude from  $45^\circ$  to  $0^\circ$ . The reason of this difference is, that in the first case its momentum is in an increasing ratio, and the force that is to move it has gradually to augment, while from  $45^\circ$  it sets off with a strong momentum which it retains till it reaches  $0^\circ$ . What the nature is of this force which goes on increasing on the return of the bar up to  $90^\circ$ , and from that point continues decreasing down to  $0^\circ$ , is a question of much importance in the theory of terrestrial magnetism. It is evidently one at right angles to the natural position of the needle, and seems to be caused by a principle identical with that of the conjunctive wire of the pile, and would strongly tend to confirm Ampère's theory of electric currents from east to west, if the supposition of the efficiency of these were not opposed by considerations to which I may return on a future occasion.

While upon this point it will be interesting to see the relative times occupied by the needle when drawn back to  $175^\circ$ ,

in its return to  $0^\circ$ , measured through regular divisions of the compass. On account of the difficulty of starting the needle from  $180^\circ$  when *in equilibrio*, the measurement only began from  $175^\circ$ .

To simplify the expression of the results, and save the use of decimals, the signs *plus* (+) and *minus* (−) are used to denote time in excess and paucity, which in the mean of eight observations were too insignificant to be otherwise brought in.

Amplitudes.	Mean.	Decrement on each side of $90^\circ$ .
$15^\circ$ to $0^\circ$	$6\frac{1}{8}$	$2\frac{1}{8}$
30 to 15	4	$\frac{7}{8}$
45 to 30	$3\frac{1}{4}$	$\frac{3}{4}+$
60 to 45	$2\frac{1}{4}$	$\frac{3}{4}-$
75 to 60	$2\frac{1}{4}$	$\frac{1}{4}$
90 to 75	$2\frac{1}{4}$	0
.....	.....	.....
105 to 90	$2\frac{1}{4}$	0
120 to 105	$2\frac{1}{4}$	$\frac{1}{4}+$
135 to 120	$3\frac{1}{4}$	$\frac{3}{4}-$
150 to 135	4	$\frac{5}{4}+$
165 to 150	$6\frac{7}{8}$	$2\frac{7}{8}$
175 to 165	9	$2\frac{9}{8}$

The mode I have described of measuring the *insensible* magnetic *attractions* of non-ferruginous bodies, has the rare advantage of the most perfect delicacy and precision in all angles up to  $90^\circ$ , but from thence to the moment when *sensible* attraction commences, there is considerable vagueness, as far as relative intensities are concerned. Where perfect *repulsion* occurs it is distinctly indicated by the impossibility of forming a connexion; but it is likewise evident that in many cases a low degree of repulsion to the magnetic needle is overcome by giving time for magnetic propagation, or by keeping the substance operated upon in contact with a powerful magnet. Where a body is very light, as cork or charcoal, the failure must be attributed to their want of density, for the part of the needle which comes in contact with the bodies is but a minute point, yet the molecules which compose the cork or charcoal in that spot, must stand at such a distance from one another as to render their magnetism ineffective, and therefore inappreciable.

The *delicacy* of this mode of measurement is sufficiently attested by its having been applied with equal success to a  
*Phil. Mag. S. 3. Vol. 30. No. 203. June 1847.* 2 H

hair of the human head, and to a spark of diamond; and the accuracy attained may be judged of by the fact, that the same hair having been measured a year afterwards, was found, without any remembrance of the previous measurement, to be what it was before, namely  $80^{\circ}$ . A specimen from another head which only gave  $76^{\circ}$  was lost, and the measurement could not be verified, but another hair from the same head being tried it was found to be  $82^{\circ}$ , a difference which may be accounted for by supposing that all the hairs on the same head are not possessed of equal magnetic intensity at the same moment of time, or by a change in the state of the health of the individual. The measurements may be relied upon as never in excess, though from causes of disturbance, such as the vibration of the house, &c., they must occasionally be capable of being pushed further. No pains however have been spared to obtain the utmost degree of accuracy. Those who shall undertake to repeat these experiments will be best aware of the extreme difficulty of the subject.

*Of the marks of indication.*

Substances not followed by any numerals have been ascertained to be magnetic, but have not been measured.

A 0 implies that no attraction could be discovered.

The sign + marks that the measurement could be carried further.

The sign  $\times$  after  $90^{\circ}$  is intended to show that the magnetism is still stronger than that of the highest case of +.

The double sign of  $\times$  (that is  $\times \times$ ) is employed to show that the connexion has been effected by the delicate employment of the finger instead of the magnet; but that the magnetic intensity of the substance is still not sufficient to attract the needle, whether ferruginous or non-ferruginous of itself. This is the "simpler mode" already alluded to.

A marks such substances as attract the needle, though at short distances.

The magnetic needle was made to connect itself with all the undermentioned substances not followed by 0, which are arranged according to their classes; and the measurements of such as I had time to ascertain are inserted after each.

*1st Class.*

Gold (pure) .....	$90^{\circ}$	Lead .....	$90^{\circ}$	Cobalt .....	A
Do. (18 carats).....	90	Zinc .....	90	Plumbago .....	0
Silver (pure).....	90	Copper .....	$90+$	Iodine .....	A
Do. (coin).....	$90+$	Mercury .....	$90 \times$	Arsenic .....	15
Platinum .....	65	Antimony .....	66	Brass .....	A
Palladium .....	68	Bismuth .....	51	German silver .....	A
Cadmium .....	90	Chromium.....	90	Bell-metal .....	90
Tin .....	90	Manganese .....	A		



I have already spoken of the manner of raising the magnetic intensity of various substances, by keeping them in close contact with a bar-magnet for a short time. The following metals had their magnetic intensities slightly raised by this means:—

Platinum, from  $65^{\circ}$  to  $90^{\circ}$ ;  
Antimony, from  $66^{\circ}$  to  $90^{\circ}$ ;  
Bismuth, from  $45^{\circ}$  to  $80^{\circ}$ .

It is very remarkable that it is not of the least consequence whether the substance is kept in juxtaposition with the N. or S. end of the magnet, for the result will be just the same in either case, and the N. or S. end of the needle may be made to attach itself with equal indifference to the part of the substance that has been so excited. Whether this proceeds from a want of polarity in ferruginous magnetism, or from the molecules of the substances having been rendered more obedient to the coercive energy of the needle, it is difficult to say. As the magnetism still remains, after such influence, of the insensible kind, it is clear that they cannot penetrate to any depth, and this may be the reason why no polarity can be formally proved. The possible existence of polarity is countenanced by the fact that the magnetism is capable of being heightened, and by the analogy of the connexion that takes place between two similar poles of magnets when brought forcibly into contact, as well as by what takes place with arsenic. The magnetism which is found to exist in all matter, and that to a degree which has never been anticipated, would be of tremendous operation in nature if it were not like or identical with cohesive affinity, and consequently acting only at distances that may be considered as *insensible*. In this respect it contrasts strongly with the power of gravitation, and with that even of the magnetism of steel.

The effects that take place by keeping substances in contact with a magnet cannot be attributed to induction, as they continue for a longer or shorter space of time after separation.

The measurements of the first class of substances speak so clearly for themselves, that there would be but little occasion for any remarks upon the greatest portion of them, if it were not for the contrast in which some of them stand to the important results obtained by Dr. Faraday, when he operated with a very powerful electro-magnet. From the effects he observed he divides all metals into two classes, namely, *magnetic* and *diamagnetic*, according as they showed attraction or repulsion; the first of these states being indicated by the cylinder placing itself in the line of the axis of the magnet, and

the other, on its taking up a position at right angles to the same axis. Now it is not a little remarkable that platinum and palladium, which he selects as magnetic, exhibit, it will be seen, a lower state of attraction than cadmium, copper, gold, mercury, and silver, and that these, in common with antimony, bismuth, lead, tin, and zinc, in which he found repulsion, all exhibit attraction, according to the present form of experiment, and some in a very strong degree.

The case of antimony and bismuth is deserving of attention, for by a method somewhat analogous to the present one, the late M. Lebaillif found that they exhibited repulsion; though in his experiments the bodies were not forced into contact, and therefore their occult properties were not elicited. In these two instances, as is the case with all bodies of low attractive power, the connexion was formed with some difficulty; but as it was repeated on many occasions at different intervals of time, and even within a day of these remarks, not the slightest doubt can exist on the point. But I shall have to return to this subject again.

Another fact resulting from the two modes of experimenting employed by Dr. Faraday and myself, is that the connexion could be formed on any side or end of the substance, and that form had no influence upon the result. Indeed this was to be expected, as the transverse position which the substances assumed in his hands resulted from their shape, as every oblong body under repulsion must necessarily place itself at right angles to the resultant of forces, with a facility proportioned to its length and slenderness. Where the shape of the substance examined would permit of it, the end has of late always been selected in preference to the side, with a view to ascertaining this point; and this was particularly the case with the antimony and bismuth, which were in bar, and had been cut across.

I have introduced iodine into the class of metals and metalloids, as its semi-metallic lustre seemed to justify it, and because I found that it attracts the magnet of itself; but it shows no polarity. All that is necessary is to hold a scale of this body against the point of the magnetic needle, when, on drawing it back, the needle will remain connected with it for  $40^\circ$  or  $50^\circ$  of the compass. A piece of arsenic too, which showed very low magnetic intensity ( $15^\circ$ ), acquired a similar power, but in a higher degree, after it had been for a short time in close juxtaposition with the end of a bar-magnet; but it was remarkable that this power only appeared on the flat side of the arsenic where it had pressed against the crucible in which it was melted, the other broken parts of the sides

being only raised by the same process to  $85^{\circ}$ . I attribute this peculiarity to the greater number of points that are brought in contact with the magnet where the surface is flat. The character of the magnetism thus acquired is entirely the same as that of steel, attracting and repelling in the same manner, but comparatively in a very low degree, and consequently quite superficial. If brought quite close to that end of the needle for which it exhibits repulsion, it will, like a magnet, in consequence of the proximity, attract it. It lost its magnetic power in the course of a few weeks, but it has since been renewed with the same facility as before.

It appeared to me that the needles got out of order, for a short space of time, by their contact with the iodine and arsenic; but upon this point I cannot at the present moment speak positively. Certain it is that they became quite paralysed on two or three occasions, but were restored by having recourse to *the touch*, and subsequently by only letting them lie quiet an hour or two. I suspected the same effect to have taken place once or twice, after keeping them in contact with antimony, but at other times I could assign no cause for the change.

All the specimens of brass I have examined are very magnetic, and may have that property considerably increased by putting them in contact with a magnet. One specimen, which is a bar, causes attraction at one end, but the repulsion which existed at the other has disappeared. The former state appears to continue unchanged, at least I have seen no difference in the course of a year. Bell-metal has very low magnetic powers; so much so indeed that I despaired of making the needle connect itself with it; nor was this object effected till they had both been kept in connexion for some time, and even then there was every symptom that this was attained only by overcoming a natural repulsion, as in the case of chromium, of which I have already spoken. No change seemed to be effected by keeping it in contact with a magnet. German silver is magnetic, and if brought within about the eighth of an inch of the needle will attract it. But the specimen I had, seemed by its colour to contain an unusual proportion of copper, or otherwise it might have proved more magnetic. No increase of magnetism resulted from connection with a larger magnet. Cobalt and manganese, as is known, attract. The needle clung at once to a globule of mercury, against which it had been urged by the magnet.

All the rarer substances, including pure gold, in the preceding class, I believe to be chemically pure, as they were obtained from Messrs. Boyveau and Pelletier, and M. Ber-

hemot of this city (Paris), who considered them as such; but the tin and lead were taken from the ordinary specimens of commerce, and if the supposition which accounted for the magnetism of non-ferruginous bodies by their containing minute portions of iron, had any foundation, it would have been apparent in these two substances; yet it will be seen that the magnetism of both shows nothing very remarkable, and that the tin exhibits the same degree of attraction as the specimen of cadmium which I believe to be chemically pure, and these two will be found, nearly in all circumstances, to exhibit as remarkable a conformity magnetically as they are known to do chemically. When I show, however, the mutual attractions of non-ferruginous bodies, it will be seen that the notion of Coulomb, and other experimenters posterior to him, cannot be accepted as accounting for the results they observed.

### 2nd Class.

Selenium ..... 10° | Sulphur 28°

Selenium and sulphur both exhibit very low intensities with the magnetic needle, and even these are obtained with so much difficulty as to demonstrate that they are forced states. I have paid particular attention to these two singular substances, and have found that they resemble one another as much by their magnetic properties as by those that are chemical. I will here anticipate the branch of this inquiry which is to follow, by stating that they are remarkable in nearly agreeing in their affinities for the metals, but particularly for gold. They, however, show not the slightest attraction for one another, and therefore must be considered as mutually repulsive. I have invariably found that where there is a chemical resemblance there is likewise similarity in magnetic properties, and these two substances maintain their resemblance in the same way as tin and cadmium, which only exhibit a difference of nature with respect to arsenic; but everything regarding this last body seems anomalous, which, like sulphur, has no affinity for selenium. Contact with the magnet appears to elevate their power of attraction, at least this was the case, as will be seen hereafter, when they were tested with brass.

### 3rd Class.

Diamond .....	65	Oural emerald .....	32	Agate.	°
Do. (2nd specimen)	65	Garnet .....		Cornelian (red).....	35
Ruby .....	90+	Tourmaline .....	32	Do. (white) .....	37
Sapphire .....	90+	Amethyst .....	65	Blood-stone .....	37
Topaz .....	74	Turquoise .....	47	Quartz .....	90
Do. (pale).....	66	Opal .....	0	Flint .....	90
Jargoon.....	90+	Mexican opal.			
Emerald .....	90+	Moon-stone.			



The present class exhibits many various degrees of attraction for the magnetic needle, but the ruby, the emerald, the sapphire and the jargoon, it will be seen, stand high in rank for this property, while tourmaline and ural emerald are but low. The diamond was the only one of these precious stones that was placed in contact with the magnet, and it was found to have its energy raised by the connexion. Though the diamond does not exhibit much attraction for the magnetic needle, it will be seen hereafter that this is compensated for by its affinity for copper, platinum and glass of every kind; and it will be generally found that what is of difficult connection with the magnetic needle is remarkably the reverse with glass, and they may respectively be placed at the head of a binary division of all substances; and what makes the fact the more remarkable, is that the attractive properties of glass are heightened in a very sensible degree by its remaining in contact with the magnet. Glass has a striking affinity for cornelian, red and white, and for blood-stone, while a connexion between the magnetic needle and these stones is formed with extreme difficulty.

It might have been expected that flint would have shown much resemblance to quartz in its attraction for the magnetic needle, but the connexion was always formed with great difficulty, and on many occasions, though it appears as high as  $90^{\circ}$ , it could not be raised above  $20^{\circ}$  in some particular specimens, and many were tried. Yet the needle attached itself with great facility at  $40^{\circ}$  and  $50^{\circ}$  to quartz, and finally reached  $90^{\circ}$ , but not by any forcing process: that measure however is still its ultimate limit. Flint has a decided repugnance for the magnetic needle, but still the magnet can overcome it, and a refractory specimen was raised by this means from  $20^{\circ}$  up to  $40^{\circ}$ .

*4th Class.*

Flint glass (prism).. $30^{\circ}$	White cameo (artificial).	Felspar ..... $10^{\circ}$
Do. (tumbler) ..... $90^{\circ}+$	Brown ground of do.	Selenite..... 15
Eye glass ..... 43	Porcelain (dentist's) 74	Talc ..... 60
Crown glass ..... $90^{\circ}\times$	Blue enamel ..... 36	Sulphate of iron.. 48
Blue glass..... 90	Gypsum-stone ..... 26	Protosulphate of
Green fancy glass... 90	Alabaster ..... 25	iron ..... $90^{\circ}\times\times$
Green bottle glass.. 70	Marble (yellow) ... 10	Carbonate of iron 16
Red glass.	Marble (gray) ..... 55	Sulphate of copper.
	Slate ..... 30	Alum..... 15
Stone-ware ..... 75	Hone..... 24	Rock salt.
China.	Red chalk.	Saltpetre.
Porcelain (French).. 90	White chalk.	Sugar candy (white and
Do. (English).	Crystallized tin..... 0	brown).
Delf (English).	Iceland spar ..... 90	Lump sugar.

The prism was raised by the magnet from  $30^{\circ}$  to  $60^{\circ}$ , but the green glass, to which the magnetic needle attached itself with great difficulty, seemed to acquire repulsive rather than attractive powers by the same means. This was a specimen of fancy glass, of an apple-green colour, and cut to represent the calyx of a flower. I am ignorant as to what metal it owed its colour. It will be seen hereafter that the prism, which was of English fabric, exhibited an extraordinary affinity for all the metals, as well as for flint glass. The common green glass employed for wine bottles, and crown glass, notwithstanding the iron they contain, exhibited no striking attraction for the magnetic needle, which can be easily accounted for by remembering that the iron is in intimate combination with other bodies, and having its affinity employed, simulates neutrality.

The particular kind of porcelain used by dentists has very little attraction for the magnetic needle, and in other respects exceeds even the glasses in its affinity for non-ferruginous metals and gelatinous bodies. It and stone-ware are much alike.

The crystals of various kinds in this class, with the exception of protosulphate of iron, exhibit very low magnetic intensities for the needle, and confirm what I have said about the common glasses. Carbonate of iron, it will be seen, has but little affinity for the magnet. Iceland spar is as difficult and capricious as flint in its attraction for the needle, but is remarkable in its affinity for non-ferruginous bodies. Talc has but little attraction for the magnet and zinc, but it is otherwise with iron and gold. It was not tried with other bodies. The specimen, which was remarkably thin, came from Delhi. The other substances were not measured, but the needle connected itself with them. Crystallized tin failed with the magnetic needle, but rose to  $90^{\circ}$  after contact with the bar-magnet.

#### 5th Class.

Pearl.....	$90^{\circ}+$	Horn (cow's) .....	$85^{\circ}$	Human hair (female's, $^{\circ}$	
Nacre .....	$90 \times \times$	Do. (chamois').....	90	young, 2nd specimen)	76
Ivory (new) .....	$90^{\circ}+$	Leather.		Do. (do. 3rd specimen)	82
Do. (old) .....	58	Black seal's skin ...	62	Do. (female's, old) ...	$90^{\circ}+$
Enamel (human		Quill.		Do. (gray, male's) ...	$90^{\circ}+$
tooth).		Pig's bristle.		Do. (do. female's) ...	$90^{\circ}+$
Bone (mackerel's)	90	Human nail.		Do. (beard, gray).....	90
Whalebone .....	90	Human hair (brown,		Hare hair (gray) .....	90
Tortoise-shell ...	85	male's) .....	90	Do. (black) .....	90
Horn (buffalo's)..	90	Do. (female's, young)	80	Cat's whisker .....	$90 \times$

Amongst the animal products, pearl and mother-of-pearl are conspicuous in this class for their attraction for the magnetic needle, and for gold, copper, brass, lead, platinum, zinc, tin, and in all probability for the other metals, though they were not tried. Glass attracts it with equal facility. Horn of all kinds is extremely magnetic. The hair of the head has been already alluded to, and it must excite surprise that a body so fine could contain such high magnetic properties; and the fact is of extreme use as proving the very circumscribed extent of the spot, or rather of the point, which is connected with the needle in these experiments, for the very finest hair of the head that can be found exhibits as strong an affinity as the thickest of the same kind. Amongst hair the white whisker of a cat, of the mixed Angora breed, must not be omitted, as it was distinguished above all for its attractive properties which it maintained for all the substances with which it was tested.

### 6th Class.

Jet.	°	Spermaceti.	°	Sealing-wax (black).....	25
Sepia.....	24	Composition candle.		India rubber.....	80
Indian ink.....	70	Lac (pure) .....	35	Gas coal .....	15
Amber .....	75	Do. (red) .....	17	Pit coal .....	0
Resin.....	90	Do. (yellow).		Charcoal (of commerce)	0
Bee's wax.		Sealing-wax (red).....	36	Do. (box) .....	0

The substances of this class show no remarkable attraction for the magnetic needle, but this is not surprising when their nature is considered.

### 7th Class.

Deal wood.....	30	Cane* (outside).	°	Vegetable ivory (2nd speci-	°
Box wood.		Do. (inside).		men) .....	0
Rose wood.		Wheat straw.		Do. (3rd specimen) .....	0
Poplar .....	52	Plain card .....	80	Glue .....	29
Oak.		Polished card.		Dry paste on paper .....	0
Lignum vitæ .....	33	Cedar wood .....	0	Letter paper (thin French).	
Beech.		Sandal wood .....	0	Do. (thin English) .....	0
Walnut wood.....	0	Cork .....	0	Do. blue laid (hot pressed)	0
Mahogany.		Vegetable ivory .....	56	Do. thin Bath (hot pressed)	0

\* *Calamus rotang*.

The woods, as might be expected, did not exhibit much attraction for the needle; but I shall have to speak of them again when I treat of the magnetism of non-ferruginous bodies, to which I now proceed.

[Continued at page 502.]

LXIX. *On Quaternions; or on a New System of Imaginaries in Algebra.* By Professor Sir WILLIAM ROWAN HAMILTON, LL.D., V.P.R.I.A., F.R.A.S., Corresponding Member of the Institute of France, and of other Scientific Societies in British and Foreign Countries, Andrews' Professor of Astronomy in the University of Dublin, and Royal Astronomer of Ireland.

[Continued from vol. xxix. p. 328.]

29. **I**F we denote by  $\alpha$  and  $\beta$  two constant vectors, and by  $\rho$  a variable vector, all drawn from one common origin; if also we denote by  $u$  and  $v$  two variable scalars, depending on the foregoing vectors  $\alpha, \beta, \rho$  by the relations

$$\left. \begin{aligned} u &= 2S.\alpha\rho = \alpha\rho + \rho\alpha; \\ v^2 &= -4(V.\beta\rho)^2 = -(\beta\rho - \rho\beta)^2; \end{aligned} \right\} \dots \dots (1.)$$

we may then represent the central surfaces of the second degree by equations of great simplicity, as follows:—

An ellipsoid, with three unequal axes, may be represented by the equation

$$u^2 + v^2 = 1. \dots \dots (2.)$$

One of its circumscribing cylinders of revolution has for equation

$$v^2 = 1; \dots \dots (3.)$$

the plane of the ellipse of contact is represented by

$$u = 0; \dots \dots (4.)$$

and the system of the two tangent planes of the ellipsoid, parallel to the plane of this ellipse, by

$$u^2 = 1. \dots \dots (5.)$$

A hyperboloid of one sheet, touching the same cylinder in the same sheet, is denoted by the equation

$$u^2 - v^2 = -1; \dots \dots (6.)$$

its asymptotic cone by

$$u^2 - v^2 = 0; \dots \dots (7.)$$

and a hyperboloid of two sheets, with the same asymptotic cone (7.), and with the two tangent planes (5.), is represented by this other equation,

$$u^2 - v^2 = 1. \dots \dots (8.)$$

By changing  $\rho$  to  $\rho - \gamma$ , where  $\gamma$  is a third arbitrary but constant vector, we introduce an arbitrary origin of vectors, or an arbitrary position of the centre of the surface, as referred to such an origin. And the general problem of determining that individual surface of the second degree (supposed to have a centre, until the calculation shall show in any particular question that it has none), which shall pass through *nine given*



points, may thus be regarded as equivalent to the problem of finding three constant vectors,  $\alpha, \beta, \gamma$ , which shall, for nine given values of the variable vector  $\rho$ , satisfy one equation of the form

$$\{\alpha(\rho - \gamma) + (\rho - \gamma)\alpha\}^2 \pm \{\beta(\rho - \gamma) - (\rho - \gamma)\beta\}^2 = \pm 1; \quad (9.)$$

with suitable selections of the two ambiguous signs, depending on, and in their turn determining, the particular species of the surface

30. The equation of the ellipsoid with three unequal axes, referred to its centre as the origin of vectors, may thus be presented under the following form (which was exhibited to the Royal Irish Academy in December 1845):

$$(\alpha\rho + \rho\alpha^2) - (\beta\rho - \rho\beta)^2 = 1; \quad (1.)$$

and which decomposes itself into two factors, as follows:

$$(\alpha\rho + \rho\alpha + \beta\rho - \rho\beta)(\alpha\rho + \rho\alpha - \beta\rho + \rho\beta) = 1. \quad (2.)$$

These two factors are not only separately linear with respect to the variable vector  $\rho$ , but are also (by art. 20, Phil. Mag. for July 1846) *conjugate quaternions*; they have therefore a common *tensor*, which must be equal to unity, so that we may write the equation of the ellipsoid under this other form,

$$T(\alpha\rho + \rho\alpha + \beta\rho - \rho\beta) = 1; \quad (3.)$$

if we use, as in the 19th article, Phil. Mag., July 1846, the characteristic  $T$  to denote the operation of taking the tensor of a quaternion. Let  $\sigma$  be an auxiliary vector, connected with the vector  $\rho$  of the ellipsoid by the equation

$$\sigma = \rho(\alpha - \beta)\rho^{-1}; \quad (4.)$$

we shall then have, by (3.), and by the general law for the tensor of a product,

$$T(\alpha + \beta + \sigma) \cdot T\rho = 1; \quad (5.)$$

but also

$$(\alpha - \beta + \sigma)\rho = (\alpha - \beta)\rho + \rho(\alpha - \beta), \quad (6.)$$

where the second member is scalar; therefore, using the characteristic  $U$  to denote the operation of taking the *versor* of a quaternion, as in the same art. 19, we have the equation

$$U(\alpha - \beta + \sigma) \cdot U\rho = \mp 1; \quad (7.)$$

and the dependence of the variable vector  $\rho$  of the ellipsoid on the auxiliary vector  $\sigma$  is expressed by the formula

$$\rho = \pm \frac{U(\alpha - \beta + \sigma)}{T(\alpha + \beta + \sigma)} \quad (8.)$$

Besides, the length of this auxiliary vector  $\sigma$  is constant, and equal to that of  $\alpha - \beta$ , because the equation (4.) gives

$$T\sigma = T(\alpha - \beta); \quad (9.)$$

we may therefore regard  $\alpha - \beta$  as the vector of the centre C of a certain auxiliary sphere, of which the surface passes through the centre A of the ellipsoid; and may regard the vector  $\alpha - \beta + \sigma$  as a variable and auxiliary *guide-chord* AD of the same *guide-sphere*, which chord determines the (exactly similar or exactly opposite) direction of the variable radius vector AE (or  $\rho$ ) of the ellipsoid. At the same time, the constant vector  $-2\beta$ , drawn from the same constant origin as before, namely the centre A of the ellipsoid, will determine the position of a certain fixed point B, having this remarkable property, that its *distance* from the extremity D of the variable guide-chord drawn from A, will represent the *reciprocal of the length of the radius vector*  $\rho$ , or the *proximity*  $(AE)^{-1}$  of the point E on the surface of the ellipsoid to the centre (the use of this word "proximity" being borrowed from Sir John Herschel). Supposing then, for simplicity, that the fixed point B is external to the fixed sphere, which does not essentially diminish the generality of the question; and taking, for the unit of length, the length of a tangent to that sphere from that point; we may regard AE and BD' as two equally long lines, or may write the equation

$$\overline{AE} = \overline{BD'}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (10.)$$

if D' be the other point of intersection of the straight line BD with the sphere.

31. Hence follows this very simple *construction\* for an ellipsoid* (with three unequal axes), by means of a sphere and an external point, to which the author was led by the foregoing process, but which may also be deduced from principles more generally known. From a fixed point A on the surface of a sphere, draw a variable chord AD; let D' be the second point of intersection of the spheric surface with the secant BD, drawn to the variable extremity D of this chord AD from a fixed external point B; take the radius vector AE equal in length to BD', and in direction either coincident with, or opposite to, the chord AD; *the locus of the point E, thus constructed, will be an ellipsoid*, which will pass through the point B.

\* This construction has already been printed in the Proceedings of the Royal Irish Academy for 1846; but it is conceived that its being reprinted here may be acceptable to some of the readers of the London, Edinburgh, and Dublin Philosophical Magazine; in which periodical (namely in the Number for July 1844) the first *printed* publication of the fundamental equations of the theory of quaternions ( $i^2 = j^2 = k^2 = -1$ ,  $ij = k$ ,  $jk = i$ ,  $ki = j$ ,  $ji = -k$ ,  $kj = -i$ ,  $ik = -j$ ) took place, although those equations had been communicated to the Royal Irish Academy in November 1843, and had been exhibited at a meeting of the Council during the preceding month.

92. We may also say that if of a quadrilateral ( $ABED'$ ), of which one side ( $AB$ ) is given in length and in position, the two diagonals ( $AE$ ,  $BD'$ ) be equal to each other in length, and intersect (in  $D$ ) on the surface of a given sphere (with centre  $C$ ), of which a chord ( $AD'$ ) is a side of the quadrilateral adjacent to the given side ( $AB$ ), then the other side ( $BE$ ), adjacent to the same given side, is a chord of a given ellipsoid. The form, position, and magnitude of an ellipsoid (with three unequal axes), may thus be made to depend on the form, position, and magnitude of a generating triangle  $ABC$ . Two sides of this triangle, namely  $BC$  and  $CA$ , are perpendicular to the two planes of circular section; and the third side  $AB$  is perpendicular to one of the two planes of circular projection of the ellipsoid, because it is the axis of revolution of one of the two circumscribed circular cylinders. This triple reference to circles is perhaps the cause of the extreme facility with which it will be found that many fundamental properties of the ellipsoid may be deduced from this mode of generation. As an example of such deduction, it may be mentioned that the known proportionality of the difference of the squares of the reciprocals of the semiaxes of a diametral section to the product of the sines of the inclinations of its plane to the two planes of circular section, presents itself under the form of a proportionality of the same difference of squares to the rectangle under the projections of the two sides  $BC$  and  $CA$  of the generating triangle on the plane of the elliptic section.

[To be continued.]

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LXX. *Observations on the Analysis of the Spectrum by Absorption.* By Sir DAVID BREWSTER, K.H., D.C.L., F.R.S., and V.P.R.S. Ed.\*

IN a paper On the Production of Light and Heat by Dr. Draper, published in the last Number of this Journal, the ingenious author has made some observations on my Analysis of the Solar Spectrum, which it is necessary that I should notice. While he admits that I have "shown that *red, yellow, blue*, and consequently *white light* exist in every part of the spectrum," he ascribes this result to the non-coincidence, or the overlapping of the colours of the several spectra formed on a screen by the rays falling near the edge, and those falling near the back of a prism with a refracting face of considerable magnitude. "In such a spectrum," he says, "there must undoubtedly be a general commixture of the rays; but may we not fairly inquire whether, if an elementary prism were

\* Communicated by the Author.

used, the same facts would hold good; or, if the anterior face of the prism were covered by a screen, so as to expose a narrow fissure parallel to the axis of the instrument, would there be found in the spectrum it gave every colour in every part, as in Sir David Brewster's original experiment?"

As my experiments were not made upon spectra formed upon screens by prisms with large refracting surfaces, they are not liable to this criticism, even if it were otherwise well-founded. The spectra which I use are so pure, and free from all commixture, that Fraunhofer's *black* lines are distinctly visible; and the results are precisely the same when the refracting face of the prism is reduced to the smallest possible dimensions.

My analysis of the spectrum by absorption, therefore, does not *indirectly controvert* the principle, that "to a particular colour there ever belongs a particular wave-length, and to a particular wave-length there ever belongs a particular colour," as Dr. Draper states, in theoretical language, the well-known proposition of Sir Isaac Newton, but it *directly controverts* it, and *absolutely overturns* it.

St. Leonard's College, St. Andrews,  
May 13, 1847.

## LXXI. *Intelligence and Miscellaneous Articles.*

### ON THE PREPARATION OF THE AMMONIACO-MAGNESIAN PHOSPHATE FROM URINE. BY M. BOUSSINGAULT.

THE author remarks that some time since Mr. Stenhouse proposed a process for rendering the phosphoric acid and alkaline phosphates contained in urine useful in agriculture. It is indeed sufficient to add milk of lime to it, to occasion a precipitate of phosphate of lime.

It occurred to M. Boussingault that it was possible, by means of a magnesian salt, to collect both the phosphoric acid and a part of the ammonia developed during the putrefaction of the urine; and the result was that of obtaining a manure which contains two substances useful to vegetation—phosphoric acid and ammonia.

In the month of June M. Boussingault mixed a solution of hydrochlorate of magnesia with about 123 pounds of urine, fresh collected in the morning. At the end of five days the urine assumed a milky appearance, and from this time the deposit of ammoniaco-magnesian phosphate increased rapidly. A month afterwards the solution was poured off, and the phosphate was collected on a cloth to wash it. The salt was very white, and in small well-terminated crystals; when dried in the air, it weighed about one pound and nearly 100 grains avoirdupois. The presence of the magnesian salt greatly diminished the infectious odour emitted by the putrefying urine. It will be ob-



served besides, that no disengagement of carbonate of ammonia would occur, for the alkali developed was immediately converted into a fixed salt.

M. Boussingault observes that the above-described process appears to be susceptible of being advantageously employed in situations in which a large quantity of urine may be collected, as in the workshops, hospitals, prisons, &c. of towns, because an efficacious manure would be obtained which is particularly easy of transport. The diminution of the bad smell ought also to be considered; for it is evident that a magnesian salt acts under these circumstances as a disinfectant. In establishments situate in agricultural districts, the process would possess but little utility, because the urine could be there employed immediately and without any preparation; and the only advantage which could result from the use of a magnesian salt under these circumstances, would be the prevention of the loss of ammonia, a loss which the author thinks has been much exaggerated. —*Ann. de Ch. et de Phys.*, Mai 1847.

#### CHEMICAL RESEARCHES ON THE YOLK OF EGGS.

M. Gobley states that the fresh researches which he has made on this subject were undertaken with a two-fold object, one of which was to consider certain points which he had scarcely touched upon in his first communication, another object was to determine how far the objections of M. Sacc against his conclusions were well-founded; with these views the author modified several of his analytical processes, so as to be quite certain that he would avoid the causes of the errors which had been supposed to be discovered.

M. Gobley concludes from his recent experiments,—

1st. That the fatty matter of the yolk of egg is formed, as had previously been stated, of two distinct parts; one a fixed oil, the *oil of egg*, and of a soft, not fusible substance, *viscous matter*.

2nd. That the phosphorus does not occur in the oil, but in the viscous matter.

3rd. That oleic, margaric, phosphoglyceric, lactic acids, and the extract of meat are not products of oxidizement.

4th. That the viscous matter is not, as the author imagined, a combination of oleic, margaric and phosphoglyceric acids with ammonia, but constitutes a body of a complex nature from which two different substances have been separated, to which the author has provisionally given the names of *phosphorized matter* and *cerebric matter*.

5th. That the phosphorized matter which forms in the future animal the substance described by M. Fremy as *oleophosphoric acid*, yields oleic, margaric and phosphoglyceric acids with the greatest facility, as products of decomposition in the presence of acids and mineral alkalis under the influence of water or alcohol, and without the intervention of the oxygen of the air.

6th. That the cerebric matter is analogous, if not identical with the substance which Vauquelin, MM. Couerbe and Fremy have succes-

sively described under the names of *fatty matter of the brain*, *cérébrote* and *cerebric acid*.—*Comptes Rendus*, Avril 12, 1847.

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ON PLASTER OF PARIS. BY M. PLESSY.

All chemists have agreed with M. Gay-Lussac, that sulphate of lime which had lost the whole of its water of crystallization by the action of heat, amounting to 21 per cent. or two equivalents, might be re-hydrated, and the setting of plaster was explained by it. M. Millon has, however, communicated to the Academy the following remarks: "Sulphate of lime heated to 230° F. loses  $1\frac{1}{2}$  equivalent of water; at 284° to 293° F., mere traces of water are separated; lastly, when heated to 572°, the plaster is rendered anhydrous and does not then set."

These results, opposed to those obtained by M. Gay-Lussac, induced M. Plessy to occupy himself with a question which chemists had supposed to have been long settled, the consideration of which M. Millon has resumed.

The facts stated by M. Plessy are,—1st, that gypsum and crystallized sulphate of lime, artificially prepared, lose 21 per cent. of water at 230° to 239° F. in a current of dry hydrogen, a gas which is well known to exert no action on the salt under the circumstances in which M. Plessy operated; 2nd, plaster heated in the open air from 230° to 239° F. readily lost 15 per cent. of water, and there then appeared a stop to occur; but at 266° to 284° F., the limit at which plaster is dehydrated, according to Mr. Graham, M. Plessy separated, not traces of water, but 2 per cent., and this was effected in a very short space of time; lastly, at 392° to 482° F., the plaster operated on suffered a total loss of 21 per cent. The author satisfied himself that it was anhydrous, by heating it strongly with a spirit-lamp, and yet it recombined with the whole of its water of crystallization so perfectly, that after having set, it lost 21 per cent., or two equivalents of water.

It appears from the above statement that plaster does not retain very precisely half an equivalent of water; for at 270° to 284° F., the loss is 17 per cent., and the half equivalent of water is greatly endangered; the compound  $S^2O^6 2CaO, HO$  of M. Millon is destroyed, and yet it is formed at 230° to 239° F.; at this temperature, moreover, it does not exist in a current of dry gas; this combination has, notwithstanding, been obtained. M. Johnston found in the boiler of a steam-engine, prismatic crystals of sulphate of lime which contained half an equivalent of water.

It results from the experiments now detailed, that sulphate of lime which has lost the whole of its water of crystallization, may regain it when they are brought into contact.

In concluding, M. Plessy remarks that his results are opposed to those of M. Millon, but that they have been previously announced by M. Gay-Lussac and several other chemists.—*Comptes Rendus*, Avril 12, 1847.

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LXXII. *On the Phenomena exhibited by Polarized Light when its plane of polarization is rapidly rotated.* By Professor DOVE\*.

AS unpolarized light exhibits the same physical properties in all planes throughout the ray, whilst in rectilinearly polarized light these are different in the different planes, and attain their maxima and minima in two planes situated at right angles to each other, and gradually pass into one another by intermediate stages in those which lie between them, the question arises, what will be the properties of polarized light the plane of polarization of which is put into rapid rotation. The experimental answer to this question gives rise to very numerous phenomena, which illustrate in a remarkable manner several positions in the theory of light, which have as yet been only theoretically investigated.

Two supports of the same height as the other supports were placed to the triangular prism of the polarizing apparatus, described by the author in the *Scientific Memoirs*, vol. i. p. 86, in the fixed rings of which, hollow rollers could be set in rapid rotation by a band passing round a wheel with two grooves. Both the rollers can rotate in the same direction, and, by crossing one of the bands, in the opposite direction. Within and on both sides of the hollow rollers, Nichol's prisms, circularly polarizing plates of mica or ground crystalline plates can be screwed. The condensing lens of the apparatus is so placed as to concentrate the light of an Argand or monochromatic lamp in the polarizing apparatus, whether at rest or in rotation. The results of the experiments were as follows:—

1. Light which is completely polarized rectilinearly when the Nichol's prism is at rest, possesses exactly its ordinary properties when it is set in uniform rapid rotation, if the analysing apparatus is not rotated. On examination by a doubly

\* Translated by Dr. Griffith from the *Berichte der Berliner Akademie* for Feb. 1847.

refracting prism, when the rotation is slow, it constantly exhibits figures of the same brightness. The intense complementary colours of their overlapping margins, on introducing a lamina of selenite, mica, or a thin layer of calcareous spar, completely disappear. The complementary system of rings of calcareous spar and apophyllite, ground at right angles to the axis, the splendid isochromatic curves of nitre, ground at right angles to the line bisecting the angle formed by the two axes, completely neutralize each other, becoming white. The same applies to the colours of unannealed and compressed glass. The centre of the system of rings which in rock-crystal (on slowly rotating the polarizing apparatus) passes through the colours of the spectrum, appears perfectly colourless on rapid rotation.

We thus have in the rotating polarizing apparatus, a circle of colours for the complementary colours; and not only for those which occur in Newton's rings, but also for the complicated system of rings of those crystals the binary axes of which are either situated in one plane with several lines of bisection, or in different planes with the same line of bisection (selenite, borax).

We may easily convince ourselves of the true existence of the polarization on the one hand, and the coloured curves on the other, however rapid the rotation may be. We need only place a Leyden jar, which spontaneously discharges itself, in the situation of the lamp, to illuminate for an instant the Nicol's prism rotating in the dark, and to convert the rotation into apparent repose\*. With monochromatic light, the dark lines of interference vanish in a uniform illumination. If the red system of rings be separated from the violet in a binaxial crystal by a glass coloured by cobalt, both disappear during the illumination in an indefinite coloured mixture, in which it is indifferent whether, as in nitre, the red axes form the lesser angle, or, as in carbonate of lead, the violet.

\* If light which has been perfectly polarized by reflexion from a mirror be transmitted first through an unannealed glass and then through two glass appendages, of which the refracting plane of one is situated in the reflecting plane of the polarizing mirror, the refracting plane of the other being at right angles to it, the colours of the cooled glass entirely vanish and it appears white, provided each of the two analysing apparatus is so inclined to the incident ray as to polarize an unpolarized incident ray to the same extent. With the slightest alteration of either apparatus the complementary images immediately appear, when the figure previously appeared colourless. This at once affords a very simple means of comparing the power of two polarizing apparatus, based upon simple refraction, and of illustrating the views of Sir David Brewster on the dependence of the polarization upon a number of plates, their refractive power, and the incidence of the light.



2. If the polarizer and analyser be rotated with equal velocity in opposite directions, that system of rings is seen which is obtained when both are in repose, and inclined to each other at an azimuth of  $45^\circ$ . This is particularly distinct when the light is rendered monochromatic by using a flame of spirit, coloured yellow by common salt. The cause of this phenomenon, which appears so remarkable at first, is at once seen when we recollect that, when one apparatus only rotates, the analysing or the polarizing, during an entire revolution, the arms of the white cross fall twice as often on the same spot as those of the black, and the dark lines of interference alternate with the bright lines of coincidence exactly in the same spots. When however both apparatus rotate in opposite directions, the system of rings with the white cross appears at the points  $0^\circ$ ,  $90^\circ$ ,  $180^\circ$ ,  $270^\circ$ , whilst the system of rings with the black cross corresponds to the points  $45^\circ$ ,  $135^\circ$ ,  $225^\circ$  and  $315^\circ$ . As the bright arms of the white coincide with the coloured spaces of the dark cross, the impressions of the two are added, and we obtain the appearance above stated. If the rapidity of rotation of both rollers is exactly the same, the figure appears motionless; but if the rapidity of rotation of the two is slightly different, it slowly revolves, because the points of coincidence gradually alter.

3. If the polarizing and analysing apparatus revolve rapidly with the same velocity in the same directions, the linear system of rings becomes converted into that which is obtained when circularly polarized light is circularly analysed. If, before rotation, the Nichol's prisms are so placed that in calcareous spar the black cross is visible, on rotation we obtain the Newtonian reflected rings with a black centre, but without the cross; but if, when at rest, the white cross is visible, we then see, on rotation, the Newtonian transmitted rings with a white centre. If the polarizing and analysing apparatus do not rotate with the same rapidity, we see the remarkable phenomenon, that the reflected Newtonian rings with the black centre, and the transmitted ones with the white centre, constantly alternate in definite periods in the same spot of the field.

4. If a circularly polarizing apparatus be rotated in an opposite direction to the linear analyser, the same phenomenon is seen as when a linear polarizer and a linear analyser revolve in opposite directions, but it is less distinct (2).

5. If the circularly polarizing apparatus be rotated, and the linear analyser be kept at rest, we obtain the system of rings appearing at the quadrants exactly on rotation as on repose.

6. If the circularly polarizing apparatus be kept at rest,

and the linear analyser be rotated, we obtain a faint system of rings without a cross, with a gray centre surrounded by a black circle. The direction of the rotation is here indifferent.

7. If the circularly polarizing apparatus be rotated in the same direction as the linear analyser, we obtain the appearance, but fainter, which is seen when a linear polarizer and a linear analyser are rotated in the same direction, the rotating velocity not being the same; hence the alternating system of rings (3).

8. But if a circularly polarizing and a circularly analysing apparatus be rotated, either in the same or in opposite directions, this has no influence on the appearance, which is perfectly identical with that which is seen when both apparatus are at rest.

9. If a lamina of mica, circularly polarizing at an azimuth of  $45^\circ$ , be introduced between the Nichol's prisms, and be kept at rest whilst the polarizing Nichol's prism rotates, the systems of rings neutralize each other on linear and circular analysis, forming white light, in uni- and binaxial crystals and in rock-crystal.

10. If the lamina of the polarizing apparatus, which produces circular polarization at an azimuth of  $45^\circ$ , be rotated between the Nichol's prisms which are at rest, we obtain as the resultant of all the phænomena of light polarized elliptically, circularly and rectilinearly in a single plane, on linear analysis, the phænomena of partly rectilinearly polarized light, on circular analysis, those of a mixture of ordinary and circular light, *i. e.* in the first case the system of rings with a dark cross, in the latter the system of rings which vanishes at the quadrants; they are however faint.

In all the foregoing experiments the Nichol's prisms were crossed before the rotation was commenced.

11. It is so difficult to set two laminæ of mica or two Nichol's prisms either at an exactly equal or exactly opposite rotating velocity by means of a band passing round a wheel, that in those experiments in which the laminæ of mica being kept at rest, the Nichol's prisms rotate in the same or in opposite directions, or in which the Nichol's prisms being at rest, the laminæ of mica rotate, the relative position of the laminæ or of the Nichol's prisms is constantly becoming somewhat changed, and hence the phænomena of elliptical become mixed with those of the circular light. Hence, for the production of these phænomena, the motion must be effected by wheel-work.

12. Light, originally polarized rectilinearly, when transmitted through a rotating Nichol's prism and then analysed, yields light partially polarized in the original plane.

13. A rotating velocity, which continues to decrease and increase through an entire revolution, in (1) and (9) produces partially polarized instead of ordinary light.

14. Unannealed glasses rotating between Nichol's prisms at rest, do not neutralize each other and form white light.

15. Light from a rotating Nichol's prism falling upon a metallic mirror, possesses the ordinary characters.

16. The twin-growth of crystals gives rise to very complicated phænomena of colour. This is very beautifully seen in plates of calcareous spar, ground at right angles to the axis of the inclosing individual. The author has formed some of them artificially, by introducing a lamina of selenite of a definite thickness between two accurately centred plates\*. If a plate of a natural twin-crystal be rotated, after having been accurately centred as regards the axis of rotation, the colours become neutralized, forming white light; whilst a simple plate, rotated on its axis, preserves its system of rings unchanged.

N.B. [The phænomena of *interrupted crystals*, described in paragraph 16 of this paper, were observed by Sir David Brewster previous to 1819; and the method of forming them artificially by enclosing laminæ of selenite and mica between accurately centred plates of calcareous spar, and between equal plates of the *spath calcaire basée*, was practised by him previously to March 1827, the date of which is signed, in his *Journal of Experiments*, by the Vice-President of the Royal Society of Edinburgh. The experiments, too, are briefly described in his *Treatise on Optics*, Part II. chap. xxii. § 115, p. 197.—Ed.]

### LXXIII. On the Depolarization of Light. By Prof. DOVE.

IF rectilinearly polarized light be allowed to fall upon a rough surface, as a white wall, it is perfectly depolarized. This phænomenon is most distinctly seen, by allowing the spectra of a prism of rock-crystal, polarized at right angles to each other, to fall upon such a surface, and analysing the purple-red resulting from the violet end overlapping the red extremity, with a Nichol's prism. On rotating the latter, there is not the slightest alteration in colour. This depolarization, which eight years ago the author, in a paper communicated to the Berlin Society of Friends of Natural History, showed also to occur on the rough internal surface of bodies covered with a reflecting layer, progressively diminishes with the obliquity of the incidence, so that even the dullest surface ceases to depolarize at very acute incidences. With perpendicular incidence, it also extends to circular and elliptical light, which, when reflected by a rough surface, is depolarized, *i.e.* possesses the properties of common light. If the explanation of the depolarizing property of rough surfaces is found in the supposition that such a surface reflects in all directions, by the converse, the combination of light polarized in all possible

\* Poggendorff's *Annalen*, vol. xxxv. p. 594.

planes yields common light by reflexion. A truncated hollow cone, the smaller circular section of which was about fourteen lines in diameter, and the larger seventeen lines, was ground into a glass plate, four lines in thickness and three inches in diameter. The lowermost surfaces of the glass were roughened at the margin of the circular section and blackened, and the larger circular section was covered with a glass plate, upon which a circular piece of tin-foil was pasted, so that its centre coincided with the axis of the truncated cone. The angle of the cone at the apex amounted to  $70^{\circ} 50'$ . When this apparatus is turned towards the sun, so that the rays fall at right angles to the base of the cone, they enter the annular space between the tin-foil and the dark coating of the sheath, and arrive at the reflecting surface of the hollow cone at an angle of  $35^{\circ} 25'$ , thus becoming polarized in all the planes of reflexion, and after reflexion, crossing at one point of the axis. In the apparatus made by Cœrtling, this point was exactly in the plane of the smaller aperture of the cone, and was visible when received upon the surface of a piece of white paper placed there. As the polarized light would be depolarized on account of the depolarizing action of this rough surface, it could only be determined that it was unpolarized. This was effected by polarizing the light incident upon the cone by means of an inserted glass plate, and introducing a plate of mica between the inserted glass plate and the cone. The point remained colourless.

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LXXIV. *On Thialdine and Selenaldine, two new artificial organic bases. By WÖHLER and LIEBIG. Communicated by Dr. A. W. HOFMANN\*.*

1. *Thialdine.*

**T**HE peculiar deportment of hydrated cyanic acid with aldehyde, which gives rise to a new acid containing the elements of aldehydite of ammonia and hydrated cyanic acid, induced us to study the action of some other substances on aldehyde and aldehydite of ammonia.

The most interesting product which we obtained along with others, is a new organic alkali free from oxygen, and containing carbon and hydrogen in the same proportions as acetic acid, combined with sulphur and the elements of sulphide of ammonium. This substance, which we call thialdine†, is highly remarkable from its composition, and we have no doubt that the method which led to its discovery, when applied to other compounds analogous to aldehyde and aldehydite of ammonia, will yield a long series of new and inter-

\* Communicated by the Chemical Society; having been read January 4, 1847.

† Contracted from *θειον* and aldehyde.



esting products. The study of these substances promises to become highly important for the theory of the formation of organic bases, in which the action of ammonia is frequently very prominent, and it may be expected that a close investigation of the production of organic substances containing sulphur will throw some light upon their generation in the organism of the plant.

The preparation of thialdine is very simple. One part of aldehydite of ammonia free from æther and alcohol is dissolved in from twelve to sixteen parts of water; one ounce of this liquid is mixed with from ten to fifteen drops of solution of ammonia, and treated with a slow stream of hydrosulphuric acid; after half an hour the solution begins to become turbid, and crystals are deposited of the appearance of camphor and of considerable size; after four or five hours the liquid again becomes clear and the operation is terminated. The crystals are collected on a funnel and washed with water until all the sulphide of ammonium is separated. Pressure between bibulous paper removes the adhering water. The dry crystals are dissolved in æther, the solution mixed with a third of its volume of alcohol and left to spontaneous evaporation. The liquid soon deposits very regular rhombic tables, which can be obtained of half an inch in dimensions if too rapid an evaporation of the solution has been avoided.

If no more of the solution is left than is required to cover the crystals formed, the mother-liquid is decanted off; the crystals are then dried by leaving them for some time between folds of bibulous paper. The mother-liquor yields some more thialdine on evaporation. The crystals thus obtained are however not quite colourless. The remaining liquid contains much sulphide of ammonium, part of which arises from the ammonia added.

It happens sometimes that on passing hydrosulphuric acid gas through the solution of aldehydite of ammonia, no crystalline body, but a heavy colourless oil of offensive odour is deposited. This oil is a mixture of two bodies, the greater part being thialdine, the fusing-point of which is depressed to the temperature of the atmosphere by the presence of a liquid substance\*. In order to obtain pure thialdine from this mixture, the supernatant aqueous layer is separated, and the remaining liquid (consisting principally of the oil and but a small amount of the aqueous solution) is agitated with half its bulk of æther, which instantaneously dissolves the oil. The æthereal solution, which may be easily separated from

\* Crystals of thialdine when placed in contact with sulphide of ammonium for some time are entirely converted into a heavy oil, insoluble in water, which has not been further examined.

the water, is agitated in a stoppered bottle with a little concentrated hydrochloric acid. The mixture generally solidifies as a mass of fine needles, which may be separated from the oil by washing with æther. In this manner hydrochlorate of thialdine is obtained, from which pure thialdine may easily be prepared by moistening the dry crystals with a concentrated solution of ammonia, and extracting the separated base by means of æther. The æthereal solution yields by spontaneous evaporation crystals of pure thialdine. Addition of alcohol to the æthereal solution facilitates the crystallization exceedingly; the crystals become larger, more regular, and perfectly transparent.

Pure thialdine is heavier than water; its specific gravity is 1.191 at 18° C.; it forms large, transparent, colourless, shining crystals of the form of common gypsum, possessing a highly refracting power and a peculiar aromatic, but after some time very disagreeable odour. The crystals fuse at 43° C.; the fused crystals solidify at 42°; they are volatile at the temperature of the atmosphere without residue. Thialdine distils with the vapour of water, but is decomposed when distilled alone, a heavy oil of offensive odour passes over, solidifying only partly and after some time, whilst a brown, syrupy residue containing sulphur is left in the retort. Thialdine in this respect resembles aldehydite of ammonia, which, though volatile itself, cannot be exposed to a higher temperature without decomposition. If a crystal of thialdine remains for several hours in an atmosphere containing acid vapours, such as is the case in the laboratory, a white envelope of fine silky needles is found at a certain distance from the planes of the crystals.

Thialdine is but little soluble in water; it is soluble in alcohol, and much more so in æther; when powdered it deliquesces at the common temperature in vapour of æther, or in an atmosphere containing vapour of æther.

An alcoholic solution of thialdine exhibits the following reactions:—

With acetate of lead, at first no precipitate, but after some time a yellow deposit appears, which soon becomes red and finally black.

Nitrate of silver causes a precipitate, which at first is white, then yellow, afterwards black.

With chloride of mercury a white precipitate is produced, turning rapidly yellow.

With bichloride of platinum a dingy, yellow precipitate is thrown down after some time.

Thialdine has no reaction on vegetable colours; it is soluble in all acids with which it combines, forming crystallizable salts; the hydrochlorate and the nitrate are remarkable for

the facility with which they crystallize and for the beauty of the crystals.

Thialdine and its salts are decomposed when gently heated with a solution of nitrate of silver; sulphide of silver is produced with disengagement of a combustible gas, easily inflammable and possessing the odour and all the properties of aldehyde; namely, it is entirely absorbed by concentrated sulphuric acid and solution of potassa, the former becoming brown immediately, the latter remaining clear but depositing when gently heated a yellowish-brown precipitate of resin of aldehyde, and assuming the peculiar and offensive odour which accompanies the decomposition of aldehyde under these circumstances.

When thialdine is ignited with lime, chinoline appears among the products of decomposition, which may be easily recognised by its characteristic odour and by its beautiful platinum salt.

With cyanide of mercury thialdine exhibits a peculiar comportment. On mixing the two solutions a white precipitate is produced, which on boiling is converted into amorphous, black sulphide of mercury. If this decomposition takes place in a retort its neck becomes coated with fine crystalline needles, extremely volatile, insoluble in water but easily soluble in alcohol and æther. This new body, in which part or all the sulphur appears to be replaced by cyanogen, is formed in but small quantities, which up to this moment has prevented us from studying it any further.

The analysis of thialdine was performed in the usual way; the combustion with oxide of copper is attended with difficulties, as the conversion of carbon into carbonic acid is obstructed by the large quantity of sulphur contained in the body forming a corresponding amount of sulphide of copper at the point of contact between the substance and oxide of copper.

On burning thialdine with oxide of copper and separating the sulphurous acid formed by means of peroxide of lead, a gas is obtained containing volumes of nitrogen and carbonic acid in the proportion of 1 : 12. Aldehydite of ammonia, from which thialdine is produced, contains 4 equivs. of carbon to 1 equiv. of nitrogen, from which it is evident that aldehydite of ammonia on being converted into thialdine by means of hydrosulphuric acid has lost two-thirds of its nitrogen.

We have mentioned that thialdine, on being gently heated with a solution of nitrate of silver, has its sulphur converted into sulphide of silver. This mode of decomposition was employed to determine the sulphur. The nitrogen of the thialdine remains in the residuary liquid in the form of ammonia, from

which it was precipitated by bichloride of platinum, and determined in the usual way\*.

0.3633 grm. of thialdine gave 0.5845 grm. of carbonic acid and 0.267 grm. of water.

0.509 grm. of thialdine gave 0.816 grm. of carbonic acid and 0.3720 of water.

0.4508 grm. of thialdine gave 0.566 grm. of ammoniochloride of platinum.

0.6430 grm. of thialdine gave 1.923 grm. of sulphide of silver.

0.3140 grm. of thialdine gave 0.8420 grm. of water.

The atomic weight having been ascertained by the analysis of the hydrochlorate and nitrate, the preceding numbers lead to the following formula—



as will be seen on comparing the theoretical and experimental numbers.

			Theory.	Mean of experiments.
12 eqs. of Carbon	. .	72	44.17	43.80
13 ... Hydrogen	. .	13	8.04	8.04
1 ... Nitrogen	. .	14	8.58	8.54†
4 ... Sulphur	. .	64	39.26	39.14
		<u>163</u>		

*Hydrochlorate of Thialdine.*—Thialdine is dissolved in large quantity by hydrochloric acid: the saturated solution has an acid reaction. On spontaneous evaporation, or concentration on the water-bath, it deposits large, regular, transparent prisms of great lustre, and frequently an inch in length. The hydrochloric solution of crude thialdine contains in admixture a foreign body of most offensive odour; on agitating with æther this odour disappears immediately, and the liquid which formerly was turbid becomes perfectly transparent. Hydrochlorate of thialdine is rather soluble in water, less so in alcohol; both liquids dissolve a large quantity when hot, and deposit beautiful and perfectly regular crystals on cooling. Hydrochlorate of thialdine is insoluble in æther. When heated in the dry state this salt is decomposed without fusion; it becomes brown, and chloride of ammonium sublimes with evolution of a gas possessing a most offensive odour and burning with a dull flame. From the solution of this salt nitrate of silver throws down a yellow precipitate, which, on being gently heated, turns black with evolution of aldehyde. This black precipitate is a mixture of chloride and sulphide of silver; all the nitrogen of the base remains in the liquid in the form of ammonia.

\* The following analyses were made by Dr. Strecker at Giessen, and Dr. Städler in Göttingen.

† Calculated from the proportion of 12 C : 1 N.



0.4577 gm. of hydrochlorate of thialdine gave 0.5933 gm. of carbonic acid and 0.2850 gm. of water, corresponding to 35.35 per cent. of carbon and 6.92 per cent. of hydrogen.

0.726 gm. of hydrochlorate of thialdine gave 0.789 gm. of ammonio-chloride of platinum.

0.7735 gm. of hydrochlorate of thialdine gave 0.8225 gm. of ammonio-chloride of platinum. In the mean, 6.79 per cent. of nitrogen.

0.7798 gm. of hydrochlorate of thialdine gave 1.890 gm. of sulphide of silver = 32.09 per cent. of sulphur.

0.7735 gm. of hydrochlorate of thialdine gave 1.915 gm. of sulphide of silver = 31.92 per cent. of sulphur.

0.7598 gm. of hydrochlorate of thialdine was precipitated with nitrate of silver and boiled until evolution of gas had ceased; the precipitate, when extracted with ammonia and precipitated with nitric acid, gave 0.5283 gm. of chloride of silver = 17.14 per cent. of chlorine.

0.7735 gm. treated in the same way, gave 0.5405 gm. of chloride of silver = 17.24 per cent. of chlorine.

0.776 gm. dissolved in alcohol and precipitated in the same manner with nitrate of silver, gave 1.912 gm. of sulphide of silver = 31.903 per cent. of sulphur and 0.551 gm. of chloride of silver = 17.551 per cent. of chlorine.

0.441 gm. ignited with a mixture of lime and nitrate of potassa, gave 0.32 gm. of chloride of silver = 17.94 per cent. of chlorine.

These results lead to the following formula for hydrochlorate of thialdine,—



as may be seen from the following comparison between the theoretical and experimental numbers:—

		Theory.	Mean.
12 eqs. of Carbon . .	72	36.10	35.35
14 ... Hydrogen . .	14	7.02	6.79
1 ... Nitrogen . .	14	7.02	6.92
4 ... Sulphur . .	64	32.09	31.97
1 ... Chlorine . .	35.4	17.77	17.47
	199.4	100.00	

*Nitrate of Thialdine.*—This salt may be obtained by mixing this base directly with dilute nitric acid; it is, however, preferable to dissolve crude thialdine in æther, and agitate the liquid with moderately concentrated nitric acid; the solution solidifies into a crystalline mass, which is washed with æther, dissolved in water, and crystallized by evaporation and cooling.

Nitrate of thialdine forms fine white needles, more soluble in water than the hydrochlorate; they are soluble in cold, and more so in hot alcohol, from which liquid they likewise

recrystallize on cooling: they are insoluble in æther; on heating they fuse and are decomposed.

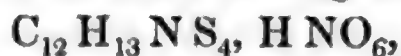
On burning this salt with chromate of lead, more accurate numbers for the carbon and hydrogen were obtained than in the analysis of thialdine itself or the hydrochlorate, a result which can easily be explained by the co-operation of the oxygen in the nitric acid.

0.357 grm. of nitrate of thialdine gave 0.4155 grm. of carbonic acid and 0.2045 grm. of water.

0.514 grm. of nitrate of thialdine gave 1.116 grm. of sulphide of silver.

0.6696 grm. of nitrate of thialdine, when ignited with a mixture of potassa and nitre, gave 1.4063 grm. of sulphate of baryta; the mean of the two latter experiments equals 28.4 per cent. of sulphur.

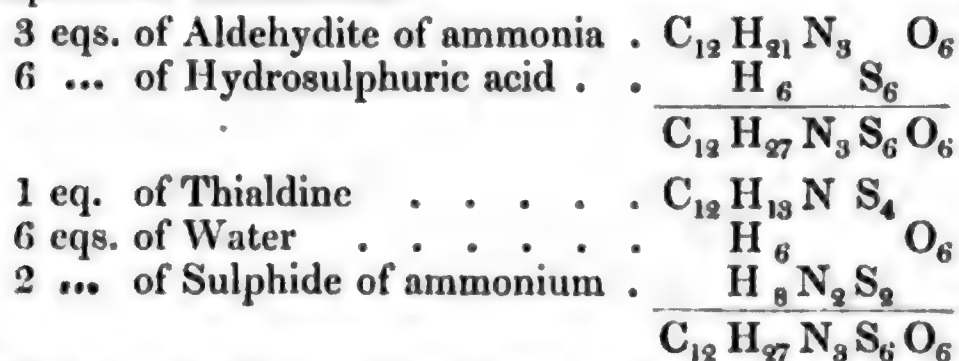
These results lead to the following formula for nitrate of thialdine,—



as will be seen by the following comparison between the theoretical and experimental numbers:—

		Theory.	Experiment.
12 eqs. of Carbon	. 72	31.80	31.75
14 ... Hydrogen	14	6.19	6.36
2 ... Nitrogen	28		
4 ... Sulphur	. 64	28.34	28.40
6 ... Oxygen	. 48		
	<u>226</u>		

The formation of thialdine is easily accounted for: 3 eqs. of aldehydite of ammonia and 6 eqs. of hydrosulphuric acid transpose into 1 eq. of thialdine, 6 eqs. of water, and 2 eqs. of sulphide of ammonium.



But the question respecting the real constitution of this body is too closely connected with that of the true constitution of organic bases in general to admit of any definite opinion being promulgated at this moment, when so few experiments on the subject are as yet before us. Still we can imagine how important for the general question facts like the formation of thialdine may hereafter become, and that just from this quarter perhaps a way for its solution may be ob-

tained. We will here merely allude to an explanation of the constitution and basic character of thialdine, at once obvious, namely, that it may be regarded as a copulated compound of sulphide of ammonium with 3 eqs. of a body



which is the sulphide corresponding to the oxide in aldehyde, namely, sulphide of acetyl.

## 2. Selenaldine.

The existence of thialdine and the mode in which this base is formed, induced us to try the preparation of a corresponding selenium compound. We perfectly succeeded in obtaining that body: selenaldine, however, is so soon and easily altered, that we have not as yet been able to subject it to a more profound investigation.

Selenaldine was produced by passing into a concentrated solution of aldehydite of ammonia hydroselenic acid gas, disengaged from selenide of iron by means of sulphuric acid. In order to prevent the decomposition of hydroselenic acid by the oxygen of the atmosphere, the whole apparatus had been filled previously with hydrogen. The excess of the poisonous hydroselenic acid was condensed in a potassa apparatus. After some time the solution of aldehydite of ammonia begins to become turbid, and crystals of selenaldine are deposited. When the mass of crystals is no longer augmented, the excess of hydroselenic acid is expelled by a stream of hydrogen gas; the solution of selenide of ammonium, covering the crystals, which soon turns red and deposits selenium, is now displaced by introducing a stream of cold water, free from atmospheric air, through one of the glass tubes connected with the bottle. The crystals are then collected upon a filter, pressed between folds of bibulous paper and dried over sulphuric acid.

Selenaldine, as it is deposited from the liquid in which it forms, presents itself in small colourless crystals, doubtless isomorphous with thialdine. In contact with the atmosphere it turns immediately yellow. This substance has a feeble but disagreeable odour; it is slightly soluble in water, on which account it should not be washed too long. The aqueous solution and the last washings become soon turbid in contact with the atmosphere, and deposit an orange-yellow body. The same deportment is exhibited by the solution of selenaldine in alcohol and æther, in both of which it is easily soluble. In consequence of its being so easily altered, we did not succeed in obtaining this base in crystals from any of the solutions mentioned. On evaporating them *in vacuo* over

sulphuric acid, the greater portion of the substance is volatilized with decomposition, the yellow body being likewise produced, while sulphate of ammonia is formed in the sulphuric acid. Also, when heated alone, selenaldine is decomposed, with disengagement of a gas of most offensive odour. Selenaldine is a base like thialdine; it is soluble in dilute hydrochloric acid, and reprecipitated by ammonia as a crystalline mass. The hydrochloric solution also immediately decomposes with evolution of a most offensive odour. Selenaldine is altered in a similar manner by boiling with water. The yellow body, the formation of which seems to be always attended with a liberation of aldehydite of ammonia, when collected is orange-yellow, amorphous, soluble in alcohol and æther, and fuses, when heated with water, to a reddish yellow mass, which remains soft for a long time. When heated alone, this body is charred with evolution of an oil containing selenium and possessing a most offensive odour.

We did not obtain a decisive result when trying to produce a *telluraldine*, the tellurium which we employed in the preparation of the hydrotelluric acid accidentally containing so large an amount of selenium, that merely selenaldine was deposited from the purple solution, containing, as it appeared, only telluride of ammonium. We intend to repeat this experiment with tellurium free from selenium.

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LXXV. *Some Remarks on the Air and Water of Towns.*  
By ROBERT ANGUS SMITH, Ph.D.\*

HAVING given considerable attention to the inquiry into the causes affecting the health of towns, I was anxious to find what the real evil in their polluted atmosphere consisted of; the air has been frequently examined, but the differences found do not sufficiently account for the differences perceptible in breathing for the first time in entering a large town from the country, or for the very great difference in the colour and appearance of both when contrasted side by side, by an individual outside a town, having both before his eyes. The accumulation is great in this case, it is true, but therefore so much the more fitted for showing us its true character.

I first examined the rain-water taken from a cistern; a little was boiled down and the solid residue was like fatty matter, which burnt and gave the smell of a fat, leaving afterwards a strong odour of nitrogenized organic matter. As this amounted to nearly one per cent., I was disposed to con-

\* Communicated by the Chemical Society; having been read January 4, 1847.



sider it an accidental impurity, and collected some rain in a porcelain vessel previously carefully cleaned. A smaller amount was obtained, but still from 500 grains of the water a sufficient quantity to make the smell distinct. I know that organic matter has been found in rain-water, but I am not aware that the smell of nitrogenized matter has been distinctly observed. It has been obtained also by boiling down large quantities: I have never found it to fail in evaporating as little as 1000 grs. The amount was generally about  $\cdot 01$  of a grain in 1000 grs., but the average cannot be got without trying many seasons and many places.

1000 grs. collected on the 23rd of November, 1846, in a platinum vessel, after raining thirty hours with little intermission, gave with nitrate of silver a precipitate weighing 0.11 grs. = 0.027 of chlorine, and with a salt of barytes 0.1, equal to 0.0343 grs. of sulphuric acid, besides a perceptible smell of organic matter as before.

I do not adduce this analysis as of any great importance in showing the amount; the quantity operated upon is small, and I was desirous, for the sake of the salts, not to expose it to the contact of any vessel which could be acted on; but I mention it to show, that the quantity is not such as can be only procured by the questionable process of boiling down large quantities in vessels too large to admit of being perfectly cleaned.

The water is often alkaline—I say often, but I have always found it so; by boiling this is lost, proving, as I think, that there is more carbonate of ammonia set free from the burning of coals than is necessary to saturate the sulphuric acid in the atmosphere.

The air is often acid, as evidenced by test-papers exposed to it; and it certainly proves very acid when certain quantities of coal are burnt.

It would have been well to have examined water from the country falling at the same time, but being persuaded that water so impure would long ago have been examined if it even fell over large districts in such a state, I preferred looking to its origin.

As far as I am aware no examination of unventilated places has given us a good proof that carbonic acid is the greatest agent in making them so actively injurious. I breathed, and had others to breathe, through a bent tube into a large jar, and examined the moisture in the jar. As it always contained organic matter in large quantities, I inferred that the same must occur when the perspiration and breath condense upon the windows of crowded rooms.

This condensed moisture is not easily obtained in sufficient quantities ; but on the occasion of a concert at the Mechanics' Institution here, I collected 200 grs. from one window, letting it drop into a small platinum basin. By burning 5 grs. of this the smell was made apparent ; by burning 150 grs. a strong smell of human sweat came off, continuing until the substance was dry. It will therefore be difficult to tell how much organic matter was contained in the whole, as a great deal evaporated, but above 2 grs. were left. When heated, this smelt like burning flesh, and was very disagreeable.

We know that volatile products escape from the bodies of animals, and are mixed with the air so as to undergo complete oxidation ; but this cannot be any important part which the air has to perform, as the following facts show that the oxidation is best effected in the earth. It may however be important that bodies should decompose in this manner, because they are also more readily conveyed by the atmosphere to plants.

When we see the volatile and decomposing matter from the body so distinctly as in the last case mentioned, it is possible for us to conceive that infectious matter is not a substance so very subtle as has been supposed ; we have only to allow it to be in a peculiar state of decomposition.

Burning in a platinum crucible and the odour discover a very small quantity of organic matter, such as it is scarcely safe to trust to the results of the balance. I hope in a future paper to show the qualities of several water districts in this respect, but at present give an abstract only of the results of such observations as I have made, hoping that the constant and almost monthly variation of the water of whole districts will be a sufficient excuse for the absence of an arranged set of results.

When water from a peaty district is boiled down and the ashes burnt, the smell of peat is distinctly observed. The river Dee at Chester contains 25 grs. of organic matter to the gallon, and the smell is as perceptible as at Llangollen, close to a peaty district. 500 grs. of the water are sufficient to indicate this very strongly. Water from the Lancashire and Yorkshire hills may be known to contain peat also by testing the same quantity, although many of the streams do not contain 15 grs. per gallon. Some of the canals near Manchester may be known to be supplied from peaty districts by the same tests. But whenever we approach a town the smell from the burnt ash changes, and organic matter from the decomposition of protein compounds is distinctly traced. So easily is this known, that when water was brought to me

from the Dee, above Llangollen and below, it appeared evident that even this small town had affected the river. Before the water arrives at Chester the peat predominates too much to make it perceptible that matter from towns has been thrown into it; below it is distinct. Water from surface-drainage indicates by the same mode of treatment nitrogenous matter undecomposed.

Observing this increase of organic matter on approaching towns, I naturally expected to find a large quantity in the wells, of which there are many in Manchester, placed as they often are in the neighbourhood of cesspools, besides being exposed to the infiltration of the water of drains.

A well was first examined in a garden near the town, the proprietor having complained of it as being unwholesome. It contained 40 grs. of common salt per gallon, 14 grs. of organic matter and salts of ammonia, besides other ingredients to be expected in connection with organic matter. I next examined a well near a burying-ground; it was loaded with inorganic salts. Its taste was very bitter; when evaporated and burnt, the remaining inorganic matter gave out a pungent acid vapour. Four wells in the same neighbourhood were examined, all with the same results. I then went to nearly all the churchyards in Manchester, of which there is a considerable number, but finding that the neighbourhood of the collegiate church gave the same indications as all the rest, although there have been no burials there for many years, I was induced to ascribe it to the presence of large dung-heaps close to the well. Other wells distant from churchyards, but near cesspools, gave the same result. In the suburbs they were not so much tainted, but in some newly-built streets close to the town, in gravelly soil, were very bad.

I have no doubt that the churchyards and the cesspools act in a similar manner, as we might naturally expect. The chief impurity is a collection of inorganic salts. It is remarkable how rapidly the organic matter becomes oxidized and converted into nitric acid in the soil. In one case I found as much as 70 grs. of nitrates in a gallon of water, besides magnesia, lime and alkalies. This water seems to be a favourite with some persons; when not quite so heavily charged it has a fine sparkling appearance. The wells in the centre of the town have nearly disappeared, otherwise it is probable that organic matter would have been found there in a less oxidized state.

As these wells have undergone several changes during the autumn and early part of the winter, I shall not give the results of the analyses which I have made, until a few seasons shall enable me to make a more complete examination of the

subject. At present, when the ground is so wet, it is difficult to obtain a sufficient quantity of the salts, which are peculiar. At present I may say that sulphuric acid added to the water gives off chlorine; that heat evolves chlorine and peroxide of nitrogen from it; and that the remaining salt is a mixture of a chloride and a nitrate chiefly. This will be sufficient to show that neither the water nor the air of Manchester has so very minute an amount of impurity as to put it without the power of detection.

LXXVI. *On certain Atmospheric or Barometric Waves which traversed Europe during November 1842.* By WILLIAM RADCLIFF BIRT\*.

AT the sittings of the Geological Society on the 3rd and 17th of May in the year 1816, a paper was read by Dr. Macculloch On the Employment of the Barometer in measuring heights. In this paper the author endeavoured to show that the *differences* indicated by different instruments, placed at various stations on the earth's surface, are *real* differences of atmospheric pressure, and that the changes which take place at several stations are far from being simultaneous: he also expressed an opinion, that the differences of pressure, combined with their progressive character, are in some way or another connected with the direction of the wind, especially the two cardinal currents N.E. and S.W. of our island; but the imperfect state of the barometer at that time was such as to prevent an efficient investigation of the subject.

In the year 1832, Professor Forbes, in his Report on the Recent Progress and Present State of Meteorology, presented to the British Association for the Advancement of Science during its sitting at Oxford, alluded to the *accidental* variations of barometric pressure as greatly influenced by latitude, the amount of variability increasing towards the poles. He appeared to regard these variations as resulting from *great atmospheric tidal waves*, which perpetually traverse our oceans and continents, and he viewed a future and more advanced state of meteorological science, as furnishing data from which the most interesting and important conclusions might be drawn in immediate connexion with such atmospheric waves†.

On the 21st of March in the year 1835, Sir John Herschel made a series of hourly meteorological observations at Feld-

\* From Report of the British Association for the Advancement of Science for 1846.

† Report of the First and Second Meetings of the British Association for the Advancement of Science, p. 235.



hausen Wynberg, near the Cape of Good Hope. These were accompanied by a simultaneous set at the Royal Observatory near Cape Town. The observations thus set on foot were continued at numerous stations during the following three years; and the discussion of the whole, collected by Sir John, forms the subject of a report presented by that gentleman to the British Association during its sitting at Cork in the year 1843. One of the objects to which Sir John directed his attention in this discussion, was the barometric fluctuations considered with a view to the propagation of atmospheric waves. For this purpose the stations were distributed in groups, in one of which (the European) Sir John succeeded in detecting and tracing two well-defined waves; one on the 21st of September 1836, the other on the 21st of December 1837. These waves were so vast in their dimensions, that to take in (says Sir John) an effect of this nature, we must enlarge our conception of an atmospheric wave till it approaches in some degree, in the extent of its sweep and the majestic regularity of its progress, to those of the tide-waves of the ocean\*.

The principle on which the discussion of these hourly observations was conducted, was that of projecting the barometric curves for all the stations of one group on a single sheet for each separate term, the scale being one inch to an hour in *time*, and one inch to 0<sup>in</sup>·066 of barometric altitude. From the nature, however, of the observations, no one curve was continued longer than twenty-four hours, except on the occasion of a remarkable storm. During the progress of this work, it was deemed desirable to obtain the complete rise and fall of the barometer; so that a complete wave might be observed at least at one station. Observations undertaken with this view, and projected in curves, illustrate Sir John Herschel's report. As a test of any conclusions that may be drawn from a discussion of observations on this principle, the observations may be so combined as to exhibit, not only the barometric relations with regard to *time*, but also with regard to space: in other words, the distribution of pressure over a tract of country, at any given epoch, will indicate the real character of the waves then transiting.

The last report which I had the honour to present to the British Association during its sitting at Southampton, contains an examination of certain barometric observations exclusively on the last-named principle. These observations were made during November 1842, the same month in which the observations were made from which some of the curves illustrating

\* Report of the Thirteenth Meeting of the British Association for the Advancement of Science, pp. 60-103.

Sir John Herschel's report were projected. The result of the examination has been the apprehension of several large atmospheric waves of so distinct a character, that the barometric movements over an area included by the angular points, the Orkneys, St. Petersburg, Geneva and Cork, have been fully explained, especially during the earlier portion of the month. In order clearly to place these barometric waves before the readers of the *Philosophical Magazine*, I shall at once proceed to make various extracts from the report bearing immediately upon the subject, commencing with the

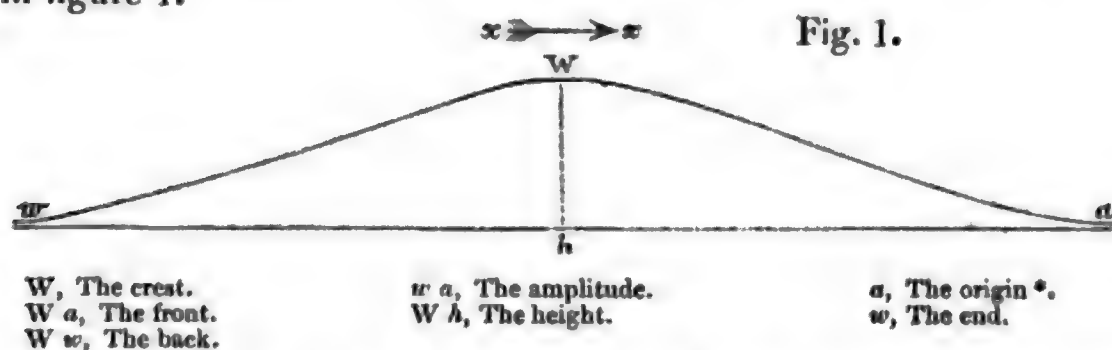
*Definition and Phenomena of an Atmospheric Wave.*

When a number of barometric observations are projected on paper according to a suitable scale, and continued for months and years, the eye on contemplating them will recognise a variety of curved forms, some of large and some of small amplitude; some rising to a considerable altitude, others sinking far below the level, representing the mean barometric pressure at the station of observation. At first there appears but little regularity in these curvilinear records of the ever-shifting state of our atmosphere, but here and there the attentive observer will notice some similarity existing between two or more *individual* curves, and he may notice some which possess a certain symmetrical arrangement of the ascents and descents. In consequence of this similarity and symmetrical arrangement, he examines more carefully the records of barometric pressure, and not only discusses the observations at *one* station, but compares those observations with others made at *various* stations; and here again he finds apparent irregularity and confusion. The curves to a certain extent agree, but in many minor points they differ often very considerably, in some cases rising at one station while falling at another; this induces a still more minute and careful investigation: the distribution of pressure over the largest area he can command is carefully examined; and whether his stations are few or many at any given time, he finds on this area a point of maximum pressure and a point of minimum pressure; between these points he finds various pressures, generally increasing from the point of least pressure to the point of greatest pressure. On some occasions he finds a *line of high* pressure, stretching quite across the area, and on others a *line of low* pressure. By continuing his inquiries for successive epochs, he finds these lines of high and low pressure move across the area, or in other words, the high pressure or low pressure is gradually transferred from one point to another. He also finds at still more remote epochs other lines of high and low pressure, some having the same direction with the lines originally noticed, and others crossing the direction of the original lines at various angles.

The questions which now suggest themselves are the following:—What are these movements? How can they be represented? In what manner can they be explained? A simple consideration of the curves suggests the idea of waves as explanatory of the phænomena.

mena, and the term atmospheric wave has been used to designate that *ideal individuality* which the mind attributes to the process which it observes of the successive change of place which the barometric maxima and minima undergo, and by which they regularly succeed each other over the area under examination; this ideal individuality has been employed as a mean of examining the movements just alluded to.

Mr. Scott Russell, in his admirable report on waves, presented to the British Association during its sitting at York in 1844, has given the elements of an aqueous wave. These elements appear so clearly to express the relations of the barometric phenomena (simply considered as such), that I shall avail myself of Mr. Russell's terms to indicate the distribution of pressure over the surface of the earth, as in figure 1.



In reference to this figure, the line of high pressure stretching across the area (the figure being supposed to cut this line transversely) has been termed the *crest*, W; the line of low pressure in advance of the crest, the *anterior trough*, a (the origin of Mr. Scott Russell's water wave); the line of low pressure succeeding the crest, the *posterior trough*, w (the end of Mr. Scott Russell's water wave); the line w...h, as measured by the mercurial column, the altitude of the wave; the slope W a, the anterior slope or front of the wave; the slope W w, the posterior slope or back of the wave; w a constitutes the amplitude of the wave, and x — x in the same direction, the axis of translation.

The observations to which allusion has been made, were published by Mr. William Brown in a paper that appeared in the *Philosophical Magazine* for April 1846. This paper is entitled "On the Oscillations of the Barometer, with particular reference to the Meteorological Phænomena of *November 1842*." The object of the author is to show that the barometric oscillations are produced by the *meeting* of opposite or nearly opposite aerial currents; that one current thus meeting or impinging on another, deflects it, and under some circumstances produces a rise of the mercurial column, but under others occasions a fall in many cases of considerable magnitude. In order to elucidate his views, Mr. Brown has collected barometric observations from eleven stations, which are scattered over an area included by the following angular points:—The Orkneys, Christiania in Norway, Paris, Plymouth and Cork. These observations are in most cases given as read

\* Mr. Scott Russell designates the point a the *origin*; a better term I apprehend would be *commencement*.

off from the scale. In addition to these the paper is accompanied by six plates, in which the direction of the wind at numerous stations is indicated for every day during twenty-six days in the month by arrows. The anemonal observations published in the body of the paper not being in all cases for consecutive days, a comparison of them with the plates is rendered difficult; nevertheless the plates form a very valuable portion of the communication, and if they have been laid down from accurate observations, they furnish us with an important addition to our knowledge of the *arrangement* of the aerial currents, especially with respect to the distribution of pressure. It is a matter of regret that Mr. Brown did not so arrange his observations and plates, that the accuracy of the latter could have been seen by inspection.

This paper is peculiarly interesting at the present time, when the attention of meteorologists is directed to the important and interesting problem of the barometric oscillations. I have already noticed the views which have been taken of these oscillations as the results of waves. In this paper they are regarded as the effects of currents; and it appears that meteorologists are now divided as to which of these hypotheses they shall be referred. Professor Dove remarks, in his letter to Colonel Sabine on the *Magnetical and Meteorological observations*\*, "the so-called irregular variations of the barometer are regarded by some persons as only the effects of currents of air of unequal temperature and moisture; other persons distinguish the effects of currents from the effect of undulations *progressing in the manner of waves of sound, and propagating themselves with great velocity over large portions of the earth's surface.*"

It is worthy of notice, that these movements have been referred to two very distinct kinds of waves. Prof. Forbes speaks of them as great atmospheric *tidal* waves, and Prof. Dove speaks of a class of individuals who regard them as progressing in the manner of *waves of sound*.

It is not my intention to enter into an examination of the conclusions and results which Mr. Brown has arrived at; as the question is open, I apprehend I have not committed an injustice towards that gentleman by having employed a rather different process to that which he has used, and further discussed the observations he has given. I beg to acknowledge the obligations I am under to him for these observations, and especially for the plates, of which I have before spoken: they are extremely interesting in the present inquiry.

In accordance with these remarks, I have selected the following stations from Mr. Brown's list:—the Orkneys, Belfast, Shields, Cork, Bristol, Plymouth, London, Paris, and Christiania. The reason I have omitted Glasgow and Armagh will be apparent from Mr. Brown's notes. As I have discussed these observations with especial reference to the wave hypothesis, I have most cautiously avoided in my remarks any thing that may at all bear on Mr. Brown's views. The plan I have proceeded on is as follows. I have selected the

\* Report of the Fifteenth Meeting of the British Association for the Advancement of Science, p. 30.

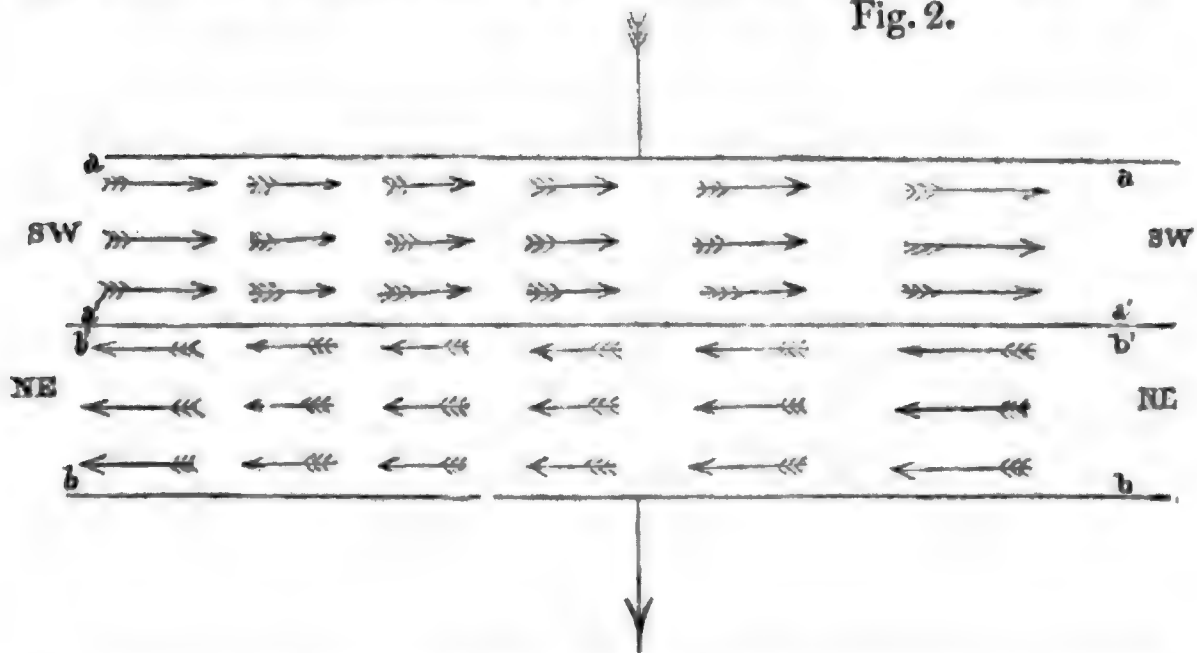


middle observation of each day; at those stations where only two are given morning and evening; I have taken a mean of them. These observations I have so arranged as to exhibit the distribution of pressure over the area for each day—the line or lines of the greatest diminution of pressure—and the relation of such distribution and of such lines to the aerial currents or winds. As a convenient method of readily expressing these various relations and giving to the discussion that completeness which otherwise it would want, I have adopted the wave hypothesis, and to every line of barometric maxima applied the term *crest* and to every line of minima the term *trough*. In a word, I have regarded the progress of the barometric and anemonal phenomena as the progress of *waves*. The observations remain the same both in Mr. Brown's and my own discussions, the results only are different; and it remains for other philosophers, by more closely investigating the subject, and submitting the observations to a more rigorous and searching discussion, to advance this interesting inquiry and to become more intimately acquainted with the causes of these interesting phenomena.

In addition to the discussion of these observations making us acquainted with several large waves similar in character to the definition before given, it also reveals to us the arrangement of the aerial currents with respect to such waves, or rather by means of these observations and the plates accompanying them, we are able to ascertain, not only the general direction of the aerial currents, but also the barometric phenomena resulting from their transit.

Prof. Dove, in his second letter to Col. Sabine relative to the magnetical and meteorological observations, has announced his opinion that the equipoise of the atmosphere is maintained in the temperate zone by currents *on the same level flowing in opposite directions*\*; thus we have a bed or stratum of air moving from the S.W., and on each side of this are strata of N.E. winds. We

Fig. 2.



\* Report of the Fifteenth Meeting of the British Association for the Advancement of Science, p. 61.

may here inquire, how are these alternate aërial currents related to the waves before alluded to? It is one of the objects of the discussion to exhibit this relation, which may be thus briefly expressed, at least in so far as the examination of the observations has yet extended\*. Let the strata  $a a a' a'$ ,  $b' b' b b$ , fig. 2, represent two parallel aërial currents,  $a a a' a'$  being from S.W. and  $b' b' b b$  from N.E., and conceive them both to advance from the N.W. in the direction of the large arrow, that is the strata themselves will advance with a *lateral* motion. Now conceive the barometer to commence rising just as the edge  $b b$  passes any line of country, and to continue rising until the edge  $b' b'$  arrives at that line, when the maximum is attained. The wind now changes and the barometer immediately begins to fall, and continues to fall until the edge  $a a$  coincides with the line of country on which  $b b$  first impinged. During this process we have all the phænomena exhibited by an atmospheric wave; when the edge  $b b$ , fig. 2, passes the line of country, the point  $a$ , fig. 1, of the wave (the anterior trough) transits that line of country and the barometer begins to rise with a N.E. wind. During the period the stratum  $b' b' b b$ , fig. 2, transits the line the anterior slope  $W a$ , fig. 1, passes; when the conterminous edges of the strata  $a' a'$   $b' b'$ , fig. 2, pass, the crest  $W$ , fig. 1, extends in the direction of the preceding trough: the barometer now begins to fall, and when the edge  $a a$ , fig. 2, occupies the place of  $b b$ , also fig. 2, the descent of the mercurial column is completed; the posterior slope  $W w$ , fig. 1, has passed, and the posterior trough  $w$ , fig. 1, now occupies the line in which the anterior trough extended.

This arrangement of the aërial currents in connexion with a barometric wave may be termed the *air-motion* of the wave. In contemplating the transference of the barometric maxima and minima, we regard only the *wave-motion*. In contemplating the direction and force of the wind, we regard also the *air-motion*. The *wave-motion* consists in the *lateral* transference of the parallel beds of aërial currents; the *air-motion* being these oppositely directed parallel currents themselves.

From these considerations, we readily see that the *wave* is a convenient method of representing the barometric fluctuations. We have seen that the rise is due to the anterior slope and the fall to the posterior; and we now further learn that the direction of the aërial current on the anterior slope is at right angles to the axis of translation directed towards the *left-hand*, while on the posterior slope it is the reverse; still at right angles to the axis of translation, but directed towards the *right-hand*.

Having thus noticed the *wave-motion* with its accompanying *air-motion*, these interesting questions suggest themselves:—How are the forces of this *air-motion* arranged? Do all the particles move with the same velocity? Are there different velocities in different parts of the wave? Our anemometers will answer these questions. In the troughs, the edges  $b b a a$ , the forces are strongest; as the barometer rises, the force gradually subsides; when the crest passes,

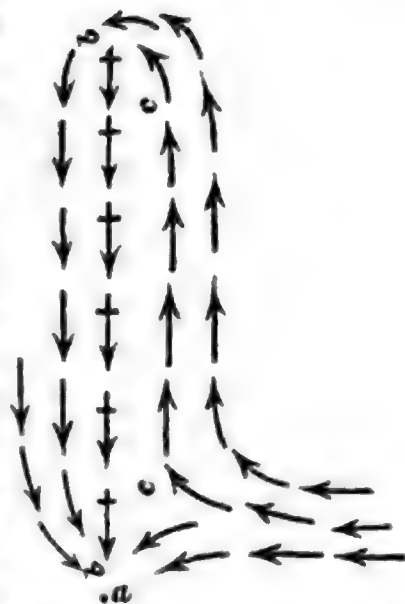
\* For this knowledge I am indebted to Mr. Brown's plates.

it is zero; and as the barometer falls, it increases until the trough passes, when it is again strongest.

M. Dove has suggested that the parallel currents, by which the equipoise is maintained in the temperate zones, may be shifting ones, and we have supposed that the parallel currents of N.E. and S.W. winds may advance from the N.W. with a lateral motion. The same cause that produces the well-known opposite and superposed equatorial and polar currents, will also give rise to the same opposite *but* parallel currents in the temperate zones, namely, the ascending column of heated and consequently rarefied air. Now it is well known that in stormy weather, when the wind is blowing with great force, the barometer being nearly at its minimum, upon the *wind changing* the barometer *commences rising*; the wind however continues to blow with about the same force as it did with the previous falling barometer. Upon M. Dove's view of parallel and opposite currents, somewhere in or near the line forming the boundary between the currents, towards or in the torrid zone, we ought to find the point of rarefaction, and to this point the N.E.

current would rush with the greatest force to supply the ascending column of heated air\*. This N.E. current would be compensated by a S.W. current of nearly or quite the same force, situated just to the S.E. of it, as in fig. 3, in which let a point of rarefaction, *a* for instance, exist in any locality, so that a N.E. current may be established to supply the ascending column; suppose the greatest force to exist along the line of crossed arrows *b b*, the air would be drawn from the end of this line to fill up the vacuum at *a*, and a compensating S.W. current, *c c*, established. This S.W. current would be established partly by the descent of the overflowing current at *a*, and partly by the rush to supply the air

Fig. 3.



\* In attributing the greatest force to the N.E. current, I do not by any means wish to put forward or support any hypothesis that would at all interfere with the well-known fact, that the greatest force is usually manifested by S.W. winds. The point to which I wish more particularly to solicit the attention of the Association is this, *the cause which induces the south-westerly current itself*. This must reside in or near the torrid zone. Here we have a sufficient cause; we are presented with phenomena fully adequate to explain an influx of cool air from the N.E. This is the current that must first be established, and in the first instance its force will be *greatest*. We have however only to turn to Prof. Dove's letter to Col. Sabine (Report of the British Association, 1845, p. 61), and we shall at once find the reason why S.W. winds manifest by means of our instruments the greatest force. The N.E. currents are *narrower*, and the force *soon abates* as they pass over towards the S.E.; while on the other hand the same station is not only *oftener*, but *longer* in the S.W. currents, and as the line of greatest force approaches, the force increases, on some occasions very rapidly, until the wind changes. The line of greatest force soon passes the station, so that upon a mean of numerous observations the south-westerly wind exhibits the *greatest* force.

constantly drawn off to feed the ascending column. When however it is once established, the velocity of the line of S.W. current nearest to the N.E. would probably be equal, or nearly so, to that of the N.E. current itself.

In this way it is easy to conceive that a complete barometric wave may be produced; the lines of greatest velocity of the parallel currents will indicate the trough; the rapidity with which the currents pass in opposite directions greatly diminishes the pressure, and according to this view somewhere near the direction of the trough and to the S.W. of it, we ought to find the point of greatest rarefaction: the velocity decreases on each side this trough, and with this decrease of velocity the pressure increases, so that we have a distribution of pressure of a *wave form* gradually rising on each side the trough, the *pressure* being dependent on the velocity of the parallel currents.

The constant ascent of air at the point of rarefaction would continually draw off a quantity of air from the S.E. side of the line of greatest velocity  $b-b$ , fig. 3, and this would be attended with two results; first, there would be a *real* hollow or trough formed in the line of junction of the parallel currents; and secondly, this line would gradually advance towards the S.E.; for as more air would be drawn off from that side, the whole body of air would advance in that direction to supply the deficiency; and should the rarefying process cease, we can readily conceive that not only will the *wave-form* be continued, but also *wave-motion*. The establishment of the parallel currents will give the *air-motion*; the diminution of pressure towards the lines of greatest velocity will give the *wave-form*; and the drawing-off of air from the S.E. will induce the *wave-motion*. The wave thus generated is *negative*; it consists of a hollow produced by the ascending current of heated air carrying off a considerable portion of air set in motion by this ascending column, and its direction of motion is determined by more air being drawn off from the S.E. slope than the N.W.

#### *Cross Waves.*

The examination of the transit of a single wave by means of barometric and anemonal observations, would be comparatively easy, but it seldom happens, from the operation of natural causes, that an isolated or solitary wave is produced. In almost every instance (except in those in which the generating power is very much greater than any which occasions the production of smaller waves) the wave is contemporaneous with others of equal, if not of greater magnitude, so that different systems are in motion at the same time, each individual pursuing its own course, and although perfectly independent of every other, yet greatly modifying the *resulting phenomena* as exhibited by the barometer and anemometer. When therefore we proceed with the examination of certain barometric and anemonal phenomena in the manner above alluded to, we are speedily perplexed with the barometric and anemonal effects



of *cross waves*; the flowing of one set of waves in a certain direction is apparently interrupted and interfered with by another in a different direction, and before the first set can be exhibited with its proper proportions, and the true altitudes, amplitudes, velocities, and directions of its individual waves assigned, all the phenomena of the other set must be carefully disentangled and separated from the aggregate phenomena presented by the contemporaneous systems. The barometric curve, including a complete rise and fall at any one station, is *not* the curve resulting from the transit of any *one* wave; it does *not* represent the *form* of any *reality* in nature; but it *does* represent, and is an exponent of the effects resulting from the contemporaneous transits of waves, or systems of waves, such as have been described.

The discussion of Mr. Brown's observations has clearly brought to light a set of parallel and opposite currents at right angles to those we have been contemplating, namely, from N.W. and S.E. with a *wave-motion* towards the N.E., producing the cross waves which occasion the complexity before alluded to. The late Professor Daniell has remarked that the curves increase in range towards the N.W., and in general the neighbourhood of *water* presents curves remarkable for the boldness of their contour and the large extent of their range. In venturing a speculation on these cross waves from the S.W. with parallel and opposite currents from N.W. and S.E., I should be inclined to attribute them to the effect of the solar influence on the terrestrial surface, extending from Cape Verd in Africa to the extreme north of Lapland in Europe. This surface extends from S.W. to N.E., or somewhat in that direction. It may be remarked, that to the *north-east* of Cape Verd is situated the Sahara or Great Desert of Africa, and here we have a great rarefying surface. To the north-west or west-north-west of this extensive rarefying surface, the broadest part of the Atlantic ocean is situated. The relative positions of the Great Desert and the broadest extent of the Atlantic will produce a great indraught of cool air from the ocean; the direction of this wind will be W.N.W. or N.W. To the north-east of this current, probably in the neighbourhood of Morocco, Fez, Algiers, Spain and Portugal, and the north-west portions of the Mediterranean sea, we ought to find the counter current from the S.E. or E.S.E., the two portions in juxtaposition moving with the greatest velocity. Somewhere in the Atlantic the turning-point of these oppositely directed currents should exist. The line of junction of these parallel currents will determine the trough of the wave, and as before shown, in consequence of the air being drawn off from the north-east to supply the ascending current, the wave will progress towards that quarter; the barometer first descending with the S.E. wind as the trough approaches stations to the N.E., and rising with the N.W. as the current produced by the rarefaction approaches, until the crest passes, when the new counter current or slope of the next wave would set in \*.

\* In the above suggestion I have considered the northern portion of the African

Pursuing this idea further, there can be no question that Ireland and Scotland become points, or unitedly constitute a great point of rarefaction, forming as they do the nearest land to the northern part of the Atlantic, the land becoming hotter than the neighbouring water, and in consequence a N.W. current with its compensating current from the S.E. is induced. Not only will the rapidity of the currents reduce the pressure, but the ascending column from the land will transfer some of the air into the general current of the atmosphere, and there will be a real difference in the distribution of air as well as pressure; a section transverse to the line of greatest velocity will exhibit a hollow or trough, and the same phenomena will result from this arrangement of the aerial currents as we noticed arising from the N.E. and S.W. currents, the only difference being in direction.

In thus considering these rectangularly posited systems of parallel and opposite currents, many complex anemonal and barometric phenomena receive an easy explanation, particularly the revolution of the vane in one uniform direction, and the barometric wind-rose. When the conterminous edges of any two currents pass a station, the barometer is either at a maximum or minimum with respect to that particular system of currents; the wind also changes at this time. If the barometer has previously been rising with a north-easterly wind, it now begins to fall with a south-westerly: the cross currents are however passing at this time with a lateral motion towards the N.E.; in this set of cross currents the barometer will rise with a north-westerly wind and fall with a south-easterly. Suppose while the posterior slope of a N.W. wave transits, wind S.W., and before its trough passes, the trough of the cross wave from the S.W. also transits, and is immediately succeeded by the following anterior slope with its N.W. current, the wind will pass from S.W. to W. Now while this slope continues, upon the trough of the N.W. system passing, the wind changes to N.E., and the resultant of the two currents is N. It is easy to pursue this reasoning, and thus trace the changes of the wind arising from these two cross systems completely round the compass.

The two systems of cross currents naturally divide themselves into four beds of opposite currents, namely, N.E. S.W., N.W. S.E.; with the first of each system, N.E. N.W., the barometer rises, and with the last of each, S.W. S.E., it falls, so that in the barometric wind-rose the maximum is found about the N.E., the prevailing system, and the minimum near the S.W., the opposite current of this system.

The extent of arc which the wind-vane frequently describes, especially in stormy weather, also receives an explanation from these systems of cross currents. A contemporaneous S.W. with a N.W. wind will occasion large arcs to be described between these points;

continent as inducing the N.W. current, but of course, the entire surface, as far as the extreme north of Europe, including Great Britain and Ireland, will act as a rarefying surface.

the south-westerly gusts prevailing, directing the vane to that quarter; and the north-westerly immediately following, instantly occasions a change carrying the vane towards the N.W. These sudden and extensive changes are rendered more distinctly perceptible by means of a small kite flown with about 250 or 300 feet of string, or even more; the distinctness and independence of the direction of the two currents are readily seen, as well as the difference in their strength.

*Discussion of Mr. Brown's Observations.*

In the discussion to which I have alluded, I have first arranged such of the observations collected by Mr. Brown, or deductions from them, as indicate the barometric pressure about the middle of each day at the stations before-named, as near as the data furnished by that gentleman will allow. These observations or deductions will be found in Table I. The arrangement is such that the eye may readily ascertain the barometric state of the atmosphere at any station on any day embraced by the area and period included in the table. The changes at any one station are also readily seen, the altitudes above 30 inches being distinguished from those between 29 and 30, and those below 29 also being distinguished from the rest. This table forms the basis of the conclusions which have been thus arrived at. The values corresponding to each day have been arranged with especial reference to the maximum and minimum of that day *in space*, that is, the station exhibiting the greatest pressure on any particular day has generally been placed first on the list for that day; and that exhibiting the least, last. At the head of each list are placed the directions of the crests as indicated by the observations. Crests passing from N.W. to S.E. are distinguished by the odd numbers, and those passing from S.W. to N.E. by the even. When the observations give two slopes from a crest or trough passing between such slopes, the observations have been arranged to exhibit this. After the arrangement of the observations, the lines of the greatest diminution of pressure corresponding in a majority of cases to tranverse sections of the waves, and exhibiting either their anterior or posterior slopes, are inserted. These are succeeded by the direction of the wind on each side of the crests as given in Mr. Brown's plates, and the discussion of each day's observation is concluded by a few explanatory notes\*.

\* For the complete discussion of Mr. Brown's observations, the reader is referred to the author's third report on atmospheric waves in the Report of the Sixteenth Meeting of the British Association for the Advancement of Science, held at Southampton in 1846, pp. 141 to 162.

TABLE I.—Barometric Observations, November 1842.

Station.	In.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.
Orkneys . . .	30	<u>·16</u>	<u>·23</u>	<u>·24</u>	<u>·49</u>	<u>·52</u>	<u>·46</u>	<u>·15</u>	<u>·63</u>	<u>·80</u>	<u>·39</u>	<u>·24</u>	<u>·10</u>	<u>·35</u>
Belfast . . .	30	<u>·33</u>	<u>·18</u>	<u>·18</u>	<u>·45</u>	<u>·55</u>	<u>·51</u>	<u>·43</u>	<u>·04</u>	<u>·41</u>	<u>·57</u>	<u>·02</u>	<u>·21</u>	<u>·27</u>
Shields . . .	30	<u>·17</u>	<u>·19</u>	<u>·10</u>	<u>·34</u>	<u>·33</u>	<u>·35</u>	<u>·27</u>	<u>·97</u>	<u>·28</u>	<u>·58</u>	<u>·99</u>	<u>·07</u>	<u>·24</u>
Cork . . . .	30	<u>·15</u>	<u>·92</u>	<u>·83</u>	<u>·16</u>	<u>·32</u>	<u>·30</u>	<u>·33</u>	<u>·01</u>	<u>·42</u>	<u>·20</u>	<u>·91</u>	<u>·31</u>	<u>·40</u>
Bristol . . .	30	<u>·18</u>	<u>·05</u>	<u>·96</u>	<u>·14</u>	<u>·20</u>		<u>·18</u>	<u>·07</u>	<u>·60</u>	<u>·46</u>	<u>·03</u>	<u>·31</u>	
Plymouth . .	30	<u>·21</u>	<u>·04</u>	<u>·91</u>	<u>·15</u>	<u>·22</u>	<u>·24</u>	<u>·24</u>	<u>·13</u>	<u>·72</u>	<u>·48</u>	<u>·12</u>	<u>·46</u>	<u>·46</u>
London . . .	30	<u>·17</u>	<u>·10</u>	<u>·96</u>	<u>·13</u>	<u>·12</u>	<u>·16</u>	<u>·13</u>	<u>·08</u>	<u>·70</u>	<u>·64</u>	<u>·00</u>	<u>·33</u>	<u>·26</u>
Paris . . . .	30	<u>·04</u>	<u>·86</u>	<u>·73</u>	<u>·80</u>	<u>·75</u>	<u>·83</u>	<u>·89</u>	<u>·90</u>	<u>·76</u>	<u>·63</u>	<u>·25</u>	<u>·43</u>	<u>·53</u>
Geneva . . .	30	<u>·34</u>	<u>·09</u>	<u>·88</u>	<u>·82</u>	<u>·79</u>	<u>·86</u>	<u>·93</u>	<u>·01</u>	<u>·08</u>	<u>·06</u>	<u>·84</u>	<u>·84</u>	<u>·10</u>
Christiania . .	29	<u>·78</u>	<u>·11</u>	<u>·31</u>	<u>·37</u>	<u>·27</u>	<u>·21</u>	<u>·02</u>	<u>·67</u>	<u>·37</u>	<u>·24</u>	<u>·48</u>	<u>·20</u>	<u>·94</u>
St. Petersburg	29	<u>·45</u>	<u>·80</u>	<u>·21</u>	<u>·22</u>	<u>·25</u>	<u>·16</u>	<u>·96</u>	<u>·96</u>	<u>·92</u>	<u>·89</u>	<u>·66</u>	<u>·87</u>	<u>·62</u>
Station.	In.	14.	15.	16.	17.	18.	19.	20.	21.	22.	23.	24.	25.	26.
Orkneys . . .	29	<u>·76</u>	<u>·01</u>	<u>·22</u>	<u>·35</u>	<u>·18</u>	<u>·91</u>	<u>·96</u>	<u>·86</u>	<u>·43</u>	<u>·33</u>	<u>·10</u>	<u>·07</u>	<u>·10</u>
Belfast . . .	29	<u>·91</u>	<u>·82</u>	<u>·06</u>	<u>·51</u>	<u>·37</u>	<u>·86</u>	<u>·91</u>	<u>·95</u>	<u>·42</u>	<u>·32</u>	<u>·79</u>	<u>·82</u>	<u>·04</u>
Shields . . .	29	<u>·82</u>	<u>·83</u>	<u>·03</u>	<u>·45</u>	<u>·42</u>	<u>·77</u>	<u>·85</u>	<u>·89</u>	<u>·30</u>	<u>·27</u>	<u>·78</u>	<u>·82</u>	<u>·99</u>
Cork . . . .	29	<u>·60</u>	<u>·37</u>	<u>·70</u>	<u>·31</u>	<u>·18</u>	<u>·92</u>	<u>·80</u>	<u>·83</u>	<u>·58</u>	<u>·10</u>	<u>·54</u>	<u>·80</u>	<u>·04</u>
Bristol . . .	29	<u>·65</u>	<u>·61</u>	<u>·78</u>	<u>·36</u>	<u>·42</u>	<u>·98</u>		<u>·79</u>	<u>·39</u>	<u>·26</u>	<u>·79</u>	<u>·84</u>	<u>·14</u>
Plymouth . .	29	<u>·68</u>	<u>·64</u>	<u>·70</u>	<u>·36</u>	<u>·47</u>	<u>·14</u>	<u>·73</u>	<u>·79</u>	<u>·53</u>	<u>·30</u>	<u>·91</u>	<u>·93</u>	<u>·20</u>
London . . .	29	<u>·80</u>	<u>·62</u>	<u>·79</u>	<u>·36</u>	<u>·53</u>	<u>·06</u>	<u>·77</u>	<u>·82</u>	<u>·28</u>	<u>·59</u>	<u>·92</u>	<u>·88</u>	<u>·17</u>
Paris . . . .	29	<u>·67</u>	<u>·55</u>	<u>·50</u>	<u>·99</u>	<u>·38</u>	<u>·17</u>	<u>·55</u>	<u>·57</u>	<u>·13</u>	<u>·41</u>	<u>·02</u>	<u>·01</u>	<u>·17</u>
Geneva . . .	30	<u>·10</u>	<u>·08</u>	<u>·86</u>	<u>·89</u>	<u>·33</u>	<u>·53</u>	<u>·10</u>	<u>·71</u>	<u>·73</u>	<u>·86</u>	<u>·55</u>	<u>·36</u>	<u>·27</u>
Christiania . .	29	<u>·35</u>	<u>·70</u>	<u>·86</u>	<u>·94</u>	<u>·11</u>	<u>·91</u>	<u>·60</u>	<u>·54</u>	<u>·55</u>	<u>·62</u>	<u>·66</u>	<u>·62</u>	<u>·57</u>
St. Petersburg	29	<u>·53</u>	<u>·14</u>	<u>·45</u>	<u>·68</u>	<u>·85</u>	<u>·16</u>	<u>·87</u>	<u>·78</u>	<u>·42</u>	<u>·59</u>	<u>·03</u>	<u>·08</u>	<u>·11</u>

The numbers in the columns immediately succeeding the names of the stations indicate the initial inch of the barometric readings of the 1st and 14th of November, the succeeding numbers are decimals of an inch. Observations above 30 inches are not underlined. Those between 29 and 30 inches have a single line —, and those below 29 inches a double line =.



*Results of the Discussion of Mr. Brown's Observations.*

In collecting the results of this discussion, I have arranged in Tables II. and III. the principal lines of diminution of pressure; the succession of waves as well as the distinct systems become very apparent from these tables. The succeeding Tables IV. and V. exhibit the principal features of the respective waves of each system. The most prominent result appears to be the confirmation of Prof. Dove's suggestion of parallel and oppositely directed currents. The diagrams of the wind in connection with the barometric observations clearly exhibit such currents, and we see by a glance at Tables IV. and V. that the beds of these currents varied considerably in breadth. At the opening of the observations they were very much *broader* than at the close, and the N.W. system (waves No. 2, 4, 6) were altogether larger than the S.W. We have in fact two systems of waves or currents crossing each other at right angles, the individuals in both gradually decreasing in size. In the speculation which has been ventured relative to the S.W. system, the mass of terrestrial surface forming the N.W. boundary of the great eastern continent has been assumed as the rarefying surface, producing the set of parallel and oppositely directed S.E. and N.W. winds, the currents gradually shifting towards the N.E. The gradual contraction of the beds of each system as the observations proceed is a highly interesting feature, which requires a more extensive discussion for its elucidation.

TABLE II.—Exhibiting the principal lines of the greatest diminution of pressure of the N.W. system of waves, Nos. 2, 4, and 6.

Epochs.	Directions.	Values.	Slopes.
Nov. 1	Belfast to Paris .....	·29	Anterior, No. 2
2	Orkneys to Paris.....	·37	2
4	Orkneys to Paris.....	·69	2
5	Belfast to Paris .....	·80	2
6	Belfast to Paris .....	·68	2
7	Belfast to Paris .....	·54	2
9	Paris to Orkneys.....	·96	Posterior, No. 2
10	London to Cork .....	·44	2
11	Paris to Cork .....	·34	2
16	Orkneys to Paris.....	·72	Anterior, No. 4
17	Belfast to Paris .....	·52	4
18	London to Cork .....	·35	Posterior, No. 4
18	London to Orkneys.....	·35	4
20	Orkneys to Paris.....	·41	Anterior, No. 6
21	Belfast to Paris .....	·38	6
22	Cork to Paris .....	·45	6
24	Paris to Cork .....	·48	Posterior, No. 6
25	Paris to Cork .....	·21	6
26	Paris to Cork .....	·13	6

TABLE III.—Exhibiting the principal lines of the greatest diminution of pressure of the S.W. system of waves, Nos. 1, 3, 5, 7, 9, 11, 13, 15.

Epochs.	Directions.	Values.	Slopes.
Nov. 1	Belfast to Christiania ...	·55	Anterior, No. 1
3	Christiania to Paris.....	·58	Posterior, No. 1
11	Christiania to Cork .....	·57	3
12	Christiania to Shields...	·13	3
12	Plymouth to Shields ...	·39	Anterior, No. 5
13	Paris to Christiania.....	·59	5
14	Belfast to Christiania ...	·56	5
15	Orkneys to Cork .....	·64	Posterior, No. 5
17	Belfast to Christiania ...	·57	Anterior, No. 7
18	London to Christiania...	·42	7
21	Belfast to Christiania ...	·41	Anterior, No. 11
23	Christiania to Cork.....	·52	Posterior, No. 13
24	Christiania to Cork.....	1·12	13

TABLE IV.—Exhibiting the principal features of the waves of the N.W. system Nos. 2, 4, and 6.  
Wave No. 2.

Epochs.	Phases.	Directions and Localities.	Altitudes.	Winds.	
				Anterior Slope.	Posterior Slope.
Nov. 1	Crest.	N.W. of the United Kingdom .....			
2	Crest.	N.W. of the United Kingdom .....	...	N.E.	
3	Crest.	N.W. of the United Kingdom .....	...	N.E.	
4	Crest.	N.W. of the United Kingdom .....	30·49+	N.E.	
5	Crest.	From Cork to the Orkneys .....	30·55	N.E.	
6	Crest.	S.E. of Belfast .....	30·51+	N.E.	
7	Crest.	S.E. of the Orkneys .....	30·43+	N.E.	S.W.
8	Crest.	Passes Plymouth.....	30·13		
9	Crest.	S.E. of Paris .....	29·90+		S.W.
9	Post. Trough.	Passes the Orkneys.....	28·80		S.W.
10	Post. Trough.	Near the eastern coast of Ireland extending to Christiania.....	29·24		S.W.
11	Crest.	Considerably S.E. of Paris.....			
11	Post. Trough.	Passes Plymouth.....	29·12		S.W.
Wave No. 4.					
Nov. 17	Crest.	Passes Belfast.....	30·51	N.E. E.	
18	Crest.	Passes London .....	30·53	N.E.	S.W.
19	Crest.	Considerably S.E. of Paris.....			
Wave No. 6.					
Nov. 21	Crest.	N.W. of Belfast and Cork .....	.....	N.E.	
22	Crest.	Near Cork, Belfast and Orkneys.....	29·58		
23	Crest.	Passes London .....	29·59		S.W.
24	Crest.	Near to or S.E. of Paris.....	.....		S.W.

TABLE V.—Exhibiting the principal features of the waves of the S.W. system Nos. 1, 3, 5, 7, 9, 11, and 13.

## Wave No. 1.

Epochs.	Phases.	Directions and Localities.	Altitudes.	Winds.	
				Anterior Slope.	Posterior Slope.
Nov. 1	Crest.	Belfast to Paris .....	30.33	N.W.	S.E.
2	Crest.	Between Belfast and Christiania ...	30.23+	N.N.W.	S.E.
3	Crest.	W. or S.W. of Christiania .....	30.31+	N.W.	S.E.
7	Post. Trough.	N.E. of Belfast and Paris .....			

## Wave No. 3.

Nov. 7	Ant. Trough.	N.E. of Belfast and Paris .....			
8	Crest.	S.W. of Belfast and London .....	.....	N.W.	
10	Crest.	Belfast to Paris .....	29.64	N.W.	S.E.
11	Crest.	Near Christiania .....	29.48		S.E.
12	Post. Trough.	Between Belfast and Shields .....			

## Wave No. 5.

Nov. 12	Ant. Trough.	Between Belfast and Shields .....			
13	Crest.	S.W. of Cork, Plymouth and Paris...			
14	Crest.	Belfast to London .....	29.91	N.W.	S.E.
15	Crest.	Passes the Orkneys.....	30.01		S.E.
16	Crest.	Between Orkneys and Christiania...			E. <sup>b</sup>

## Wave No. 7.

Nov. 17	Crest.	Passes Belfast .....	30.51		S.E. E. <sup>b</sup>
18	Crest.	Between Cork and the Orkneys.....			
19	Post. Trough.	Near Belfast and Shields .....			S.E.

## Wave No. 9.

Nov. 19	Ant. Trough.	Near Belfast and Shields .....			
20	Crest.	Passes the Orkneys.....	29.96		S.E.

## Wave No. 11.

Nov. 21	Crest.	Near Belfast and Shields .....	29.95	N.W.	S.E. E. <sup>b</sup>
22	Post. Trough.	Near Belfast and Shields .....			

## Wave No. 13.

Nov. 23	Crest.	S.W. of Christiania.....	29.62		
	Post. Trough.	Cork to Bristol .....			

<sup>b</sup> Resultants of N.E. and S.E. currents.

TABLE III.—Exhibiting the principal lines of the greatest diminution of pressure of the S.W. system of waves, Nos. 1, 3, 5, 7, 9, 11, 13, 15.

Epochs.	Directions.	Values.	Slopes.
Nov. 1	Belfast to Christiania ...	·55	Anterior, No. 1
3	Christiania to Paris.....	·58	Posterior, No. 1
11	Christiania to Cork .....	·57	3
12	Christiania to Shields...	·13	3
12	Plymouth to Shields ...	·39	Anterior, No. 5
13	Paris to Christiania.....	·59	5
14	Belfast to Christiania ...	·56	5
15	Orkneys to Cork .....	·64	Posterior, No. 5
17	Belfast to Christiania ...	·57	Anterior, No. 7
18	London to Christiania...	·42	7
21	Belfast to Christiania ...	·41	Anterior, No. 11
23	Christiania to Cork.....	·52	Posterior, No. 13
24	Christiania to Cork.....	1·12	13

TABLE IV.—Exhibiting the principal features of the waves of the N.W. system Nos. 2, 4, and 6.

Wave No. 2.

Epochs.	Phases.	Directions and Localities.	Altitudes.	Winds.	
				Anterior Slope.	Posterior Slope.
Nov. 1	Crest.	N.W. of the United Kingdom .....			
2	Crest.	N.W. of the United Kingdom .....	...	N.E.	
3	Crest.	N.W. of the United Kingdom .....	...	N.E.	
4	Crest.	N.W. of the United Kingdom .....	30·49+	N.E.	
5	Crest.	From Cork to the Orkneys .....	30·55	N.E.	
6	Crest.	S.E. of Belfast .....	30·51+	N.E.	
7	Crest.	S.E. of the Orkneys .....	30·43+	N.E.	S.W.
8	Crest.	Passes Plymouth.....	30·13		
9	Crest.	S.E. of Paris .....	29·90+		S.W.
9	Post. Trough.	Passes the Orkneys.....	28·80		S.W.
10	Post. Trough.	Near the eastern coast of Ireland extending to Christiania.....	29·24		S.W.
11	Crest.	Considerably S.E. of Paris.....			
11	Post. Trough.	Passes Plymouth.....	29·12		S.W.

Wave No. 4.

Nov. 17	Crest.	Passes Belfast.....	30·51	N.E. E.	
18	Crest.	Passes London .....	30·53	N.E.	S.W.
19	Crest.	Considerably S.E. of Paris.....			

Wave No. 6.

Nov. 21	Crest.	N.W. of Belfast and Cork .....	.....	N.E.	
22	Crest.	Near Cork, Belfast and Orkneys.....	29·58		
23	Crest.	Passes London .....	29·59		S.W.
24	Crest.	Near to or S.E. of Paris.....	.....		S.W.



TABLE V.—Exhibiting the principal features of the waves of the S.W. system Nos. 1, 3, 5, 7, 9, 11, and 13.

Wave No. 1.

Epochs.	Phases.	Directions and Localities.	Altitudes.	Winds.	
				Anterior Slope.	Posterior Slope.
Nov. 1	Crest.	Belfast to Paris .....	30·33	N.W.	S.E.
2	Crest.	Between Belfast and Christiania ...	30·23+	N.N.W.	S.E.
3	Crest.	W. or S.W. of Christiania .....	30·31+	N.W.	S.E.
7	Post. Trough.	N.E. of Belfast and Paris .....			

Wave No. 3.

Nov. 7	Ant. Trough.	N.E. of Belfast and Paris .....			
8	Crest.	S.W. of Belfast and London .....	.....	N.W.	
10	Crest.	Belfast to Paris .....	29·64	N.W.	S.E.
11	Crest.	Near Christiania .....	29·48		S.E.
12	Post. Trough.	Between Belfast and Shields .....			

Wave No. 5.

Nov. 12	Ant. Trough.	Between Belfast and Shields .....			
13	Crest.	S.W. of Cork, Plymouth and Paris...			
14	Crest.	Belfast to London .....	29·91	N.W.	S.E.
15	Crest.	Passes the Orkneys.....	30·01		S.E.
16	Crest.	Between Orkneys and Christiania...			E. <sup>b</sup>

Wave No. 7.

Nov. 17	Crest.	Passes Belfast .....	30·51		S.E. E. <sup>b</sup>
18	Crest.	Between Cork and the Orkneys.....			
19	Post. Trough.	Near Belfast and Shields .....			S.E.

Wave No. 9.

Nov. 19	Ant. Trough.	Near Belfast and Shields .....			
20	Crest.	Passes the Orkneys.....	29·96		S.E.

Wave No. 11.

Nov. 21	Crest.	Near Belfast and Shields .....	29·95	N.W.	S.E. E. <sup>b</sup>
22	Post. Trough.	Near Belfast and Shields .....			

Wave No. 13.

Nov. 23	Crest.	S.W. of Christiania.....	29·62		
	Post. Trough.	Cork to Bristol .....			

<sup>b</sup> Resultants of N.E. and S.E. currents.

*General Conclusion.*

From the collation of Mr. Brown's with the St. Petersburg and Geneva observations which have been added to Table I., it is readily apparent that the results arrived at in the discussion to which reference has been made have been fully confirmed, and there appears to be but little doubt that the waves as determined in the first instance by a discussion of observations from the stations announced in my first report\*, and further identified and illustrated by the observations collected by Mr. Brown, as well as those which have been brought to light by means of Mr. Brown's observations, and confirmed and illustrated by the St. Petersburg observations, had a *real existence*; an *individuality* has been attributed to certain arrangements of aërial currents and distribution of pressure in connexion with such currents, the aggregate phænomena forming an atmospheric wave. Of the waves thus brought to light, *two* occupy very prominent positions, they stand out as it were from the others; the individuality of each is very striking, and the velocities with which they traversed the area isolate them from their predecessors and exhibit them not as gregarious, but solitary waves. These waves are B° and crest No. 4, the first occurring just previous to the setting in of the great wave, and the last forming its crown. The wave, crest No. 4, appears from its elevated position on the symmetrical or normal wave, admirably adapted to crown our investigations with success, especially in so far as its amplitude, velocity and path are concerned, we are now, I apprehend, in possession of materials to determine with a considerable approximation to accuracy, these elements. Its longitudinal direction appears to have been very extensive. This element would receive considerable elucidation by means of observations from the south of France, Spain, Portugal and the north of Africa. It is highly probable that this wave in the direction of its length stretched from the extreme south to the very north of Europe.

The waves just alluded to will furnish material for a further paper, in which their essential features, elements, &c. will be fully discussed. In the mean time the attention of the reader is directed to several interesting features of waves Nos. 1 and 2.

Wave crest No. 1.—An inspection of Table V. shows that this crest extended from Belfast to Paris on the 1st; on the 4th it passed Christiania, and on the 5th it was vertically over St. Petersburg. On the same day, the 5th, Mr. Brown's observations indicate that the crest No. 2 extended from Cork, past Belfast, towards the Orkneys; so that the point of *intersection* of the two crests, Nos. 1 and 2, must have been situated toward the north-west of Norway. This at once explains the greater amount of pressure in the north-west of Europe in the early part of November.

During the first eight days of November, the movements at St. Petersburg and Geneva were strictly in accordance with the transit

\* Report of the Fourteenth Meeting of the British Association for the Advancement of Science, page 267.

of a large wave from S.W. to N.E., its amplitude being about double the distance between the stations: they were of a precisely opposite character, as indicated in the following table:—

TABLE VI.

Epoch.	Geneva.	St. Petersburg.
Nov. 1	30·34	29·45
2	30·09	29·80
3	29·88	30·21
4	29·82	30·22
5	29·79 <sup>m</sup>	30·25 <sup>M</sup>
6	29·86	30·16
7	29·93	29·96
8	30·01	29·96

M maximum.      <sup>m</sup> minimum.

The line joining Belfast and Paris, the direction of crest, No. 1 on the 1st, when produced, will pass near Geneva, the anterior slope extending to or beyond St. Petersburg. The numbers in the above table clearly show, that when the barometer was *falling* at Geneva from the posterior slope, it was *rising* at St. Petersburg from the anterior. On the 5th the crest passed the latter station, when the posterior trough passed Geneva. This gives for the semi-amplitude, or half breadth of the wave, 1365 miles; the velocity of the *crest*, from its passing Geneva to its arrival at St. Petersburg, being 14·22 miles per hour. The altitude, as measured by the mercurial column, equal to ·46 inch. The fall at St. Petersburg, from the posterior slope of this wave, and the rise at Geneva, from the anterior slope of wave crest No. 3, is also clearly seen in the above table.

Wave crest No. 2.—Table IV. shows that on the 5th this crest extended from Cork to the Orkneys. The anterior trough of this wave was also in the neighbourhood of Geneva on this day. This gives for the semi-amplitude 792 miles. The altitude of the wave appears to have been ·76 inch, and the velocity of the crest about 8·25 miles per hour.

This wave, with its front towards the south-east, was very distinctly developed during the early part of November. The altitude of the crest appears to have *subsided* as the wave progressed; the highest reading at Belfast was 30·55 on the 5th, at London 30·16 on the 6th, and at Paris 29·90 on the 8th. The following tables exhibit the features of the anterior slope. Table VII. shows the barometric rise and fall at stations arranged more or less with regard to a line cutting the crest of the wave transversely. The depressing influence of the wave, crest No. 1, is clearly seen at London and Paris on the 5th. Tables VIII., IX. and X. exhibit the depression of the south-easterly stations below those to the north-west of them while the anterior slope passed.

TABLE VII.—Barometric differences arising from Anterior and Posterior Slopes of Crest No. 2.

Epoch.	Belfast.	Bristol.	London.	Paris.
Nov. 2	−·15	−·13	−·07	−·18
" 3	·00	−·09	−·14	−·13
" 4	+·27	+·18	+·17	+·07
" 5	+·10	+·06	−·01	−·05
" 6	−·04	.....	+·04	+·08
" 7	−·08	−·01?	−·03	+·06
" 8	−·39	−·11	−·05	+·01

TABLE VIII.—Barometric differences arising from Anterior Slope of Crest No. 2.

Epoch.	Belfast.	London.	London. ±
Nov. 1	30·33	30·17	−·16
" 2	·18	·10	−·08
" 3	·18	29·96	−·22
" 4	·45	30·13	−·32
" 5	·55	·12	−·43
" 6	·51	·16	−·35
" 7	·43	·13	−·30
" 8	30·04	30·08	+·04


TABLE IX.




Epoch.	London.	Paris.	Paris. ±
Nov. 1	30·17	30·04	−·13
" 2	·10	29·86	−·24
" 3	29·96	·73	−·23
" 4	30·13	·80	−·33
" 5	·12	·75	−·37
" 6	·16	·83	−·33
" 7	·13	·89	−·24
" 8	30·08	29·90	−·18

TABLE X.

Epoch.	Belfast.	Paris.	Paris. ±
Nov. 1	30·33	30·04	−·29
" 2	·18	29·86	−·32
" 3	·18	·73	−·45
" 4	·45	·80	−·65
" 5	·55	·75	−·80
" 6	·51	·83	−·68
" 7	·43	·89	−·54
" 8	30·04	29·90	−·14



Upon comparing the numbers at the Orkneys with those at Paris, as given in Table I., we at once recognise a considerable difference in the oscillation at the two stations. At the Orkneys it amounted to 1·72, at Paris to ·79, range at the Orkneys in excess ·93. It appears probable that this difference of oscillation at two stations, as the Orkneys and Paris, may be thus explained. The curves in the north-west of Ireland, as determined by the discussion of Sir John Herschel's hourly-observations, are remarkable for boldness and freedom of contour and great range of fluctuation. The late Professor Daniell found, from an examination of the Mannheim observations, that the range increased towards the north-west, and that the greatest oscillation occurred in the neighbourhood of water. Now a wave generated in any way and approaching the continent of Europe from the north-west, would most probably impinge on it with a high and in some cases acuminated crest , but as it passed

onward the crest would gradually subside , so that at stations considerably to the south-east the fluctuations would be very much less than at or near its point of genesis. Again, a negative wave, with a deep trough also approaching from the north-west , would present large fluctuations as it impinged on the land; but after passing onwards, the opposite to subsidence would take place; the depth of trough would decrease, , and the oscillations to the south-east would also decrease. Such phenomena appear to be presented by the observations from the 5th to the 10th of November 1842.

In connexion with this subsidence of crest No. 2, as it passed from N.W. to S.E., it may be interesting to notice the passage of crest No. 3, the circumstances of the transits of the two waves being different; wave No. 2 coming *from* the sea, and passing over Ireland, Scotland and England, and the land forming the central part of Europe; while wave No. 3 skirted the western and north-western boundaries of Europe, and more or less passed over the waters of the Atlantic and North Sea. The crest No. 3 came from the S.W., so that a line from Plymouth to Christiania would cut it more or less transversely; the ranges however are nearly the same at both stations. The crest which traversed England on the 1st arrived at Christiania on the 4th; at this time the barometer had commenced rising at Plymouth from the anterior slope of crest No. 2, and it continued rising until the 7th, when the crest passed. At Christiania the barometer had fallen from the posterior slope of crest No. 2. It appears from a careful comparison and consideration of the barometric movements at Plymouth and Christiania, that crest No. 2 passed Christiania about a day earlier than it did Plymouth, that is, the longitudinal direction of the crest was such as to cause it to pass over Christiania while Plymouth was still under the anterior slope of the wave, the sections passing over Christiania and Plymouth being separate and distinct. The character of the passing wave is well-

determined at both stations, the posterior slope exhibiting a rapid and deep fall, which took place alike at Christiania and Plymouth.

The crest No. 2 passed Cork, Belfast and the Orkneys on the 5th, Plymouth on the 7th, and Paris on the 8th, with a diminution of oscillation. We find however no diminution of oscillation at Christiania as compared with Plymouth. It is highly probable the subsidence of the crest, as it proceeded towards Paris, resulted from the influence of the land in England, while both at Plymouth and Christiania the crest was but slightly interfered with by the influence of land, the difference of level resulting from the anterior slope of crest No. 3.

These considerations exhibit a large wave of considerable breadth and slow motion, extending in a longitudinal direction from the extreme south-west of England, past Norfolk, towards the Swedish capital.

The elements of the waves, as referred to in Tables II., III., IV. and V., have been determined by observations made *daily*; and a perusal of the discussion of these observations will clearly show that, in order to detect and examine the characters of the *larger* undulations, daily observations may suffice. For this purpose it will be best to choose the middle of the day. The Astronomer Royal has determined, from four years' observations at Greenwich, that at 2 hours, Göttingen mean time (20 minutes past 1 p.m. Greenwich time), the barometer varies but  $0^{\text{in}}\cdot001$  from the mean of the year, that is, if the mean pressure is to be determined from an isolated observation each day, this hour is the most suitable, the mean correction being  $0^{\text{in}}\cdot001$  to be added. This at once indicates, that for the present purpose this hour is also the most suitable, the observations being, to a great extent, free from the effects of the diurnal oscillation. If two observations are made, the most suitable epochs are 7·20 A.M. and 1·20 P.M.; and if three, 7·20 P.M. in addition. In order however to detect and examine smaller undulations, observations at shorter intervals equally disposed throughout the twenty-four hours are essential.

LXXVII. *Experiments proving the common nature of Magnetism, Cohesion, Adhesion and Viscosity.* By Sir GRAVES C. HAUGHTON, K.H., M.A., F.R.S., Foreign Associate of the Institute of France, &c.

[Continued from p. 457.]

## PART II.

*Of the mutual magnetic attractions of non-ferruginous bodies.*

AFTER a great number of substances had been measured according to the preceding plan, it occurred to me that by a slight modification of the needle, the mutual magnetic relations of non-ferruginous bodies might possibly be ascertained. Through the obliging readiness of M. Pixii of this city, I had

two needles constructed, half of which were magnets and the other half brass; the junction of the brass and steel being effected exactly mid-way where the caps were attached. As it was anticipated that the movement of such a needle would be slow, as the whole directive energy could only be obtained from the magnetic end, their caps were of stone. I had the pleasure to find that though their movement was necessarily sluggish in comparison with that of the common needle, yet that it was perfectly uniform and certain; and that the brass end attached itself to various substances with even more tenacity than had been the case with the purely magnetic needle, or with its own magnetic portion. I next had two needles made, one with a gold and the other with a silver termination; but owing to a misapprehension of the workman their caps were of brass, and from this circumstance the increased friction prevented their free movement, particularly in the case of the silver needle. However, sufficient was discovered by their employment to see that the gold and silver could likewise be made to attach themselves to non-ferruginous bodies.

It was only now necessary to make such a modification of the new principle of measurement as should enable it to be applied to any metal, and for this purpose I had short tubes soldered to the ends of half magnetic needles where they were joined to their agate caps. Into these small tubes needles of any malleable metal could be inserted at pleasure; their weight and length being just sufficient to balance the steel portion of the needles. The steel ends were necessarily made thicker than the magnetic needle I have already described, to balance the increased weight of the tubes and metal needles; and the whole weighed, when complete, in one case sixteen grains, and in the other, which was a little larger, nearly eighteen grains. The length of the steel half was twenty-three millimètres, and the whole needle therefore forty-six millimètres long. It is necessary to bear in mind that the metal needles should not exceed the magnetic ends in length, otherwise, according to the principle of the lever, they would be moved with greater facility than the steel portion, and give proportionably higher results.

The form of these needles is here given with and without their metal ends.



By a further modification, the arrangement was fitted to be

employed with any small fragment or substance that it was desirable to use as a needle. This consisted simply in making a slender needle of wood of the same form as those of metal, by means of which any fragment, though not exceeding the size of the head of a pin, could be attached with wax, and then employed, the same as if it were a needle of malleable metal, after being inserted in the little tube of the magnetic needle. The deficient weight of the wooden needle can be made up by twining round it a fine strip of sheet-lead; and in like manner the metal needles may be made to balance with equal nicety, when too light, by rolling round them a small strip of tin foil; a necessity that must often occur, owing to the different densities of the metals employed. All these details are requisite, as the experimenter must in so many cases become his own workman, to meet each particular exigency.

These needles were urged to the substances by a bar-magnet, in the same way as in the preceding experiments, and were employed either in measuring the purely magnetic intensities or those of non-ferruginous bodies, by using one or the other end as occasion required. The unmagnetic ends were likewise made to connect themselves with substances, for which they had a strong affinity, by means of the finger instead of a bar-magnet as in the former cases, but to a much greater extent, for it was the only means I had at my command of ascertaining that the junction was made with more than usual readiness, and this result is here marked by the letter *f*, instead of the signs  $\times \times$  as before. All the other signs formerly used are again employed in similar cases, and it will be seen, perhaps with some surprise, that the mutual affinities of non-ferruginous bodies greatly exceed those of the magnetic needle, there being few cases in which the measures did not reach  $90^\circ$ .

In the preceding branch of this subject, whatever has been said on the importance of giving time for the influence of the needle to take effect, is no longer necessary; and all that is required is to afford the time requisite for oscillation entirely to cease; for here the effects are purely *normal*, as non-ferruginous bodies only are mutually concerned. This result might have been easily anticipated, for it is now merely natural and spontaneous affinities that are brought into play. Magnetism, in the common acceptation of the term, will be seen to be an *abnormal* and exceptional state, and all the results that proceed from it must partake of the same character. The more powerful therefore the magnet or the loadstone, the more abnormal and exceptional must be its results.



The phænomena brought to light by Dr. Faraday's mode of experimenting are indeed very remarkable, and they appear to me to have a surprising analogy with what he and Thilourier effected, when operating on the gases. By the resources of science, those bodies which in their normal state only exhibit repulsion, were made to reveal their occult power of attraction; and in a similar manner in these new experiments, bodies that had been only remarked for indifference to magnetism, were brought within its pale. Yet the instances which I have already shown, and those which are to follow, will make it more than doubtful whether the two categories of attraction and repulsion under which bodies arranged themselves in his hands, are really those that would result from their normal conditions, if these could be known with exactness. Thus by his experiments he considers platinum as magnetic, and flint glass as diamagnetic, and they ought not therefore to agree in their affinities; yet I find that they both have a remarkable attraction for lead; crown glass too, so far from having a feeble attraction for the magnet, as we are led to suppose from his experiments, has really a very considerable affinity for it, as will be evident by the instance I gave of the needle in the compass-box, which attached itself *per saltum* to its glass screen on a magnet being held over it. Palladium and platinum also, which Dr. Faraday considers as magnetic, agree with all his diamagnetic metals with which I have operated in their attraction for glass of every kind, and I find that both palladium and platinum exhibit a much feebler degree of attraction for the magnetic needle than they do for iron that is free from magnetism. Yet in his scale crown glass ranges between palladium and platinum.

The obscure nature of affinity cannot be better exemplified than in considering these experiments, where we have a fresh demonstration of its universality. As no instance has yet been discovered of a body existing free from repulsion, for no bodies have yet been found with their molecules in close contact, so in the present case we shall see that where attraction could not be more than imagined, that that property exists in considerable strength, and that it is owing to it, by the mere affinity of points for points, that is of molecules for molecules, that bodies hold together with such tenacity, though each point is totally inert beyond its own sphere of action; indeed so much so that the finest hair or a fragment of any substance not so large as the head of a pin, has as much influence, magnetically, on the various needles that have been tried, as the largest mass. It is different however where the electro-magnet is concerned, for through its powerful influence the

magnetic energies of bodies brought within its range are heightened to a surprising degree. Magnetism proves the co-existing and alternating nature of attraction and repulsion, and that where we find one we may be quite certain that the other is only in abeyance, existing potentially, and ready to spring into action the moment the conditions of bodies are such as to admit of this law of nature coming into activity. I say bodies, because, as it is a law of relation, there must be the mutual presence of two things to enable it to exist. Now the *insensible* relations which the present experiments reveal, *seem* identical with those which Dr. Faraday was able to make *sensible*, even to an audience in a theatre, by the means of a powerful electro-magnet, just in the same way as when a bit of paper excited positively or negatively, and scarcely affording the slightest indication of its excitement, will be roused into strong repulsion by the approach of any electric body in a *similar* state with itself, but more highly excited; and indeed the degree of its repulsion will be proportioned to the energy of the electric state of the body with which it is in relation; it being one of the mysterious peculiarities of affinity that the body that exhibits it in the strongest degree raises the feebler related body up to a state of equality.

The neutral state which Dr. Faraday supposes to exist through the struggle of what he terms magnetic and diamagnetic forces, in various saline solutions, is according to the preceding views due entirely to the relative strength of the latent attraction or repulsion roused into a state of exaggeration, and if I may so say *abnormalism*, by the surpassing energy of the electro-magnet; but were we to suppose the phænomena thus exhibited to be the normal state of nature, we should interpret wrong, and mistake the exception for the rule.

But as facts are better than all reasoning, I proceed to exhibit the mutual attractions and presumed repulsions which I elicited in about 600 additional experiments, together with the measures so obtained.

The measurements of the experiments which follow were first thrown into a tabular form, the substances being arranged perpendicularly on the left side of the paper, and the needles in succession at the top, both of them being between ruled lines in opposite directions. This arrangement had the great advantage of affording an easy contrast at a glance, but notwithstanding the number of experiments that have been made it left too many *lacunæ*, owing to the great number of needles and substances which I had not time to try; for the sake of saving space it was abandoned therefore for the present plan.

The needles employed are indicated by the letters which

follow. The glass needle was of flint glass. It is possible that one of crown glass might have given even higher results; but if so its effects could not have been appreciable in the cases above  $90^\circ$ . By a reference to the counter experiments where crown and flint glass were the substances, and the metals the needles, there will be seen to be a great equality in the results.

M magnet; I iron; P. G. pure gold; G 18c. gold 18 carats; S silver; P palladium; Pl. platinum; Cad. cadmium; T tin; L lead; Z zinc; Cop. copper; B brass; Germ. S. German silver; Ars. arsenic; Gl. glass.

*1st Class.*

Gold (pure) with M  $90^\circ$ , difficult; I  $90^\circ+$ ; P. G.  $90^\circ+$ ; G 18c.  $90^\circ$ ; P  $90^\circ$ ; Pl.  $81^\circ$ ; Cad.  $90^\circ$  f.; T  $90^\circ+$ ; Z  $90^\circ+$ .

Gold (18 carats) with M  $90^\circ$ , difficult; I  $90^\circ$ ; T  $90^\circ+$ ; B  $90^\circ \times$ .

Silver (pure) with M  $90^\circ+$ ; I  $90^\circ+$ ; Cad.  $90^\circ$ ; T  $90^\circ \times$ .

Silver (coin) with M  $90^\circ+$ ; Cad.  $90^\circ+$ ; T  $88^\circ$ .

Palladium with M  $68^\circ$ ; I  $90^\circ+$ ; P  $40^\circ$ ; Cad.  $60^\circ$ ; T  $90^\circ$ .

Platinum with M  $65^\circ$ ; I  $90^\circ$ ; P. G.  $86^\circ$ ; Pl.  $75^\circ$ ; P  $25^\circ$ ; Cad.  $81^\circ$ ; T  $82^\circ$ ; Gl.  $90^\circ$  f. easy.

Cadmium with M  $90^\circ$ ; Pl.  $76^\circ$ ; after many efforts it would not come up to the  $81^\circ$  of the cadmium needle and platinum; Cad.  $90^\circ$ , easy; T  $90^\circ$ , very difficult.

Tin with M  $90^\circ+$ ; I  $90^\circ$ , but difficult; Cad.  $90^\circ$ , weak; T  $90^\circ$ , difficult at every step.

Lead with M  $90^\circ+$ ; I  $90^\circ+$ ; P. G.  $90^\circ$  f. very easy; Pl.  $90^\circ$ , easy; L  $84^\circ$ ; Z  $90^\circ$ , difficult; B  $90^\circ$  f.; Gl.  $90^\circ$  f. very firm.

Zinc with M  $90^\circ$ ; Cad.  $90^\circ$ ; T  $90^\circ \times$ ; L  $90^\circ \times$ ; Z  $90^\circ$ ; Cop.  $90^\circ$ .

Copper with M  $90^\circ+$ ; P. G.  $90^\circ$  f. firm; Pl.  $90^\circ+$ ; T  $90^\circ \times$ ; Z  $90^\circ$ ; Cop.  $90^\circ$ ; B  $90^\circ$ , f. firm.

Iron with I  $90^\circ$ , very easy; T  $90^\circ$ , rather difficult.

Mercury with M  $90^\circ$ , sticks; G. 18c.  $90^\circ$ , sticks; T  $76^\circ$ .

Antimony with M  $66^\circ$ ; I  $27^\circ$ ; P. G.  $90^\circ+$ ; Pl.  $50^\circ$ ; Cad.  $90^\circ$ , very difficult; T  $90^\circ$ ; Cop.  $90^\circ$  f.; B  $90^\circ$ , firmish with the cut end.

Bismuth with M  $51^\circ$ ; I  $77^\circ$ ; P. G.  $90^\circ+$ ; Pl.  $94^\circ$ ; Cad.  $90^\circ$ , difficult; T  $90^\circ$ ; Cop.  $90^\circ$ ; Gl.  $90^\circ$ .

Chromium with M  $90^\circ$ ; at first it quite failed, but rose by waiting a very long space of time up to  $90^\circ$ ; I  $60^\circ$ ; Pl.  $90^\circ$ ; T failed; L  $36^\circ$ .

Manganese with M. A.; Pl. failed; T  $10^\circ$ .

Cobalt with M  $90^\circ$ ; T failed.

Brass with M  $90^\circ$ ; I  $90^\circ$ , not easy; P. G.  $90^\circ$  f.; Pl.  $90^\circ$ ; T  $90^\circ \times$ ; L  $90^\circ$ , f. very easy and firm; B.  $90^\circ$ , f. very easy, firm.

German silver with M. A.; P. G.  $90^\circ$ , f.; L  $90^\circ$ , f. very easy and firm; Cop.  $90^\circ$ , f. most easy; B  $90^\circ$ , f. strong; Gl.  $90^\circ$ , f. very strong.

Bell-metal with M  $90^\circ$ ; P. G.  $90^\circ$ , f.; T  $90^\circ$ , junction easy after the use of the magnetic needle; L  $90^\circ$ , f. easy; Cop.  $90^\circ$ , f. firm; B  $90^\circ$ , f. very firm; Germ. S.  $90^\circ$ ; Gl.  $90^\circ$ , f. easy.

Arsenic with M  $15^\circ$ ; I  $34^\circ$ ; P. G.  $19^\circ$ ; S failed; P  $34^\circ$ ; Pl.  $0^\circ$ ; Cad.  $90^\circ +$ ; T failed; L  $90^\circ$ ; Z  $28^\circ$ ; Cop.  $57^\circ$ ; B failed; Germ. S. failed; Ars.  $65^\circ$ ; Gl.  $52^\circ$ .

Plumbago with M failed; I failed; Pl. failed.

In this and the following classes, the results already obtained with the magnetic needle are again repeated for the sake of affording an easy means of comparison. The iron needle was of soft iron wire, and on bringing it near a delicate magnetic needle it caused no repulsion, but attracted the N. and S. poles; I therefore believe that if it had any magnetism it did not influence the results, and if so it would have had a tendency rather to diminish than to raise the measurements, for often, as in the case of palladium, platinum, bismuth and arsenic, iron has an advantage over the magnetic needle, though, as may be observed with regard to antimony and chromium, it often falls considerably behind it. Iron in its normal state, that is free from adventitious magnetism, discovers less affinity for other bodies, and *even for itself*, than gold, cadmium, tin, copper, and some other metals. When it was made to attach itself to a bit of its own wire, the connexion was broken with as much facility as was exhibited by most of the metals. This result will be scarcely anticipated, and is a further proof that it was devoid of free magnetism. The polarity that iron exhibits is due entirely to extraneous causes, and as my opinions have been long made up upon the subject, I hope before long to make them known, and to show that it depends upon causes of the simplest kind, if that expression may be applied to phænomena in which so mysterious an agency as affinity is concerned. Manganese exhibited only an attraction of  $10^\circ$  for tin, and none for platinum. Chromium had likewise no attraction for tin, and indicated only  $19^\circ$  for gold, while for platinum it had an attraction of  $90^\circ$ , yet both chromium and platinum are magnetic metals according to Dr. Faraday's experiments, and gold diamagnetic. But all



these unexpected results will be seen at a glance by referring to the list.

Antimony and bismuth stand high with such non-ferruginous metals as have been tried ; platinum, for which the attraction of both is low, being excepted. With cadmium the attraction for antimony was raised to  $90^\circ$  by keeping the latter in contact with a bar-magnet for a short time ; yet tin, between which and cadmium there is such a similarity, attained  $90^\circ$  without being kept in contact with the bar-magnet ; but I see by a reference to the note taken at the time, that it was accomplished with some difficulty. It is remarkable that antimony was twice raised with the brass needle to  $90^\circ$  with great ease, though before it could not be got higher than  $30^\circ$ . In the same way antimony was last year made to connect itself at  $90^\circ$  with the copper needle, while now the cut end could only be got up to  $40^\circ$ , and the side to  $55^\circ$ , but this sort of capriciousness has occurred from time to time without any assignable cause.

*2nd Class.*

Selenium with M  $10^\circ$  ; I  $75^\circ$  ; P. G.  $90^\circ+$  ; G. 18c.  $30^\circ$  ; P  $20^\circ$  ; Pl.  $39^\circ$  ; Cad.  $90^\circ+$  ; T  $90^\circ+$  ; L  $35^\circ$  ; Z  $90^\circ$  ; Cop. failed ; B  $63^\circ$  ; Ars. failed ; Gl.  $90^\circ$ , f.  
Sulphur with M  $28^\circ$  ; I  $39^\circ$  ; P. G.  $90^\circ+$  ; P  $20^\circ$  ; Pl.  $65^\circ$  ; Cad.  $90^\circ$  ; T  $90^\circ$  ; L  $90^\circ\times$  ; Z  $90^\circ+$  ; Cop.  $90^\circ+$  ; B  $65^\circ$  ; Gl.  $90^\circ$  f.

What has already been said with regard to sulphur and selenium in the first part of this paper, leaves but little to be added. They will be found to show only a remarkable affinity for glass, but their contrast with lead is striking. The energy of both was heightened by being kept in contact with a magnet, and then tested with a brass needle. The preference that selenium shows for iron over the magnetic needle, is worthy of attention, as well as the contrast it stands in with sulphur, in its low attraction for lead.

*3rd Class.*

Diamond with M  $65^\circ$  ; I  $90^\circ$ , not very difficult ; G 18c.  $90^\circ$  ; Pl.  $90^\circ$ , f. strong ; Cad.  $90^\circ$ , not easy ; T  $90^\circ+$  ; Z  $90^\circ$  ; Cop.  $90^\circ$ , f. easy ; B  $90^\circ+$  ; Gl.  $90^\circ$ , f.  
Ruby with M  $90^\circ+$  ; Cad.  $90^\circ+$  ; T  $90^\circ+$ .  
Sapphire with M  $90^\circ+$  ; Cad.  $90^\circ+$  ; T  $90^\circ+$ .  
Topaz with M  $74^\circ$  ; Cad.  $75^\circ$  ; T  $68^\circ$ .  
Topaz (pale) with M  $66^\circ$  ; Cad.  $83^\circ$  ; T  $80^\circ$ .  
Jargoon with M  $90^\circ+$  ; Cad.  $90^\circ+$  ; T  $90^\circ+$ .  
Emerald with M  $90^\circ+$  ; Cad.  $90^\circ+$ .

Amethyst with M  $65^{\circ}$ ; Pl.  $90^{\circ}$ , difficult; Cad.  $90^{\circ} \times$ ; T  $90^{\circ} \times$ .

Opal with M failed; Cad.  $90^{\circ} +$ ; T  $90^{\circ} +$ .

Cornelian (red) with M  $35^{\circ}$ ; I  $90^{\circ}$ , f. very strong; P. G.  $90^{\circ}$ , f. strong; T  $90^{\circ}$ , f. strong; Z  $90^{\circ}$ , f. strong; Cop.  $90^{\circ}$ , f. strong; Gl.  $90^{\circ}$ , f. very easy and strong.

Cornelian (white) with M  $37^{\circ}$ ; I  $90^{\circ}$ , f. easy; P. G.  $90^{\circ}$ , f. strong; T  $90^{\circ}$ , f. strong; Z  $90^{\circ}$ , f. strong; Cop.  $90^{\circ}$ , f. strong; Gl.  $90^{\circ}$ , f. very easy and strong.

Blood-stone with M  $37^{\circ}$ ; I  $90^{\circ}$ , f. very easy; P. G.  $90^{\circ}$ , f. very strong; T  $90^{\circ}$ , f. strong; Z  $90^{\circ}$ , f. strong; Cop.  $90^{\circ}$ , f. strong; Gl.  $90^{\circ}$ , f. very easy and strong.

Quartz with M  $90^{\circ}$ ; Pl.  $90^{\circ}$ , *very* easy; T  $90^{\circ}$ , f. strong; Cop.  $90^{\circ}$ , f. easy; B  $90^{\circ}$ , f. firm.

Flint with M  $90^{\circ}$ ; I  $90^{\circ}$ , very difficult; P. G.  $90^{\circ}$ , f.; Pl.  $90^{\circ}$ , not very firm; Cad.  $90^{\circ}$ ; T  $45^{\circ}$ ; L  $90^{\circ}$ , f. easy; B  $90^{\circ}$ , f.; Gl.  $90^{\circ}$ , f. very strong.

This class has been tested but to a limited extent with non-ferruginous needles, but the results are sufficiently plain to prove that they follow the same law as the preceding classes, and there can be no doubt from the indications afforded by what have been tried, that they would have exhibited those strong affinities, belonging to all substances of the vitreous class. A second specimen of diamond, which was a mere spark, was tested with the magnet and iron, but the results were the same as with the first specimen. I am indebted for many of these specimens, to Messrs. Widdowson and Veale of the Strand, who very obligingly afforded me the use of them.

#### 4th Class.

Flint glass (prism, English) with M  $30^{\circ}$ ; P. G.  $90^{\circ}$ , f.; G 18c.  $73^{\circ}$ , difficult; P  $90^{\circ}$ , f.; Pl.  $90^{\circ}$ , f.; Cad.  $90^{\circ}$ , f.; T  $90^{\circ}$ , f.; L  $90^{\circ}$ , f.; Z  $90^{\circ}$ , f.; Cop.  $90^{\circ}$ , f.; B  $90^{\circ}$ , f.; Gl.  $90^{\circ}$ , f.

Flint glass (tumbler) with M  $90^{\circ} +$ ; I  $90^{\circ}$ , f.; P. G.  $90^{\circ}$ , f.; G 18c.  $90^{\circ}$ ; P  $90^{\circ}$ , f.; Pl.  $90^{\circ}$ , f.; Cad.  $90^{\circ}$ , f.; T  $90^{\circ}$ , f.; L  $90^{\circ}$ , f.; Z  $90^{\circ}$ , f.; Cop.  $90^{\circ}$ , f.; B.  $90^{\circ}$ , f.; Gl.  $90^{\circ}$ , f.

Crown glass with M  $90^{\circ}$ ; I  $90^{\circ}$ , f. very easy and strong; P. G.  $90^{\circ}$ , f. not easy; G 18c.  $90^{\circ}$ , very easy; P  $90^{\circ}$ , f.; Pl.  $90^{\circ}$ , f.; Cad.  $90^{\circ}$ , f.; T  $90^{\circ}$ , f.; L  $90^{\circ}$ , f.; Z  $90^{\circ}$ , f.; Cop.  $90^{\circ}$ , f.; B  $90^{\circ}$ , f.; Ars.  $90^{\circ}$ , f. most easy.

Blue glass with M  $90^{\circ} +$ ; I  $90^{\circ}$ , f.; P. G.  $90^{\circ} +$ ; G 18c.  $85^{\circ}$ ; P  $90^{\circ}$ , f.; Pl.  $90^{\circ}$ , f.; Cad.  $90^{\circ}$ , f.; T  $90^{\circ}$ , f.; L  $90^{\circ}$ , f.; Z  $90^{\circ}$ , f.; Cop.  $90^{\circ}$ , f.; B  $90^{\circ}$ , f.; Germ. S.  $90^{\circ}$ ; Gl.  $90^{\circ}$ , f.

Green fancy glass with M  $90^{\circ}$ ; I failed; P. G.  $90^{\circ}$ , f.; G 18c.  $70^{\circ}$ ; P.  $90^{\circ}$ , f.; Pl.  $90^{\circ}$ , f.; Cad.  $90^{\circ}$ , f.; T  $90^{\circ}$ , f.;

L 90°, f.; Z 90°, f.; Cop. 90° f.; B 90°, f.; Germ. S. 90°, f.; Gl. 90°, very easy.  
 Red glass with P. G. 90°, f.; P. 90°, f.; Pl. 90°, f.; Cad. 90°, f. very strong; T 90°, f. very strong; L 90°, f.; Z 90°, f.; Germ. S. 90°, f.  
 Green bottle glass with M 70°; P 90°, f.; Pl. 90°, f.; Cad. 90°, f.; T 90°, f.; L 90°, f.; Z 90°, f.; Cop. 90°, f.; B 90°, f.; Gl. 90°, f.

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Porcelain (French) with M 90°; P. G. 90°, f.; Pl. 90°; B 90°, f.; Gl. 90°, f.  
 Porcelain (dentist's) with M 74°; Pl. 90°, f.; L 90°, f.; Z 90°, f.  
 Blue enamel with M 36°; T 90° x.

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Gypsum stone with M 26°; Gl. failed.  
 Alabaster with M 25°; Gl. 90° x.  
 Marble (gray) with M 55°; G 18c. 90°, difficult; T. 90°.  
 Marble (yellow) with M 10°; P. G. 90°+; G 18c. 56°; P 80°; Pl. 90°; Cad. 90°+; T 90°+; Z 47°; Gl. 73°.  
 Gas coal with M 15; Gl. 0°.

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Iceland spar with M 90°; I 90°+; P. G. 90°, f.; G 18 c. 90°; S 90°, f. easy; P 90°, f.; Pl. 90°, f.; Cad. 90°, f.; T 90°, f.; L 90°+; Z 90°, f. easy; Cop. 90°, f. firm; B 90°, f. strong; Gl. 90°, f.  
 Felspar with M 10°; Cad. 30°; T 45°; Gl. 90°, f. very firm.  
 Selenite with M 15°; P. G. 86°; T 85°; Gl. 90°+.  
 Talc with M. 60°; I 90°+; G 18c. 90°; Z 40°.  
 Protosulphate of iron with M 90°, f. very easy; P. G. 90°, f. very easy; Gl. 90°, f. very easy and strong.  
 Carbonate of iron with M 16°; I 20°; P. G. 90°, f. rather difficult; S 90°, f. firm; P failed; Pl. failed; Cad. 90°, f. firm; T 90°, f. firm; L failed; Z 90°, f. most firm; Cop. 14°; B 14°; Germ. S. 9°; Gl. 90°+.  
 Sulphate of copper with Gl. 90°, f. easy and strong.  
 Crystallized tin with M failed; I 90°, f.; P 90°, not difficult; Pl. 90°, difficult; L 90°, f.; Z 90°, f. contact instantaneous; Cop. 90°, f.; B 90°, f. strong; Germ. S. 90°; Gl. 90°, f. contact instantaneous.

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Alum with M 15°; Gl. 90°, f.  
 Rock salt with Gl. 90°, f. very strong.  
 Sugar candy (white) with P. G. 90°+; T 90°+; Gl. 90° f.  
 Sugar candy (brown) with Gl. 90° f.

In this class it will be seen that iron in many cases maintains a great superiority over the magnet. The glasses seemed

to deserve particular attention, and have been carefully tested. Their strong attraction for non-ferruginous bodies must prove interesting, and Iceland spar vies with them in that property, for the needles had only generally to be pushed against it to form the connexion. The porcelains exceed even the glasses in their affinities for non-ferruginous bodies. The attraction of glass for sugar candy, rock salt and alum, is worthy of attention. The metals were not tried, for want of time, with the exception of gold and tin.

The case of carbonate of iron is very remarkable, for it might *à priori* be supposed that from the large proportion of iron it contains, it would show considerable attraction for the magnetic needle; but this is so far from being the case, that it will be seen to be one of the lowest in the scale, and after being kept eighteen hours in contact with the N. pole of a horse-shoe magnet capable of raising thirty pounds, its affinity for the magnetic needle was not increased. It might also have been expected, from its great density, and the analogy of similar cases, that in consequence of its feeble attraction for the magnet, it would exhibit a strong affinity for glass. It however is otherwise; and by a reference to all the metals with which it was tested, in consequence of these peculiarities, it will be seen to be one of the most remarkable and interesting substances that have been tried, exhibiting a very low state of attraction for almost all bodies, gold, silver, cadmium, tin and zinc being excepted, and for these metals it exhibits a remarkable affinity. The results of many of the experiments on various bodies will show a kind of elective affinity, as is observed in chemical combinations; and no substance exemplifies this remark so completely as carbonate of iron. Thus though it has the strongest affinity for zinc, it does not show an attraction for brass above what it exhibits for copper, which is as low as  $14^{\circ}$ . Yet most bodies have a surprising affinity for brass. Carbonate of iron affords a fresh instance that the iron in a body may be quite inert, in consequence of its combination with another substance neutralizing its affinities.

The low affinity of the magnetic needle for marbles is quite in accordance with the experiments of Dr. Seebeck, and the numbers he obtained for bismuth, platinum and antimony, bear a singular resemblance to those afforded by the present experiments. There may be the same accordance between the others, but there is no means of marking by the present mode, the nice shades of difference after  $90^{\circ}$ . Silver, which he places next to iron in power, seems to take the place of brass, which I find to be generally singularly magnetic.



Amongst the alloys that become neutral by combination, he mentions copper and nickel; now this alloy, which is popularly called German silver, I believe, I found to be so magnetic as to draw the needle after it; but then the proportions of the two alloys most probably differed; his specimen contained two parts of copper and one of nickel, while mine, as I have already remarked, seemed to have an unusual proportion of copper, to judge from its colour.

*5th Class.*

Nacre with M  $90^\circ$ , f. firm; I  $90^\circ$ , f.; P. G.  $90^\circ$ , f. easy; Pl.  $90^\circ$ , f. not very easy; T  $90^\circ$ , f. firm and easy; L  $90^\circ$ , f. firm; Z  $90^\circ$ , f. most easy; Cop.  $90^\circ$ , f. firm; B  $90^\circ$ , f. firm; Ars.  $90^\circ$ , f. most easy; Gl.  $90^\circ$ , f. most easy.  
 Ivory (old) with M  $58^\circ$ ; T  $48^\circ$ ; Gl.  $58^\circ$ .  
 Ivory (new) with M  $90^\circ +$ ; T  $90^\circ +$ ; B  $90^\circ$ , f. firm.  
 Enamel (human tooth) with T  $90^\circ$ .  
 Bone (mackerel's) with M  $90^\circ$ ; Cad.  $90^\circ$ .  
 Whalebone with M  $90^\circ$ ; I  $90^\circ$ ; Cad.  $90^\circ +$ .  
 Horn (buffalo's) with M  $90^\circ$ ; I  $90^\circ$ ; P. G.  $90^\circ +$ ; Cad.  $90^\circ +$ ; B  $90^\circ$ , f. very strong.  
 Horn (cow's) with M  $85^\circ$ ; I  $90^\circ$ ; P. G.  $90^\circ +$ ; Cad.  $90^\circ +$ ; B  $90^\circ$ , f. not very firm.  
 Horn (chamois') with M  $90^\circ$ ; B  $90^\circ$ , f. not very firm.  
 Albumen with Gl.  $90^\circ$  f., strong.  
 Indian ink with M  $70^\circ$ ; Cad.  $90^\circ$ ; L  $90^\circ +$ .  
 Amber with M  $75^\circ$ ; Cad.  $90^\circ$ ; T  $90^\circ$ .  
 Gas coal with M  $15^\circ$ ; Gl. 0.  
 Glue with M  $29^\circ$ ; I  $90^\circ +$ ; P. G.  $90^\circ$ , difficult; P.  $90^\circ +$ ; Pl.  $90^\circ +$ ; Cad.  $90^\circ +$ ; T  $90^\circ +$ ; Z  $90^\circ +$ ; Gl.  $90^\circ +$ ?  
 Pig's bristle with T failed.  
 Cat's whisker with M  $90^\circ \times$ ; I  $90^\circ \times$ ; G 18c.  $90^\circ \times$ ; Cad.  $90^\circ \times$ ; T  $90^\circ \times$ ; L  $90^\circ \times$ ; Z  $90^\circ$ , difficult; Cop.  $90^\circ \times$ .  
 Human hair (brown, male's) with M  $90^\circ$ ; T  $90^\circ$ .  
 Human hair (female's) with M  $80^\circ$ ; T  $90^\circ +$ .  
 Human hair (female's, 2nd specimen) with M  $76^\circ$ ; T  $90^\circ +$ .  
 Human hair (gray, male's) with M  $90^\circ +$ ; T  $50^\circ$ .  
 Human hair (gray, female's) with M  $90^\circ +$ ; T  $90^\circ$ .  
 Human hair (beard, gray) with M  $90^\circ +$ ; Cad.  $90^\circ$ .  
 Horsehair (gray) with M  $90^\circ$ ; T  $90^\circ$ .  
 Horsehair (black) with M  $90^\circ$ ; T  $90^\circ$ .

In this class of animal products the magnetism of the needle is tolerably strong, while that of the other bodies is still more remarkable. The specimen of ivory (a paper-cutter), which could not be less than thirty old years since the time of its importation, only exhibited  $58^\circ$ , but was raised to  $74^\circ$  by contact

with a magnet; while the second specimen, which was comparatively recent, as was evident from its fine colour, gave  $90^\circ$  with the magnetic needle; and the probable cause of this difference will be shortly seen, when I speak of the results obtained with a needle made out of a part of this very piece of ivory. Glue exhibits phenomena of a very interesting character. Its affinity for the magnet was only  $29^\circ$ , after the most painful efforts were repeatedly made to push it further, but on being left in contact with the bar-magnet for some hours, it instantly attached itself at  $38^\circ$ . Its affinity for the iron was  $90^\circ +$ , and it exhibited the same amount of attraction for pure gold, platinum, cadmium, tin, zinc and arsenic, which were all the metals it was tested with. But I have some doubts about all the high measurements, as I fear that the dampness of the weather may have influenced the results, though the different specimens appeared perfectly hard.

*7th Class.*

Deal wood with M  $30^\circ$ ; T  $30^\circ$ ; B  $90^\circ$ , f.

Box wood with T  $15^\circ$ .

Rose wood with T  $90^\circ$ .

Oak with I  $90^\circ$ , very difficult; B  $90^\circ$ , f.

Lignum vitæ with M  $33^\circ$ ; I  $18^\circ$ ; Cad.  $22^\circ$ ; T.  $90^\circ$ ; L  $90^\circ$ .

Cedar wood with M failed; B  $90^\circ +$ .

Vegetable ivory with M  $56^\circ$ ; I  $67^\circ$ ; G 18c.  $90^\circ$ ; Cad.  $90^\circ +$ ; T  $90^\circ$ , very difficult.

Vegetable ivory (2nd specimen) with M failed; I  $50^\circ$ ; P. G.  $90^\circ$ , f. firm; L  $80^\circ$ ; Z  $90^\circ +$ ; Cop.  $90^\circ +$ ; Gl.  $90^\circ$ , f. strong.

Vegetable ivory (3rd specimen) with M failed; I  $70^\circ$ .

Mahogany with B  $90^\circ$ , f.; Gl.  $24^\circ$ .

Lac (pure) with M  $35^\circ$ ; P. G.  $90^\circ$ ; G 18c.  $74^\circ$ ; Cad.  $90^\circ$ ; T  $90^\circ +$ ; L  $90^\circ$ ; Cop.  $65^\circ$ ; Gl.  $90^\circ$  f.

Sealing-wax (black) with M  $25^\circ$ ; I failed; T  $70^\circ$ .

India rubber with M  $80^\circ$ ; Cad.  $90^\circ +$ .

India rubber (bottle) with P. G.  $70^\circ$ ; B  $90^\circ +$ ; Gl.  $50^\circ$ , f.

Resin with M  $90^\circ$ ; T  $90^\circ \times$ ; Gl.  $48^\circ$ .

Gum with Gl.  $90^\circ +$ .

Macaroni with Gl.  $40^\circ$ .

Dry paste on paper with M failed; Gl.  $23^\circ$ .

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The peculiarity connected with the attraction of lignum vitæ and vegetable ivory for the magnet and other substances is remarkable. The lignum vitæ was raised from  $33^\circ$  to  $49^\circ$  by contact for a short time with a magnet, and the same process raised it from  $18^\circ$  to  $30^\circ$  with iron, while with tin and lead it had exhibited  $90^\circ$ , and only  $22^\circ$  are marked for cad-

mium. Three specimens of vegetable ivory were tried on many occasions with great care, and the results they afforded were always uniform, but differed from one another. The first specimen gave  $56^{\circ}$  with the magnetic needle, but the two others failed altogether. On two or three occasions the magnetic needle connected itself with one of the specimens that had previously failed, after it had been kept in contact with a magnet; but I cannot positively say that the magnet was the cause, as on repeating the same experiment on other occasions, no such result took place; still the needle did not connect itself till after the attempt to raise its magnetism, though many trials had previously been made. The attraction of iron for all three specimens is deserving of notice. This singular substance exhibited a strong affinity for all the needles with which it was tested. The affinity of lac for various metals and substances, including glass, is worthy of attention. Sealing-wax and India rubber of the bottle kind exhibit a strong affinity for many non-ferruginous bodies.

I have thought that it would be useful to sum up all the preceding results, in such a way as to exhibit their relative affinities; and with this view they have been thrown into the following tabular form, which though it does not contrast cases altogether analogous, because the substances the needles were tried with were not always the same, still some general inferences may be drawn from them, not devoid of interest.

Results of the Measurements.

Substances.	$90^{\circ}$ and above.	Under $90^{\circ}$ .	Failed.
Magnet.....	51	54	10
Iron.....	25	8	3
Pure gold.....	24	3	0
Gold, 18 carats...	12	5	0
Silver.....	5	1	1
Palladium.....	13	6	1
Platinum.....	21	7	3
Cadmium.....	33	7	0
Tin.....	43	9	2
Lead.....	21	4	1
Zinc.....	20	3	0
Copper.....	23	3	0
Brass.....	30	2	0
Glass.....	38	5	2
Total.....	360	117	23

If the relative affinities thus elicited could be depended upon, the substances would be arranged as follows, and their

relative intensities would be according to the ratio assigned to each :—

Brass .....	16·00	Lead .....	5·144
Pure gold .....	9·00	Tin .....	4·909
Copper .....	8·333	Iron .....	3·750
Zinc .....	7·666	Gold, 18 carats	3·400
Glass .....	6·428	Platinum .....	3·100
Silver .....	6·666	Palladium .....	2·857
Cadmium .....	5·714	Magnet .....	1·640

To judge by the facility with which the needles attached themselves, brass is undoubtedly worthy of the high place it maintains in the preceding list; but I should certainly say from my own convictions, that glass is much stronger in its adhesions than even pure gold, and perhaps equal to brass. The advantage that iron has over the magnetic needle is conspicuous, and is deserving of attention, though the ratio of the magnet might very fairly be raised to 2·00, for it is in part owing to the experiments with the larger range of substances, that it stands so low.

The preceding experiments include a vast variety of substances in the mineral, vegetable, and animal kingdoms, that exhibit such strong attractive affinities for one another, that however much they may differ in their external appearances, and in their very natures, they are bound together by common bonds that connect them all into a single family; for we find the metal attaching itself to crystalline, animal and vegetable substances; and again, the crystal, whether we call it by the name of diamond, salt, or sugar-candy, connecting itself readily to metallic, animal and vegetable bodies. In a similar way animal bodies attach themselves to those that are mineral and vegetable; and to complete the circle, the vegetable kingdom, by its woods, its gums, its lac and its resins, is connected with them all.

Out of about 600 measurements the proportion of those that have exhibited repulsion, or what may be presumed repulsion, for there is only negative proof on the subject, is about 1 in 25. Could a counter system of measurements be established it is possible this might be reversed, and that the same amount of repulsions would be obtained, and this perhaps is the fact in Dr. Faraday's remarkable experiments, owing to the influence of the electro-magnet. Still the facility with which the majority of these connexions was established, can hardly lead to a doubt that they are purely of a normal character, where the magnetic needle is not concerned, because they can only result from the spontaneous effects of



nature ; but in the case where the magnetic needle has been the coercing agent, and the measures indicate low intensities, and where the connexion was made with much difficulty, I should draw a contrary inference. This view is strongly supported by what took place with the cornelians.

The two extremes of the preceding scale give two alloyed metals as the highest and the lowest, namely, brass and the magnet. In seeking for two *unmixed* bodies that should properly represent the natural scale in the mineral kingdom, that is, one in which the magnetic needle is not arbitrarily assumed as the standard of reference, but one that considers the natural classes of substances, I would prefer taking platinum and diamond, and exclude brass and the magnet. The first would take the lead of all metals, namely *metalloids*, while diamond would be placed at the head of all substances of the vitreous class, or *haloids*, that is to say, all the precious stones and all crystals ; platinum as the heaviest, and diamond as the hardest of all known bodies. The class of metalloids will be as naturally closed by potassium, as it will be seen the haloids are by gum, two of the lightest of the metallic and vitreous classes of bodies. The reason for adopting this classification is, that I find that iron, from which the magnet is formed, has no remarkable affinity for other bodies, nor for itself, and that the magnet, constituting an exception to the *rule* of nature, is itself dependent for its exalted effects upon electricity and other extraneous causes, and could only keep its high position while attractive affinity, by our ignorance of natural laws, was supposed to be exclusively its peculiar property. Such a division is by no means meant to undervalue the importance of the magnet ; for though it has led to the discovery of the affinities of all the rest, there can be no doubt it exists by an exceptional law ; and we might as well place iron at the head of all metals, because we are indebted to it for the implements which have enabled us to penetrate to those very metals. The term magnetism therefore will always maintain its ground as the general expression of the magnetic attractions which connect all bodies together, even though they have nothing to do with iron ; in the same way that the term electricity has been extended to all those phænomena in which attraction or repulsion result from friction, and yet having not the slightest resemblance to the *electron* or amber that led to their discovery.

In the mineral kingdom, metallic and crystalline bodies form the mass of its constituents, and they generally stand clearly contrasted with one another ; yet platinum as well as diamond, and all vitreous substances, have a remarkable affi-

nity for other bodies ; and if we fix upon a metallic and vitreous type, no two substances are more entitled to be considered as the chiefs of their respective classes ; and if platinum had not been known, gold would have taken its place ; when those two precious bodies, gold and diamond, that have been so long conjoined in the associations of mankind, would still have maintained their rank.

Such are the views I am led to entertain of universal magnetism, that is of magnetism, cohesion, adhesion, and viscosity. And what is viscosity but the employment of a watery vehicle, which is afterwards to disappear, partly through the effects of heat, but mainly through its greater affinity for air and other substances which are said to absorb it, if inwards, by vessels, through what is called *endosmosis*, and if outwards, *exosmosis*? This fluid helps to hold together gelatinous, albuminous and glutinous substances, in such a manner as to bring all their particles into close connexion with themselves, as well as with the bodies to which they are to be attached, and thus by expelling air from between them all, brings every portion into close contact, when, the fluid part having disappeared, the whole is converted by exsiccation into a concrete mass through the power of the affinities of which we have just had so many examples. In the same manner bodies like lac, resin, pitch, &c., that only require the application of heat to become liquid, are enabled to mould themselves to the inequalities of the bodies upon which they are dropped, and thus making counter-impressions connect themselves with a force proportioned to their surfaces, that is to say, of the collection of points of which these are composed. What too is the water that generally forms the vehicle in the former cases, but a crystal or salt called ice (oxide of hydrogen) liquefied by heat, and of such universal strength of affinity while fluid, as to connect itself with nearly every body it touches, which it is then said popularly to wet, and that too in a degree always in proportion to the quantity of caloric it contains at the moment, those substances being excepted between which and it there exists such a decided repugnance, that they exhibit a repulsion that nothing can overcome, but the union of a third body that neutralizes both, and which we see exemplified in *water, grease, and soap*? Is it desired to strengthen the adhesiveness of gelatinous and albuminous bodies? we then have recourse to a fluid still more powerful than water, to one that has a still stronger affinity for these substances, and we substitute alcohol, the result being a compound of such tenacity that it is considered as a *cement* ; and such is the affinity of alcohol for other bodies that it is employed to clear away, or

to speak more philosophically, to dissolve the film of particles that covers object-glasses and mirrors so rapidly, owing to the affinity or magnetism that all bodies possess for one another, and which the astronomer and the optician unconsciously heighten by the very means to which they have recourse, as the particles that float in the air again attach themselves with increased facility by the use of the alcohol. In a similar, but reverse sense, water will not attach itself to many metals, but if they are kept in a horizontal position, the fluid, in its attempt to shrink away, will collect itself up into little drops as if the surface were greasy, and it is only prevented from flying away by its density, which condemns it to obey the law of gravitation. This repugnance of water for metals I consider to be the main cause why dew does not settle upon them, when neighbouring bodies are completely covered by it.

I had arrived at the preceding convictions, when a discovery I made excited in me a degree of surprise that will, I doubt not, be felt by others. Having formed a needle of the fresh specimen of ivory which I have already mentioned, I was tempted some days afterwards, by a movement almost of idle curiosity, as I had just succeeded with one of sealing-wax against glass, to test it also with the same substance, when I found that by a mere push of the finger, it attached itself firmly to it. In consequence of this most unexpected result, the ivory needle was tried with all the following substances, and it succeeded in every case at  $90^\circ$  by the mere use of the finger, except with those preceded by an asterisk, for which the magnet was obliged to be employed, and therefore their intensities must be considered a shade lower than those of the others, as well as a few that were lower still, but which have their measurements assigned.

Gold.	Cornelian.	Rock salt.
Silver.	Flint.	_____
Palladium.	Marble.	Horn (cow's).
Platinum.	Alabaster.	Ditto (buffalo's).
Cadmium.	Flint glass.	*Ivory (old).
Tin, $49^\circ$ .	Crown glass.	Ditto (new).
Bismuth, $76^\circ$ .	Plaster of Paris (stone)	Albumen.
Lead, $81^\circ$ .	37°.	Glue.
*Zinc.	Stone-ware.	Nacre.
Copper.	Porcelain (French).	_____
Brass.	Ditto (dentist's).	Vegetable ivory.
Bell-metal.	*Felspar.	Ditto (2nd specimen).
German silver.	*Sulphate of copper.	Indian ink.
Sulphur.	*Carbonate of iron.	Lac.
Selenium.	Crystallized tin.	Resin, $75^\circ$ .
Diamond.	Iceland spar.	*Gum.
Opal.	Talc.	

The ivory needle would not connect itself with gypsum, arsenic and antimony, and indeed when the coercing magnet

was withdrawn from the latter, the needle seemed always to set off with more than usual haste, as if strongly repelled, but no symptom of repulsion was exhibited when the ivory was brought near the magnetic needle standing in repose.

Here was a curious problem to be solved. What was the cause of this extraordinary affinity of a substance like ivory for so many heterogeneous bodies, of which porcelain offered the most remarkable instance, as the specimen employed, which stood on a surface of glass, drew the ivory as much as one-fourth of an inch after it on one occasion, and always some small distance when it was merely pushed away? The diamond exhibited the same phænomenon, but in a minor degree, and careful testing proved that electricity in no way contributed to the results.

On referring to the tabulated measurements of which I have spoken, and in which the measures of substances stand in ready contrast, I observed that the needles had attached themselves with remarkable facility to animal bodies, such as ivory, pearl, mother-of-pearl, horn, hair, glue, &c. It was evident, therefore, that it was the affinity of something common to all these substances, that caused such easy contact between them and the needles. This step gained it was not difficult to discover that the gelatine which was common to all these animal products, was the substance that chiefly produced with equal facility a contact with gold, diamond, Iceland spar, marble, alabaster, horn, vegetable ivory, gum, and such dissimilar substances, in the mineral, vegetable and animal kingdoms. The low attraction afforded by the piece of old ivory of which I have spoken, I suppose to have resulted from partial change in its structure through age; and the same cause may be assigned for the difference exhibited by the pearl and the mother-of-pearl in their measures, for from the colour and the style of setting of the pearl, less than fifty years could not be assigned for its workmanship.

I have already alluded to the facility with which all vitreous bodies form connexions, and to include the present class of substances in their proper category, we must get rid a little of our familiar notions, and remember that if gelatine, albumen, gluten, resin, lac and gums are devoted to particular uses that make us only think of them as means of adhesion, they are in fact, when considered under a more extended point of view, animal and vegetable glasses; and what fully establishes this character is the extraordinary fact which I have already mentioned, that glue was raised to  $38^{\circ}$  by contact with a bar-magnet; but in truth since that portion of this communication was despatched for the press, the piece of glue was raised to



78° by being kept in contact for four-and-twenty hours with the great horse-shoe magnet already spoken of, and the magnetic needle attached itself with singular readiness, contrary to what had been previously found to be the case. The whole effect however *gradually* disappeared, the attractive power declining, as was ascertained by successive measurements, till it fell back to its normal state of 28°, in about twenty minutes. It was by mere accident, as may be supposed, that it was kept so long in contact, and most likely so long a time is not necessary to produce the effect\*. This result is well-worthy of being contrasted with what has been said about carbonate of iron, and it will be admitted, I am sure, that two cases less reconcilable with analogy could scarcely be produced. We thus see that gelatine, and in all probability similar substances, conform to the laws that have been seen to prevail in vitreous bodies, and it is only necessary to observe their fractured edges when hard, and particularly those of glue, dried albumen and resin, to be convinced of this fact. Even India rubber when in a low temperature approaches to the same state, and it has been already noted that several needles attached themselves firmly to it. To be convinced that this view, which connects these substances with the glasses, is not fanciful, it is only necessary to call to mind the case of gelatinous hydrate of silicium, which is obtained from a mineral glass that dissolves in water; and it is not amongst the least remarkable of the analogies that exist throughout nature, to connect the *liquor of flints* with broth, white of egg, gum water and paste, and the last of these therefore, by a series of links, with the mysterious powers of the magnet, that caused St. Augustin to faint, and made Thales to believe that it was animated by a soul!

To be quite convinced of the strong affinities that ivory, mother-of-pearl, horn, albumen, tortoise-shell and quill exhibit in common, I formed needles of these substances, and tested them to an extent that left no doubt that they possess the same qualities as ivory; but on the whole the quill and mother-of-pearl approached nearest to a state of equality with that substance, though there was a very slight difference between them all. These fresh proofs leave therefore no

\* It has since however been ascertained that length of time was essentially the cause of raising the magnetism of the glue, for on keeping it subsequently in contact for thirteen hours, its magnetism could only be raised to 49°. Another very important point was likewise fully established, namely, that either pole of a magnet might be used without the glue showing any signs of polarity, for when 78° were obtained it had been kept in contact with the N. end of the magnet, and when 49° was the amount of measurement the S. end was employed, while in each case it was the S. end of the needle that formed the connexion.

doubt that it is owing mainly to the gelatine they contain, that these needles show such a remarkable degree of attraction for nearly all bodies.

I do not mean to say that all cohesion is purely *magnetical* in the extended sense I have given to the term, for such an opinion is scarcely reconcilable with what is observed in chemical combinations, where we see elements changing places according to what is called elective affinity. In the various theories that have been put forth to account for chemical combinations and the cohesion of bodies, the attractions and repulsions of different electrical states have been proposed by the celebrated Berzelius; but though this might, as a theory, obtain currency amongst scientific men, and enable them to reason with facility, as long as acids and their bases are considered, yet it is clearly in fault the moment it is applied to explain the cohesion of simple bodies, such as the pure metals. Many instances may be observed in the preceding experiments, that favour the notion of the existence of elective affinity between the various bodies that have been experimented upon, but to which *immediate* electric agency can with difficulty be conceded; and we must therefore have recourse in such cases to what are called *the laws of nature* for their explanation. With regard however to the combinations of acids and alkalies, I believe the varying preferences they exhibit when brought near one another to result from electricity, though magnetism is not without its share in effecting their union; and I hope shortly to be able to present some considerations that will render this opinion highly probable, by making known a discovery I have made of the antagonism of heat and electricity, as well as the proofs of a single electric fluid.

I cannot take leave of this subject without expressing my unfeigned admiration for the skill exhibited by Dr. Faraday, in his experiments on the same subject, as well as on so many other occasions; and if we differ in our views of magnetical phænomena, it will be easily accounted for, by the opposite results we have obtained, owing to our methods of experimenting, and the greater range and variety of observations that the present one has given birth to, over those recorded by that eminent experimenter.

I hope that the state of my health, which has compelled me to dictate this communication, and which has prevented me from giving it the benefit of after-perusal, will be accepted as a sufficient excuse for any oversights that may be observed.

Paris, 15, Rue de Tournon,  
19th May, 1847.

LXXVIII. *Intelligence and Miscellaneous Articles.*

## ON CAROTIN.

**M.** ZEISE observes that the peculiar matter of the *Daucus carota* has not hitherto been known in a pure state ; for by the process usually employed for its extraction, which consists in treating the juice coagulated by æther, its evaporation, and treating the residue with alcohol or with ammonia, the carotin is obtained always combined with a large quantity of fat oil, and the product is merely a soft mass, containing crystals of carotin. M. Zeise has tried several processes to obtain it in a pure state, and has succeeded by the following :—

The expressed juice of the roots is to be diluted with four or five volumes of water, and to the mixture is to be added sulphuric acid diluted with ten volumes of water. A very slight addition of this acid occasions the precipitation of the whole of the colouring matter of the juice. The clear liquor is to be poured off, and the mass thus obtained is to be washed with water ; after this it is to be boiled for an hour, or an hour and a half, with a strong solution of hydrate of potash : the potash saponifies the fat oil, and dissolves a residue of albumen without acting upon the carotin. The carotin is to be separated by filtration and washed with water.

Thus obtained, the carotin still contains a saline body, which must be decomposed to prevent its admixture with the carotin in the remaining operation. For this purpose, the mass diffused through water and heated is to be treated with excess of dilute sulphuric acid. After having again collected the carotin on a filter and well-washed it, the fatty acid is to be separated by alcohol, at first diluted, but eventually nearly anhydrous. By this process very little carotin is dissolved. The mass is then to be dried by the water-bath.

When perfectly free from oil, carotin is difficultly soluble in æther ; but in order to complete its purification, M. Zeise has taken advantage of a property of carotin, discovered on this occasion, which is, that it is extremely soluble in sulphuret of carbon. When the well-dried mass is treated with this liquid, a solution of a deep red colour is obtained, which is to be separated from a grayish substance which remains undissolved. After having separated nearly three-fourths of the solvent by distillation, anhydrous alcohol is to be added to the residue. When the mixture is left in an open capsule, the carotin soon separates in the form of small, deep red-coloured crystals. The mother-water is to be decanted, and the crystals are to be washed with anhydrous alcohol on a filter, till it comes away nearly colourless. The carotin is then perfectly pure ; and its appearance is somewhat similar to that of coarsely powdered cinnabar, or to that of copper, reduced from the oxide by hydrogen. The crystals are very small, and have a strongly metallic lustre. Carotin has a very slight but somewhat agreeable odour ; it is heavier than water, and perfectly insoluble in it ; nearly insoluble in alcohol, and also in pyroxylic spirit, when it is well-freed from oil ; it is very slightly

soluble in æther and in acetone. It fuses at about  $334^{\circ}$  F. into a deep red-coloured and very transparent liquid, and on cooling it becomes a vitreous mass. After having been fused, it dissolves readily in æther and in alcohol; but these solutions, when evaporated, yield merely amorphous masses. At about  $544^{\circ}$  F. it carbonizes, yielding a small quantity of an oily body, and very little gas. When heated in the air, it burns with flame, and leaves no trace of residue.

Dry chlorine does not act upon dry carotin; but water saturated with chlorine converts it into a perfectly colourless chlorinated body, which is insoluble in water, but is considerably soluble in alcohol and æther, and very much so in sulphuret of carbon. The solutions, when evaporated, yield merely a friable resinous mass.

By elementary analysis, carotin appears to be a compound of carbon and hydrogen in the atomic relation :  $10(C^5 H^8)$ . It is therefore a polymeric of oil of turpentine, a circumstance which is of unquestionable interest.—*Ann. de Ch. et de Phys.*, Mai 1847.

#### ON THE PREPARATION OF IODIDE OF LEAD.

M. Boudet observes that M. Depaire, pharmacien of Brussels, has stated that when iodide of lead is prepared by the reaction of acetate of lead and iodide of potassium, the supernatant liquor contains a considerable quantity of iodine. He found that this liquor, in which acetate of lead produced no turbidness, gave a yellow precipitate of iodide of lead on the addition of a little nitric acid, a precipitate of trisiodide of lead with trisacetate, and free iodine with sulphuric and hydrochloric acids; and submitted to evaporation, leaves a white residue, which on the addition of water, regenerates iodide of lead.

From these facts M. Depaire concluded, that in employing acetate of lead to extract iodine from iodine baths, loss of iodine was sustained; to avoid which, he proposes, as M. Cottereau, jun. had previously done, to employ the trisacetate of lead, which precipitates the iodine perfectly.

M. Depaire afterwards examined in what state the iodine could be retained in the solution from which the iodide of lead was precipitated; and after having ascertained that it was not in a state of simple solution, he appeared disposed to think that it exists in the state of potassio-iodide of lead.

M. Boudet states that experiments, which he performed four years ago, but which he neglected to publish, confirm the remarks of M. Depaire; and he is enabled to add some observations, which, he thinks, will not be uninteresting.

Having ascertained that in preparing iodide of lead with acetate and excess of acid, the weight of the product obtained was less than indicated by theory, to the amount of about 10 per cent., M. Boudet added some nitric acid to the solutions which ceased to precipitate with acetate, and he obtained an additional yellow precipitate of iodide of lead. This suggested the employment of nitrate instead of acetate of lead in preparing the iodide; and on trial it was found to yield such a product as theory would indicate, minus the



iodide dissolved. The use of nitrate of lead appears to have been preferred by several chemists; and M. Baudrimont expressly recommends it, on the ground that the acetate yields some oxiodide.

This first point being established, it remained to explain the loss of the 10 per cent. occurring by the use of acetate of lead; and presuming that the acetate of potash, formed with the iodide of lead, took some part in this phenomenon, the action of this salt on iodide of lead was examined, and the results were as follows:—

When iodide of lead is triturated with acetate of potash, the mixture becomes rapidly moist, gradually loses its colour, and soon becomes perfectly white. This pasty mixture does not act upon litmus paper and pulverized acetate of lead. When exposed to the air it partially liquefies, and partially crystallizes in silky needles of double iodide of potassium and lead; on contact with water it becomes immediately yellow, reviving yellow iodide, and the liquor becomes acid; and when filtered it yields a pale yellow precipitate with triacetate of lead, and regenerates bright yellow iodide, when nitric acid, or dilute sulphuric acid is added. By evaporation it leaves a white deliquescent residue, which regenerates yellow iodide on contact with water. If, instead of water, alcohol be added to the pasty mixture of acetate of potash and iodide of lead, it partly dissolves; the residue, which is almost white, yields yellow iodide on contact with a weak acid; and the supernatant liquor does the same on the addition of nitric acid.

When, on the other hand, iodide of lead is treated with a hot solution of acetate of lead, a colourless acid liquor is obtained, which yields white silky crystals of double iodide of potassium and lead, and a yellowish-white insoluble residue of oxiodide of lead.—*Journ. de Ph. et de Ch.*, Avril 1847.

[*Note.*—A parallel case occurs when acetate of lead is decomposed by chloride of sodium, as will appear by the following remarks in the last edition of my translation of the *Pharmacopœia*, p. 280. “I have lately found, however, that the decomposition is not complete. A double salt is formed, the nature of which I have not yet examined, but which is to a considerable extent soluble in water. Hydrochloric acid occasions the precipitation of more chloride of lead after the action of the chloride of sodium is over.”—R. P.]

#### ON THE ACTION OF BROMINE UPON CERTAIN SALTS.

BY M. AUGUSTE CAHOURS.

*Alkaline oxalates.*—When bromine is gradually dropped into a solution of oxalate of potash or soda, containing a slight excess of alkali, no action ensues if the temperature be low; but if it be raised to 104° or 122° F., action immediately commences, and becomes very vivid; much gas is disengaged, which extinguishes flame, yields a white precipitate with lime-water, and is totally absorbed by a solution of potash; thus possessing all the properties of pure carbonic acid. The liquor contains the bromide of potassium or sodium, which may be obtained in fine crystals by spontaneous evaporation. The oxalates of barytes, lead and silver, are similarly acted upon.

The action is very simple. The bromine combines with the metal of the oxide, while the oxygen of the latter acts upon the oxalic acid to form carbonic acid; the result being  $C^4O^3, MO + Br^2 = MBr^2 + 2C^2O^3$ .

*Action of Bromine on Acetates.*—When bromine is poured into a solution of acetate of potash or of soda, no reaction takes place, nor is there any increase of temperature; the only observable phenomenon is the solution of a considerable quantity of bromine. The liquor is gradually decolorized during spontaneous evaporation, owing to the disengagement of bromine, accompanied with the odour of the hypobromites; if it be heated the decolorization is more rapid. The liquid when evaporated to dryness yields a residue which is almost entirely soluble in alcohol. The alcoholic liquor, when exposed to a gentle heat, leaves pure acetate of potash.

The modes of action of bromine upon alkaline oxalates and acetates are so distinct, that it is evident, if citric and tartaric acids contained oxalic and acetic acid, the former would be entirely converted into carbonic acid, while the latter would remain unacted upon, unless in the nascent state in the presence of bromine, it formed new derivative products.

*Action of Bromine on Citrate of Potash.*—When bromine is gradually added to a concentrated solution of citrate of potash, it soon disappears, and the liquid becomes hot, accompanied with the brisk effervescence of pure carbonic acid.

Bromine is to be added till effervescence ceases, and the liquor becomes of a permanent reddish tint; if the excess of bromine be then removed by the cautious addition of a dilute solution of potash, an oily, colourless, very heavy and fluid liquid is precipitated, the aromatic odour of which resembles that of bromoform.

This liquid is extremely complex, and contains three distinct substances: the most volatile is easily obtained in a pure state; it is bromoform. The least volatile product is solid and crystallizable; it is partly altered by heat, and is evidently a new substance. The third compound, which arises during the reaction, is formed only in very small proportion, and it is impossible to deprive it entirely of bromoform or the crystallized substance.

In order to separate these substances, the rough oil is first to be washed with water to deprive it entirely of bromide of potassium; the mixture of oil and water is then to be submitted to careful distillation, stopping it as soon as but few small drops only of oil pass over with the vapour of water. This first liquid is separated from the water by decantation, dried over fused chloride of calcium, and then distilled alone, as long as its boiling-point remains constant.

The product thus purified is a colourless liquid of an agreeable aromatic and somewhat intoxicating odour; its sp. gr. is 2.90 at 54° F.; it boils at 305° F. When boiled with a concentrated solution of potash it is decomposed, giving rise to the formation of chloride [bromide] of potassium and formiate of potash.

It appeared to consist of, in 100 parts,—

C <sup>4</sup> .....	4.85
H <sup>2</sup> .....	0.40
Br <sup>6</sup> .....	94.75
	<hr/> 100.00

This product possesses therefore the properties and composition of bromoform; the density of its vapour was by experiment 8.632; calculation would give 8.55, on the supposition that the molecule of this compound yields 4 volumes of vapour.

When the distillation of the rough oil with water is continued, a period arrives at which the vapour, on condensing, deposits only a few oleaginous drops; the operation must then be stopped. The oily matter which remains at the bottom of the distilling apparatus concretes on cooling and assumes a crystalline appearance.

To purify this product, it is first washed with water, and then dissolved in boiling alcohol; the solution on cooling deposits long, brilliant, white silky needles: if, on the contrary, excess of alcohol be employed, and the solution be exposed to spontaneous evaporation, large colourless tabular crystals are obtained. The product thus purified is insoluble in water, either cold or hot; alcohol, when it is concentrated, dissolves it in considerable quantity when heated; on cooling the greater portion is obtained in very regular crystals; æther and pyroxylic spirit also dissolve it.

This substance fuses at about 167° F.: when subjected to distillation it is partly altered with the disengagement of bromine; but a considerable portion distils, and condenses on the cool parts of the retort, in long very brilliant needles. A cold concentrated solution of potash dissolves it; when heated complete decomposition occurs. When the operation is over, the retort is found to contain bromide of potassium and oxalate of potash; while the vapour of water carries off an oily aromatic matter which possesses the composition of bromoform. The reaction which occurs is readily explained,—



Concentrated sulphuric acid gently heated dissolves a small quantity; but if heated to ebullition the substance is altered, vapour of bromine and an oily substance being disengaged. Moderately strong nitric acid, when cold, does not alter it; but when heated a small quantity is dissolved, which is deposited on cooling in the form of fine, white, brilliant needles; the portion which does not dissolve assumes the appearance of an oil, and again concretes on cooling.

By analysis this substance appeared to consist of—

C <sup>12</sup> .....	7.67
H <sup>2</sup> .....	0.21
Br <sup>10</sup> .....	85.30
O <sup>4</sup> .....	6.82
	<hr/> 100.00

The formation of this product by means of solution of citrate of potash may be explained by means of the following equation,—



On account of the decomposition of this substance by the alkalies the author proposes to call it *bromoxaform*. In this operation there is then first a case of oxidizement which resolves the molecule of citric acid into more simple products; then the bromine acting by itself on one of the products formed, gives rise to a phænomenon of substitution.

On observing the preceding product so readily decomposed on the presence of bases and of water, may it not be admitted that this compound is first formed, and that the bromoform is merely the result of a secondary action?

The citrates of soda and barytes act in the same manner as the citrate of potash, bromoform and the crystalline matter being both obtained with them.—*Ann. de Ch. et de Ph.*, Avril 1847.

**A CHEMICAL INVESTIGATION OF SOME SILICATES CONTAINING CHLORINE, SULPHURIC AND CARBONIC ACID. BY M. WHITNEY OF BOSTON, UNITED STATES.**

Long ago Arfvedson observed, in examining sodalite, that it dissolved entirely in nitric acid, although it formed a gelatine with hydrochloric acid. Gustave Rose made a similar observation upon cancrinite from the Ilmen mountains, which dissolves to a clear liquid in concentrated hydrochloric acid, but suddenly gelatinizes on boiling. Mr. Whitney found that a whole series of silicates, which, as regards composition, exhibit great similarity to the above minerals, possess the property of dissolving to a clear liquid in all dilute acids. All these minerals gelatinize with concentrated acids, with the exception of cancrinite, which is likewise soluble in them. To this series belongs, besides sodalite and the cancrinite from the Ilmen mountains, elæolite from Litchfield (Maine), noseau from the Lachersee, hauijne from the Albanese mountains and from Niederme-nig, and the cancrinite discovered by Dr. Jackson of Boston, along with the above elæolite at Litchfield.

The author has examined all these minerals with the greatest care, and the following are the interesting results of his investigation:—sodalite, noseau and hauijne, may be regarded as combinations of a silicate of alumina and soda, of the composition  $\text{NaO Si O}^3 + 3(\text{Al}^2 \text{O}^3 \text{SiO}^3)$ , with chloride of sodium, sulphate of soda, and sulphate of lime. This silicate has hitherto never been met with in the isolated state. It possesses the composition which was formerly ascribed to elæolite. The following are the formulæ of the minerals under consideration:—

$\text{NaO Si O}^3 + 3\text{Al}^2 \text{O}^3 \text{Si O}^3 + \text{Na Cl}$ , sodalite.

$\text{NaO Si O}^3 + 3\text{Al}^2 \text{O}^3 \text{Si O}^3 + \text{NaO SO}^3$ , noseane.

$\text{NaO Si O}^3 + 3\text{Al}^2 \text{O}^3 \text{Si O}^3 + 2\text{CaO SO}^3$ , hauyne from Albanese mountains.

$\text{NaO Si O}^3 + 3\text{Al}^2 \text{O}^3 \text{Si O}^3 + \text{NaO SO}^3$  } hauyne from Niederme-  
 $2(\text{NaO SO}^3 + 3\text{Al}^2 \text{O}^3 \text{SiO}^3 + 2\text{CaO SO}^3)$  } nig.



The cancrinite from the Ilmen mountains and that from Litchfield are combinations of true elæolite,  $2\text{NaO Si O}_3 + 2(\text{Al}^2 \text{O}_3 \text{ Si O}_3)$ , with carbonate of lime and carbonate of soda.

$\text{Na}^2 \text{O}^2 \text{ Si O}_3 + 2\text{Al}^2 \text{O}^3 \text{ Si O}_3 + \text{CaO CO}_2$ , cancrinite from Ilmen mountains.

$\text{Na}^2 \text{O}^2 \text{ Si O}_3 + 2\text{Al}^2 \text{O}^3 \text{ Si O}_3 + \left\{ \begin{array}{l} \frac{1}{2} \text{NaO} \\ \frac{1}{2} \text{CaO} \end{array} \right\} \text{CO}_2 + \text{HO}$ , cancrinite from Litchfield.—*Bericht der Berliner Akademie*, Feb. 1847.

#### NEW BOOKS.

Scientific Memoirs, Part XVII., commencing the 5th Volume, containing Schmidt's Contributions to the Comparative Physiology of the Invertebrate Animals, being a Physiologico-Chemical Investigation; Fresnel on the Colours produced in Homogeneous Fluids by Polarized Light; Jamin on Metallic Reflexion; Dove's Researches on the Electricity of Induction.

Manual of British Botany, containing the Flowering Plants and Ferns arranged according to the Natural Orders, by Charles Cardale Babington, M.A., F.L.S. &c. 2nd edit.

The Elements of Botany, Structural and Physiological, by John Lindley, Ph.D., F.R.S. &c. 3rd edit.

#### METEOROLOGICAL OBSERVATIONS FOR APRIL 1847.

*Chiswick*.—April 1. Slight haze: sleet and hail-showers: frosty. 2. Overcast. 3. Cloudy and cold: rain. 4. Overcast. 5. Cloudy: fine. 6. Cloudy: rain at night. 7. Cloudy: rain. 8. Rain: cloudy: clear. 9. Clear and cold. 10. Cloudy: fine. 11. Uniformly overcast: rain. 12. Rain: cloudy. 13. Densely clouded. 14. Bleak and cold: slight hail-showers. 15. Snow and hail: clear, cold and dry: sharp frost at night. 16. Sharp frost: clear and cold. 17. Frosty and foggy: cold and dry. 18. Clear. 19. Clear: cloudy: slight frost. 20. Foggy: fine, with sun: cloudy. 21. Clear: overcast. 22. Cold haze: frosty. 23. Fine: clear at night. 24. Slight haze: fine, with sun: frosty. 25. Foggy: fine. 26. Rain: overcast. 27. Boisterous, with showers. 28. Fine: showery. 29. Heavy showers, partly hail: thunder. 30. Clear: heavy clouds and showers.

Mean temperature of the month .....  $44^{\circ} 28$

Mean temperature of April 1846 .....  $47^{\circ} 36$

Mean temperature of April for the last twenty years ...  $47^{\circ} 20$

Average amount of rain in April ..... 1.47 inch.

*Boston*.—April 1. Cloudy: snow A.M. and P.M.: rain yesterday. 2. Cloudy: large fall of snow A.M.: rain P.M. 3. Cloudy: snow A.M.: rain P.M. 4–6. Fine. 7. Cloudy. 8. Rain: rain early A.M.: stormy, with rain P.M. 9. Stormy. 10. Fine. 11. Fine: rain A.M. and P.M. 12, 13. Cloudy. 14. Fine. 15. Cloudy: snow-showers. 16. Fine. 17, 18. Fine: ice this morning. 19. Cloudy. 20–23. Fine. 24. Fine: rain P.M. 25. Cloudy: rain and hail P.M. 26. Rain: rain A.M. 27. Windy: rain P.M. 28. Windy. 29. Fine. 30. Cloudy: thunder and rain P.M.

*Sandwick Manse, Orkney*.—April 1. Snowing: snow. 2. Snowing: showers: snow-showers. 3. Cloudy. 4. Bright: damp. 5. Sleet-showers: showers. 6. Bright: showers. 7. Bright: hail-showers: aurora. 8. Rain: showers. 9. Showers: clear: aurora. 10. Clear. 11. Clear: rain. 12. Bright: cloudy. 13. Snow-showers: cloudy. 14, 15. Cloudy. 16. Clear: cloudy. 17. Showers: clear. 18. Showers: cloudy. 19. Fine: clear. 20. Showers: clear. 21. Cloudy: clear: frost. 22. Bright: cloudy. 23. Bright: showers. 24. Showers: drops. 25. Bright: cloudy. 26. Bright: clear. 27. Bright: rain. 28. Showers: drizzle. 29, 30. Bright: clear.

*Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at CHISWICK, near London; by Mr. Veall, at BOSTON; by the Rev. W. Dunbar, at Applegarth Manse, DUMFRIES-SHIRE; and by the Rev. C. Clouston, at Sandwick MANSE, ORKNEY.*

Days of Month.	Barometer.					Thermometer.					Wind.				Rain.													
	Chiswick.			Dumfries-shire.		Orkney, Sandwick.		Chiswick.			Dumfries-shire.		Orkney, Sandwick.		Boston.		Dumfries-shire.		Orkney, Sandwick.		Boston.		Chiswick.		Dumfries-shire.		Orkney, Sandwick.	
	Max.	Min.	Boston. 24 a.m.	9 a.m.	9 p.m.	94 a.m.	84 p.m.	Max.	Min.	Boston. 24 a.m.	Max.	Min.	93 a.m.	84 p.m.	Chiswick. 1 p.m.	Calm.	Calm.	Calm.	Calm.	Calm.	Calm.	Calm.	Calm.	Calm.	Calm.	Calm.	Calm.	Calm.
1847. April.																												
1.	29.404	29.300	29.08	29.37	29.42	29.37	29.42	46	23	38			36	34	s.	nw.	nne.	.05	.08									.28
2.	29.333	29.247	28.95	29.54	29.60	29.54	29.60	44	28	36.5			37	37	n.	calm	ne.	.07										.07
3.	29.452	29.340	29.04	29.60	29.58	29.60	29.58	44	32	35			39	38	nw.	nw.	w.	.04	.18									.05
4.	29.702	29.603	29.39	29.46	29.33	29.46	29.33	50	37	42			43½	43	nw.	ne.	sw.	.02	.22									
5.	29.745	29.629	29.33	29.35	29.45	29.35	29.45	56	39	42			43½	41	w.	n.	nw.	.04										.21
6.	29.853	29.745	29.36	29.65	29.45	29.65	29.45	55	44	46			43	43	n.	n.	se.	.04										.08
7.	29.837	29.813	29.36	29.43	29.48	29.43	29.48	56	42	49			44	42½	w.	calm	w.	.10										.07
8.	29.625	29.549	29.08	28.61	28.54	28.61	28.54	58	38	51			41½	40	w.	calm	nw.	.02	.36									.41
9.	29.775	29.689	29.16	29.18	29.48	29.18	29.48	55	34	47.5			40	39	w.	w.	nw.	.09										.62
10.	29.900	29.695	29.49	29.70	29.82	29.70	29.82	58	30	46			44	39	w.	w.	calm	.10										.43
11.	29.945	29.784	29.69	29.78	29.54	29.78	29.54	50	47	43			44	44	s.	calm	se.	.28	.07									.02
12.	29.706	29.677	29.30	29.68	29.86	29.68	29.86	64	42	53			44	38	sw.	calm	nw.	.01										.11
13.	29.922	29.841	29.50	30.01	30.18	30.01	30.18	51	33	47			41	38	ne.	e.	n.	.01										.03
14.	29.947	29.912	29.64	30.17	30.08	30.17	30.08	48	34	47			39½	42	ne.	ne.	w.	.01										.05
15.	30.025	29.906	29.61	30.05	30.02	30.05	30.02	48	23	41.5			46	43	ne.	e.	s.	.01										.15
16.	30.026	29.926	29.69	29.85	29.72	29.85	29.72	49	20	41			45½	45½	n.	n.	sse.	.03										.03
17.	29.824	29.757	29.52	29.69	29.84	29.69	29.84	54	22	43			44	43	se.	se.	nw.	.06										.06
18.	29.876	29.799	29.54	29.95	29.90	29.95	29.90	52	34	45			44	40½	ne.	n.	e.	.06										.04
19.	29.813	29.712	29.39	29.76	29.76	29.76	29.76	57	27	41			43½	40	sw.	n.	calm	.06										.12
20.	29.884	29.742	29.50	29.84	29.95	29.84	29.95	60	30	46			43	42	sw.	calm	ne.	.06										.06
21.	30.027	29.962	29.57	30.02	30.06	30.02	30.06	61	36	51.5			45½	38½	w.	n.	ne.	.04										.04
22.	30.064	29.950	29.71	30.06	30.05	30.06	30.05	60	26	50			47	41	ne.	calm	calm	.12										.01
23.	30.024	29.974	29.67	29.93	29.93	29.93	29.93	59	30	49			45	42	e.	calm	e.	.01										.35
24.	29.987	29.962	29.60	29.92	29.92	29.92	29.92	58	28	48.5			43	41	e.	s.	se.	.20										.13
25.	29.997	29.951	29.57	29.89	29.66	29.89	29.66	62	36	51			45½	44	sw.	sw.	se.	.02										.01
26.	29.788	29.781	29.30	29.25	29.39	29.25	29.39	57	43	43			41½	43½	sw.	w.	w.	.05										.35
27.	29.728	29.613	29.11	28.90	28.48	28.90	28.48	61	43	56			49	45½	w.	w.	nw.	.12										.13
28.	29.696	29.579	29.18	28.80	29.00	28.80	29.00	60	40	53			47	45	sw.	calm	wdw.	.28										.01
29.	29.571	29.528	29.10	29.31	29.50	29.31	29.50	57	35	51			45	41½	sw.	calm	se.	.02										.32
30.	29.794	29.667	29.24	29.59	29.68	29.59	29.68	61	30	46			47½	41	nw.	w.	se.	.02										
Mean.	29.809	29.726	29.38	29.611	29.619	29.611	29.619	55.03	33.53	45.9			43.40	41.11				1.08	1.77									3.32

## INDEX TO VOL. XXX.

- ACETAL, on the preparation and properties of, [220](#).
- Acetate of morphia, on the use of coffee as an antidote to, [301](#).
- Acids:—butyro-acetic, [141](#); oxypicric, [148](#); sulphovinic, [211](#); niobic, [360](#); cyanuric, [378](#); stannic, [380](#); metastannic, [381](#); sulphoxiphosvinic, [382](#); hydrofluoric, *ib.*
- Adams' (J. C.) explanation of the observed irregularities in the motion of Uranus, [130](#).
- Adhesion, on the common nature of, [437](#), [502](#).
- Airy (G. B.) on Sir D. Brewster's new analysis of solar light, [73](#); on the new analysis of solar light, [153](#); reduction of the observations of Halley's comet, [207](#); on a proposed alteration of Bessel's method for the computation of the corrections by which the apparent places of stars are derived from the mean place, [208](#).
- Algebra, on a new system of imaginaries in, [458](#).
- Alkalies, organic, on the composition of, [293](#).
- Ammonia, on the exhalation of the bicarbonate of, by the lungs, [124](#).
- Aniline, on the compounds of phosphoric acid with, [40](#).
- Ashes of organic bodies, on the examination of, [369](#).
- Astræa, elements of, [60](#); on the re-appearance of, [133](#).
- Barometric depression of the 25th Dec. 1821, remarks on the, [18](#).
- Becquerel (M. E.) on the experiments of Messrs. Foucault and Fizeau, relative to the action of the red rays upon Daguerreotype plates, [214](#).
- Berghaus' (H.) Physical Atlas, noticed, [53](#).
- Bessel (F. W.), notice of the late, [201](#).
- Bile, on the composition of crystallized, [298](#).
- Birt (W. R.) on the remarkable barometric depression of the 25th Dec. 1821, [18](#); on certain atmospheric or barometric waves, [482](#).
- Blood, on the presence of sugar in, [378](#).
- Boettger (M.) on styphnic acid, [148](#).
- Bonjean (M.) on the presence of sulphur in metallic substances struck by lightning, [222](#).
- Boole (G.) on the Rev. B. Bronwin's method for differential equations, [6](#); on a class of differential equations, [96](#).
- Bostock (Dr. J.), notice of the late, [196](#).
- Boudet (M.) on the preparation of iodide of lead, [524](#).
- Boussingault (M.) on the preparation of the ammoniaco-magnesian phosphate from urine, [462](#).
- Brass wire, on the incandescence of, in vapour of alcohol, [143](#).
- Brewster's (Sir D.) reply to the Astronomer Royal, on the new analysis of solar light, [153](#); on the knowledge of distance given by binocular vision, [305](#); on the conversion of relief by inverted vision, [432](#); on the analysis of the spectrum by absorption, [461](#).
- Bromine, action of, on salts, [525](#).
- Brouwin (Rev. B.) on the integration of some equations in partial differentials, [107](#).
- Brooke (C.) on the automatic registration of magnetometers, and other meteorological instruments, by photography, [57](#).
- Buratite, on the composition of, [65](#).
- Butyro-acetic acid, [141](#).
- Cahours (M. A.) on bisulphuret, tersulphuret, and sulphocyanuret of methyle, [65](#); on sulphocyanuret and bisulphuret of ethyle, [67](#); on the action of bromine upon certain salts, [525](#).



- Cambridge Philosophical Society, proceedings of the, [129](#), [365](#).
- Carotin, on, [523](#).
- Cayley (A.) on a system of imaginaries, [257](#).
- Challis (Rev. Prof.), report of proceedings in the Cambridge observatory relative to the new planet, [33](#); second report of proceedings in the Cambridge observatory relating to the new planet (Neptune), [241](#); on a theory of luminous rays on the hypothesis of undulations, [365](#); on a theory of the polarization of light on the hypothesis of undulations, *ib.*
- Chemical affinities, influence of statical and dynamical induction upon, [263](#).
- Chemistry, animal, researches in, [412](#).
- Children (J. G.) on the use of a mixture of spirit of wine and camphine, as a light for optical purposes, [179](#).
- Chiolite, analysis and description of, [70](#).
- Chromium, on the chromate of, [223](#).
- Cinchona, composition of, [294](#).
- Cloez (M.) on sulphoxiphosvinic acid and its compounds, [382](#).
- Clouston (C.) on the formation of cylindrical masses of snow in Orkney, [301](#).
- Cockle (J.) on some formulæ which serve to indicate the limits of the application of indeterminate methods to the solution of certain problems, [28](#).
- Coffee as an antidote to acetate of morphia, [301](#).
- Cohesion, on the common nature of, [437](#), [502](#).
- Comets, Halley's, reduction of the observations of, [207](#); on the elements of several, [60](#); historical survey of, [210](#).
- Cooke (Isaac B.) on the use of a double circuit in the measurement of electrical forces, [385](#).
- Copper, analyses of the native phosphates of, [68](#).
- Crum (W.) on a method for the analysis of bodies containing nitric acid, and its application to explosive cotton, [426](#).
- Cyanogen, on some compounds of, [378](#).
- Cyanuric acid, preparation of, [378](#).
- Daguerreotype pictures, on a method of producing lights and shades in equal perfection in, [213](#); observations relative to the action of the red rays upon, [214](#).
- Damoiseau (Baron de), notice of the late, [206](#).
- De la Rive (Auguste) on the voltaic arc, [125](#).
- Delesse (M.) on the new mineral (buratite), [65](#); on villemite, [295](#).
- Descloizeaux (M.) on villemite, [295](#).  
— (A.) on the principal geysirs of Iceland, [391](#).
- Differential equations, on the Rev. B. Bronwin's method for, [6](#), [8](#); on a class of, [96](#).
- Differentials, on the integration of some equations in partial, [107](#).
- Dihydrite, analysis of, [69](#).
- Diopside, analysis of the white, [68](#).
- Distance, on the knowledge of, given by binocular vision, [305](#).
- Dove (Prof.) on the phenomena exhibited by polarized light, [465](#); on the depolarization of light, [469](#).
- Draper (Dr. J. W.) on the existence and mechanism of the negative or protecting rays of the sun, [87](#); on a singular property of gun-cotton mixture, [299](#); on the production of light by heat, [345](#).
- Earnshaw (S.) on the mathematical theory of the two great solitary waves of the first order, [129](#).
- Earth, on the figure and primitive formation of the, [59](#); observations respecting the form of the, [194](#).
- Eggs, on the yolk of, [463](#).
- Ehlite, analysis of, [69](#).
- Electrical forces, on the use of a double circuit in the measurement of, [385](#).
- Electricity, on the induction of atmospheric, on the wires of the electric telegraph, [186](#); experiments in, [368](#).
- Electro-magnetic rotation, on a new instance of, [268](#).
- Encke (M.) on the newly-discovered planet, [181](#).
- Equations, on the geometrical representation of the roots of algebraic, [366](#).
- Erdmann (M.) on oxypicric acid—styphnic acid, [148](#).



- Ethyle, on the sulphocyanuret and bisulphuret of, [67](#).
- Eye, on a change in the state of vision of an, affected with a malformation, [366](#).
- Fluid motion, on the equation of continuity in, [104](#).
- Foucault (L.) on a method of producing lights and shades in equal perfection in Daguerreotype pictures, [213](#).
- Fremy (M. E.) on hydrates, stannic and metastannic acids, [380](#).
- Frog, examination of the papillæ and nerves of the tongue of the, [277](#).
- Geology, researches on physical, [59](#).
- Gerhardt (Ch.) on mellon and its compounds, [218](#).
- Geysirs of Iceland, physical and geological observations on the, [391](#).
- Glass, on the composition of Bohemian, [147](#); analysis of, [421](#).
- Glycerine, on the formation of, [146](#).
- Gobley (Mr.) on the yolk of eggs, [463](#).
- Goodman (J.) on a new and practical form of voltaic battery, [127](#).
- Goodwin (Rev. [H.](#)) on the geometrical representation of the roots of algebraic equations, [366](#).
- Graham (Mr.) on the elements of Astræa, [60](#); on the elements of De Vico's fifth comet, [62](#).
- Grove (W. R.) on certain phænomena of voltaic ignition, and on the decomposition of water into its constituent gases by heat, [58](#).
- Gun-cotton, on the composition and explosion of, [1](#); observations on, [258](#), [273](#), [299](#), [409](#), [426](#).
- Hail, observations on, [159](#).
- Haldat (Dr. de) on the universality of magnetism, [319](#).
- Hamilton (Sir W. R.) on quaternions, [458](#).
- Hargreave (C. J.) on the Rev. B. Bronwin's paper on the integration and transformation of certain differential equations, [8](#).
- Haughton (Sir Graves C.) on the common nature of magnetism, cohesion, adhesion and viscosity, [437](#), [502](#).
- Heat, on the production of light by, [345](#).
- Heineken (Rev. N. S.) on the mechanical productions of Abraham Sharp, [25](#); on Newton's telescope at the Royal Society, [211](#).
- Hennessey (H.) on physical geology, [59](#).
- Henry (Prof. J.) on the induction of atmospheric electricity on the wires of the electric telegraph, [186](#); electrical experiments noticed, [368](#).
- Henwood (W. J.) on some meteorological observations made during the year 1846 at Gongo Soco, in the interior of Brazil, [361](#).
- Hermann (M.), analysis of the native phosphates of copper, [68](#); on chiolite, a new mineral, [70](#).
- Hind (Mr.) on the elliptical elements of De Vico's fourth comet, [60](#); note on the first comet of Brorsen, *ib.*; on De Vico's fifth comet, [62](#); on the elements of the sixth comet of De Vico, [63](#); on the reduction of Tycho Brahe's observations of the comet of 1590, [135](#); notice respecting comet, [64](#).
- Hobson (W.) on the equation of continuity in fluid motion, [104](#).
- Hopkins (T.) on the hourly alterations of the vapour atmosphere at Bombay, [246](#).
- Hydrates, researches on the, [380](#).
- Hydrofluoric acid, on anhydrous, [382](#).
- Imaginaries, on a system of, [257](#).
- Induction, memoir on, [261](#).
- Iodide of potassium, easy method of preparing, [146](#).
- Iron bars, on the effects of magnetism upon the dimensions of, [76](#).
- Iron, reduction of persalts of, by zinc, [149](#); on the change effected in the hydrated peroxide of, when kept under water, [294](#).
- Jacob (Capt. W. S.), observations of double stars, made at Poonah, [64](#); on the orbits of double stars, [209](#).
- Johnston's (A. K.) Physical Atlas, noticed, [53](#).
- Joule (J. P.) on the maximum density of water, [41](#); on the effects of magnetism upon the dimensions of iron and steel bars, [76](#), [225](#).
- Kilburn (W. E.) on the chemical action of light, [378](#).
- Kolbe (H.) on the oxidizing power of oxygen when disengaged by means of voltaic electricity, [334](#).
- Lassaigne (M.) on the solubility of carbonate of lime in water contain-

- ing carbonic acid, 297; on the solubility of phosphate of lime in water saturated with carbonic acid, 298.
- Lassell (Mr.) on Le Verrier's planet, 134.
- Laurent (A.) on mellon and its compounds, 218; on the composition of the organic alkalies, 293.
- Lead, on the preparation of the iodide of, 524.
- Lee (Dr. R.) on the ganglia and nerves of the virgin uterus, 127.
- Lefevre (M. B.) on a method of producing lights and shades in equal perfection in Daguerreotype pictures, 213.
- Libethenite, analysis of, 69.
- Liebig (Prof.) on some new researches in animal chemistry, 412; on thialdine and selenaldine, two new organic bases, 470.
- Light, solar, on Sir David Brewster's new analysis of, 73; on the theory of the aberration of, 93; on the new analysis of solar, 153; for optical purposes, on the use of a mixture of spirit of wine and camphene as a, 179; effect of magneto-electric induction upon, 261; on the production of, by heat, 345; polarized, theory of, on a, 365; chemical action of, 378; on the depolarization of, 469.
- Lightning, on the presence of sulphur in substances struck by, 222.
- Lime, solubility of the carbonate and phosphate of, in water containing carbonic acid, 297; on the dehydration of the sulphate of, 299.
- Linnæan Society, award of medals, 149.
- Louyet (M.) on anhydrous hydrofluoric acid, 382.
- Lubbock (Sir J. W.) on the meteor of September 25, 1846, 4.
- Luminous rays, on a theory of, 365.
- Luminous rings round shadows, observations on, 137.
- Machine for calculating the products, quotients, logarithms and powers of numbers, 171.
- Maddrell (R.) on the metaphosphates, 322.
- Mädler (Prof.) on the central sun, 140.
- Magnetism, on the effects of, upon the dimensions of iron and steel bars, 76, 226; on the universality of, 319; on the common nature of, 437, 502.
- Magnetometers, on the automatic registration of, 57, 127.
- Magnets, chemical action produced by, 264.
- Majocchi (Prof. G.) on the origin of the voltaic current, 97.
- Mellon and its compounds, researches on, 218.
- Mercury, on the purification of, 70; on the distillation of, 144; on the behaviour of zinc towards solutions of, 290.
- Metaphosphates, on the, 322.
- Metastannates, observations on the, 380.
- Meteor of September 25, 1846, account of the, 4.
- Meteorological instruments, on the automatic registration of, 57, 127.
- Meteorological observations, 71, 151, 223, 303, 383, 529; made in Brazil, 361.
- Methyle, on the sulphur compounds of, 65.
- Michelsen's (Dr.) historical survey of comets, 210.
- Millon (M.) on the purification of mercury, 70; on the distillation of mercury, 144; on the formation of sulphovinic acid, 211; on the dehydration of sulphate of lime under various circumstances, 299.
- Mineralogy:—examination of buralite, 65; analysis of white diopside, 68; native phosphates of copper, 69; description of chiolite, 70; villemite, 295; American columbite, 360.
- Morphia, composition of, 293.
- Moseley (Rev. H.) on a machine for calculating the products, quotients, logarithms, and powers of numbers, 171.
- Murchison (Sir R. I.) on the discovery of Silurian rocks in Cornwall, 336.
- Muspratt (Dr. S.) on the salts of sulphurous acid, 414.
- Neptune, observations relative to the new planet, 33, 130, 134, 181, 241.
- Nicholson (E. C.) on the compounds of phosphoric acid with aniline, 10.
- Nickles (M.) on the fermentation of



- tartaric acid and its products, butyro-acetic acid, [141](#).
- Niobic acid, on the occurrence of, in the North American [columbite](#), [361](#).
- Nitric acid, on the analysis of bodies containing, [426](#).
- Numbers, on a machine for calculating, [171](#).
- Oxygen, on the oxidizing power of, when disengaged by means of voltaic electricity, [334](#).
- Oxypicric acid, [148](#).
- Paget (Dr. G. E.) on some cases of morbid rhythmical movements, [129](#).
- Patterson's (R.) Introduction to Zoology, noticed, [57](#).
- Peligot (M.) on the composition of Bohemian glass, [147](#).
- Peter's comet, notice of, [61](#).
- Phosphate from urine, on the preparation of the ammoniaco-magnesian, [462](#).
- Phosphorocalcite, analysis of, [69](#).
- Phosphorus, on the excretion of, by the kidneys in an unoxidized state, [253](#); on the amount of, in various agricultural crops, [330](#).
- Plaster of Paris, on, [464](#).
- Playfair (Dr. Lyon) on the maximum density of water, [41](#).
- Plessy (M.) on plaster of Paris, [464](#).
- Polarized light, on the phenomena exhibited by, [465](#).
- Porrett (R.) on the chemical composition of gun-cotton, [273](#); on the existence of a new vegeto-alkali in gun-cotton, [409](#).
- Poumarede (M.) on the reduction of the persalts of iron by zinc, [149](#).
- Powell's (Rev. Prof.) note to a former paper on the theory of the aberration of light, [93](#); on luminous rings round shadows, [137](#).
- Problems, on the solution of certain, [28](#).
- Pypers (M.) on an easy method of preparing iodide of potassium, [146](#).
- Quaternions, on, [458](#).
- Quina, composition of, [293](#).
- Rammelsberg (M. C.) on chromate of chromium, [223](#).
- Ransome (T.) on the composition and explosion of gun-cotton, [1](#).
- Rays, on a peculiar electrical organ found in, [47](#).
- Reinsch (M.) on the ignition of brass wire in the vapour of alcohol, [143](#).
- Robin (M. le Dr. Ch.) on a peculiar organ found in the rays, [47](#).
- Rochleder (M.) on the formation of glycerine, [146](#).
- Ronalds (Dr. E.) on the extractive material of urine, and on the excretion of sulphur and phosphorus by the kidneys in an unoxidized state, [253](#).
- (F.) on photographic self-registering meteorological and magnetical instruments, [127](#).
- Rose (H.) on the behaviour of metallic zinc towards solutions of mercury, [290](#); on the acid contained in the North American [columbite](#), [360](#); on the examination of the ashes of organic bodies, [369](#).
- Rowney (T.) on the Bohemian glass as found in the combustion tubes employed in organic analysis, [421](#).
- Royal Astronomical Society, proceedings of the, [60](#), [130](#), [207](#).
- Royal Society, proceedings of the, [57](#), [125](#), [196](#).
- Sabine (Lieut.-Col. E.) on the lunar atmospheric tide at St. Helena, [206](#).
- Salts, action of bromine upon, [525](#).
- Schumacher (Prof.) on the reappearance of Astræa, [133](#).
- Selenaldine, observations on, [470](#).
- Sharp (A.), relics of the mechanical productions of, [25](#).
- Silicates containing chlorine, sulphuric and carbonic acid, [528](#).
- Silurian rocks in Cornwall, on the discovery of, [336](#).
- Smith (Dr. R. A.) on the air and water of towns, [478](#).
- Snow, on the formation of cylindrical masses of, in Orkney, [301](#).
- Soils, on the solvent action of drainage-water on, [30](#).
- Sorby (H. C.) on the amount of sulphur and phosphorus in various agricultural crops, [330](#).
- Southern (J.) on the density, latent heat and elasticity of steam, [113](#).
- Spectrum, on the analysis of the, by absorption, [461](#).
- Spencer (H.) on the form of the earth as no proof of original fluidity, [194](#).
- Squares, on a generalization of a theorem of Euler in reference to the products of the sums of, [424](#).
- Stannates, observations on the, [380](#).
- Stars, observations on double, [64](#);



- double, orbits of, [209](#); on Bessel's method for the computation of the corrections for the apparent places of, [208](#).
- Stas (M.) on the preparation, properties and composition of acetal, [220](#).
- Steam, on the density, latent heat and elasticity of, [113](#).
- Steel bars, on the effects of magnetism upon the dimensions of, [76](#).
- Styphnic acid, [148](#).
- Sulphovinic acid, on the formation of, [211](#).
- Sulphoxiphosvinic acid and its compounds, [382](#).
- Sulphur, on the presence of, in substances struck by lightning, [222](#); on the excretion of, by the kidneys in an unoxidized state, [253](#); amount of, in various agricultural crops, [330](#).
- Sulphurous acid, on the salts of, [414](#).
- Sun, on the existence and mechanism of the negative or projecting rays of the, [87](#).
- Tagilite, analysis of, [69](#).
- Tartaric acid, on the fermentation of, [141](#).
- Taste, on the mechanism of, [277](#).
- Telegraph, electric, on the induction of atmospheric electricity on the wires of the, [186](#).
- Teschmacher (E. F.) on gun-cotton, [258](#); on the chemical composition of gun-cotton, [273](#).
- Thialdine, observations on, [470](#).
- Thompson (L.) on the exhalation of bicarbonate of ammonia by the lungs, [124](#).
- Thomson (Dr.), notice of the late, [198](#).
- Trombolite, analysis of, [69](#).
- Uranus, an explanation of the observed irregularities in the motion of, [130](#).
- Urine, on the extractive material of the, [253](#); on the preparation of the ammoniaco-magnesian phosphate from, [462](#).
- Uterus, on the ganglia and nerves of the, [127](#).
- Vapour atmosphere, on the hourly alterations of the, at Bombay, [246](#).
- Verdeil (M.) on crystallized bile, [298](#).
- Villemitte, on the composition of, [295](#).
- Viscosity, on the common nature of, [437](#), [502](#).
- Vision, binocular, on the knowledge of distance given by, [305](#); inverted, on the conversion of relief by, [432](#).
- Voltaic arc, experiments and observations on the, [125](#).
- Voltaic battery, on a new and practical form of, [127](#).
- Voltaic current, on the origin of the, [97](#).
- Waller (Dr. A.) on hail, and on the inorganic bodies contained in hail-stones, [159](#); on the papillæ and nerves of the tongue of the frog, with observations on the mechanism of taste, [277](#).
- Warner (H. S.) on  $\sqrt{-1}$ , [185](#).
- Wartmann (Prof. E.) on induction, [261](#).
- Water, on the maximum density of, [41](#); on the decomposition of, by heat, [58](#).
- Waves, of the first order, mathematical theory of, [129](#); on certain atmospheric or barometric, [482](#).
- Weld (C. R.) on Newton's telescope at the Royal Society, [139](#).
- Whitney (M.) on silicates containing chlorine, sulphuric and carbonic acid, [528](#).
- Will (Dr.) on styphnic acid, [148](#).
- Wilson (J.) on the solvent action of drainage-water on soils, [30](#).
- Wittstein (M. G. C.) on the change effected in hydrated peroxide of iron when kept in water, [294](#).
- Wöhler (M.) on thialdine and seledaldine, two new artificial organic bases, [470](#).
- Wurtz (M. A.) on the compounds of cyanogen, [378](#).
- Young (J. R.) on a generalization of a theorem of Euler in reference to the products of the sums of squares, [424](#).
- Zeise (M.) on carotin, [523](#).

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SIR ROBERT KANE, M.D. M.R.I.A.

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"Nec araneorum sane textus ideo melior quia ex se fila gignunt, nec noster  
vilior quia ex alienis libamus ut apes." *Just. Lips. Polit. lib. i. cap. 1. Not.*

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**"Meditationis est perscrutari occulta; contemplationis est admirari  
perspicua . . . . . Admiratio generat quæstionem, quæstio investigationem,  
investigatio inventionem."—*Hugo de S. Victore.***

# CONTENTS OF VOL. XXXI.

(THIRD SERIES.)

## NUMBER CCV.—JULY 1847.

	Page
Sir J. Lubbock on the Perturbations of Planets moving in Eccentric and Inclined Orbits .....	1
Prof. Schœnbein on the Discovery of Gun-Cotton .....	7
The Rev. B. Bronwin on the Inverse Calculus of Definite Integrals .....	12
Mr. W. R. Grove on certain Phænomena of Voltaic Ignition and the Decomposition of Water into its constituent Gases by Heat .....	20
Mr. C. R. Weld on the Invention of Fluxions.....	35
Sir R. Kane's Researches on the Composition and Characters of certain Soils and Waters belonging to the Flax districts of Belgium, and on the Chemical Constitution of the Ashes of the Flax Plant .....	36
Dr. Schunck on the Colouring Matters of Madder .....	46
Comparative Analysis of the Urine of the Calf and the Sheep..	49
Mr. Hind on the expected Reappearance of the celebrated Comet of 1264 and 1556 .....	50
Messrs. G. Merck and R. Galloway's Analysis of the Water of the Thermal Spring of Bath (King's Bath) .....	56
Notices respecting New Books .....	67
Proceedings of the Royal Society .....	69
Action of Chlorine on Alcohol.—Formation of Acetal.....	77
Bisilicate of Iron or Ferruginous Pyroxene .....	78
Chlorosulphuret of Silicium .....	78
Meteorological Observations for May 1847 .....	79
Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at Chiswick, near London; by Mr. Veall at Boston; by the Rev. W. Dunbar at Applegarth Manse, Dumfries-shire; and by the Rev. C. Clouston at Sandwick Manse, Orkney.....	80

## NUMBER CCVI.—AUGUST.

The Rev. N. J. Callan on a new Voltaic Battery, cheap in its construction and use, and more powerful than any Battery yet made; and on a cheap substitute for the nitric acid of Grove's Platina Battery.....	81
Sir J. Lubbock on the Perturbations of Planets moving in Eccentric and Inclined Orbits ( <i>concluded</i> ) .....	86
Sir J. Lubbock on the Heat of Vapours .....	90

	Page
Mr. W. R. Grove on certain Phænomena of Voltaic Ignition and the Decomposition of Water into its constituent Gases by Heat ( <i>concluded</i> ) . . . . .	91
Mr. W. R. Grove's Supplementary Paper on certain Phænomena of Voltaic Ignition and the Decomposition of Water into its constituent Gases by Heat . . . . .	96
Sir D. Brewster on the Modification of the Doubly Refracting and Physical Structure of Topaz, by Elastic Forces emanating from Minute Cavities. (With a Plate). . . . .	101
Sir R. Kane's Researches on the Composition and Characters of certain Soils and Waters belonging to the Flax districts of Belgium, and on the Chemical Constitution of the Ashes of the Flax Plant ( <i>concluded</i> ). . . . .	105
Mr. J. P. Joule on the Theoretical Velocity of Sound . . . . .	114
Mr. E. C. Nicholson on the Composition of Caffein, and of some of its Compounds . . . . .	115
Prof. J. R. Young's Note in reference to the extension of Euler's Theorem. . . . .	123
Prof. A. Connell on the Precipitate produced in Spring and River Waters by Acetate of Lead. . . . .	124
Mr. J. Mercer on the Action of a mixture of Red Prussiate of Potash and Caustic Alkali upon Colouring Matters. . . . .	126
Dr. W. Gregory on the Preparation of Hippuric Acid. . . . .	127
Proceedings of the Cambridge Philosophical Society . . . . .	130
————— Royal Astronomical Society. . . . .	143
On a new Test for Prussic Acid, and on a simple method of preparing the Sulphocyanide of Ammonium, by Prof. Liebig . .	146
On the Fusion of Iridium and Rhodium, by R. Hare . . . . .	147
On Testing the Comparative Value of Astringent Substances for the purposes of Tanning, by Robert Warington, Esq. . . . .	150
On the two varieties of Arsenious Acid, by M. Bussy. . . . .	151
On the Preparation of Gun-Cotton . . . . .	152
On Balsam of Tolu, and some products derived from it . . . . .	153
On the Equivalent of Titanium, by M. Isidore Pierre. . . . .	155
On a modification of the Apparatus of Varrentrapp and Will for the estimation of Nitrogen, by Warren De la Rue, Esq. . . . .	156
On the Detection of Cotton in Linen, by G. C. Kindt. . . . .	157
The Planet Hebe . . . . .	158
Meteorological Observations for June 1847. . . . .	159
————— Table. . . . .	160

#### NUMBER CCVII.—SEPTEMBER.

Dr. T. Anderson on certain Products of Decomposition of the Fixed Oils in contact with Sulphur . . . . .	161
Mr. J. P. Joule on the Mechanical Equivalent of Heat, as determined by the Heat evolved by the Friction of Fluids. . .	173



	Page
Letter from Prof. Schœnbein to Prof. Faraday, F.R.S., on a new Test for Ozone .....	176
Dr. G. Wilson on the Decomposition of Water by Platinum and the Black Oxide of Iron at a white heat, with some observations on the theory of Mr. Grove's Experiments.....	177
Mr. J. J. Sylvester's account of a Discovery in the Theory of Numbers relative to the Equation $Ax^3 + By^3 + Cz^3 = Dxyz$ ..	189
Experiment made at the Kew Observatory on a new Kite-Apparatus for Meteorological Observations, or other purposes ..	191
Dr. L. Playfair on Transformations produced by Catalytic Bodies.....	192
Sir W. R. Hamilton on Quaternions; or on a New System of Imaginaries in Algebra ( <i>continued</i> ) .....	214
Notices respecting New Books .....	219
Proceedings of the Royal Society.....	222
Suggestions for the observation of the Annular Eclipse, Oct. 9, 1847, made by the British Association for the Advancement of Science, Oxford, June 26, 1847 .....	228
On the Preparation and Composition of the Salts of Antimony, by M. E. Peligot .....	230
Action of Hydrochloric Acid in the Formation of Oxalic Acid .....	233
Projection of Aldebaran on the Moon .....	233
The Puff Parliamentary:—Disinfection .....	233
A Grant of 200 <i>l.</i> to Mr. William Sturgeon .....	236
Observations on Creatine, by M. Heintz .....	236
The New Planet Iris .....	237
Suggestions for Promoting the Science of Meteorology .....	238
Meteorological Observations for July 1847 .....	239
————— Table.....	240

## NUMBER CCVIII.—OCTOBER.

Prof. E. Wartmann's Fourth Memoir on Induction. (With a Plate.) .....	241
Mr. S. M. Drach on eliminating the Signs in Star-Reductions..	251
Mr. J. Brown on the Molybdate of Lead. ....	253
Dr. R. D. Thomson's note on a new Test for Arseniates, &c... ..	258
Mr. E. W. Binney on Fossil <i>Calamites</i> found standing in an erect position in the Carboniferous Strata near Wigan, Lancashire .....	259
Mr. E. Frankland and Dr. H. Kolbe upon the Chemical Constitution of Metacetic Acid, and some other Bodies related to it .....	266
Messrs. T. H. Rowney and H. How's Analysis of the Ashes of the Orange-Tree ( <i>Citrus aurantium</i> ) .....	271
Sir W. R. Hamilton on Quaternions; or on a New System of Imaginaries in Algebra ( <i>continued</i> ) .....	278

	Page
Mr. J. J. Sylvester on the Equation in Numbers $Ax^3 + By^3 + Cz^3 = Dayz$ , and its associate system of Equations ( <i>continued</i> ) ..	293
Mr. R. Taylor on the Invention and First Introduction of Mr. Koenig's Printing Machine .....	297
Proceedings of the Cambridge Philosophical Society .....	301
On the Artificial Production of Minerals, and especially of Precious Stones .....	311
Analysis of Kupfernickel .....	314
On the Dehydration of Monohydrated Sulphuric Acid .....	314
Observations on Silica, by M. Doveri .....	315
On Nitric Mannite, by M. Sobrero .....	316
On the Extraction of Silver, by MM. Malaguti and Durocher ..	317
Vanadate of Lead and Copper .....	319
Meteorological Observations for August 1847 .....	319
————— Table .....	320

#### NUMBER CCIX.—NOVEMBER.

Prof. M. A. De la Rive's Researches on the Voltaic Arc, and on the influence which Magnetism exerts both on this Arc and on bodies transmitting interrupted Electric Currents ..	321
Mr. T. Richardson's Analyses of the Ashes of Rough Brown Sugar and Molasses .....	336
Letter from Prof. Loomis of the New York University to Lieut.-Colonel Sabine, Foreign Secretary of the Royal Society, on the determination of differences of Longitude made in the United States by means of the Electric Telegraph, and on projected observations for investigating the Laws of the great North American Storms .....	338
The Rev. B. Bronwin on the Algebraic Equation of the Fifth Degree .....	341
Letter from Capt. J. H. Lefroy, R.A., Director of the Magnetic Observatory of Toronto in Canada, to Lieut.-Colonel Sabine, R.A., on a great Magnetic Disturbance on the 24th of September 1847 .....	346
Dr. H. Kolbe on the Decomposition of Valerianic Acid by the Voltaic Current .....	348
Mr. R. Adie's Account of Experiments with Galvanic Couples immersed in pure water and in oxygenated water .....	350
Dr. R. Hare on certain Improvements in the Construction and Supply of the Hydro-Oxygen Blowpipe, by which Platinum may be fused in the large way .....	356
Dr. J. W. Griffith on the Composition of the Bile of the Sheep ..	366
The Rev. J. Slatter's Notice respecting the Meteor of September 25, 1846 .....	368
Mr. J. Glaisher on the Aurora Borealis, as it was seen on Sunday evening, October 24, 1847, at Blackheath .....	369

	Page
Proceedings of the Royal Society.....	372
———— Cambridge Philosophical Society.....	376
———— Royal Astronomical Society .....	380
On the Gelatinous Substances of Vegetables .....	389
Preparation of Protoxide of Tin .....	392
On the Presence of Arsenic, Copper and Tin, in the Mineral Waters of Bavaria .....	392
Solubility of Common Salt in Alcohol .....	393
On some Improved Forms of Chemical Apparatus, by Thomas Taylor, Esq. ....	393
Preparation and Composition of Lignin .....	397
Solubility of Chloride of Silver in Hydrochloric Acid .....	398
Daubeny on Active and Extinct Volcanos .....	399
Meteorological Observations for September 1847 .....	399
———— Table.....	400

NUMBER CCX.—DECEMBER.

Prof. M. Faraday on the Diamagnetic conditions of Flame and Gases .....	401
Prof. Zantedeschi on the Motions presented by Flame when under the Electro-Magnetic Influence .....	421
Mr. T. Weddle on Asymptotic Straight Lines, Planes, Cones and Cylinders to Algebraical Surfaces .....	425
Mr. R. A. Couper on the Chemical Composition of the Sub- stances employed in Pottery .....	435
Sir D. Brewster on the Polarization of the Atmosphere .....	444
Mr. A. Smith on the Hydrates of Nitric Acid.....	454
Mr. F. Field on the Products of the Decomposition of Cuminate of Ammonia by Heat.....	459
Mr. J. J. Sylvester on the General Solution (in certain cases) of the equation $x^3 + y^3 + Az^3 = Mxyz$ , &c.....	467
Mr. W. De la Rue on Cochineal ( <i>Coccus Cacti</i> ). First Memoir	471

NUMBER CCXI.—SUPPLEMENT TO VOL. XXXI.

Mr. W. De la Rue on Cochineal ( <i>Coccus Cacti</i> ). First Memoir (concluded).....	481
Sir D. Brewster on the Existence of Crystals with different pri- mitive forms and physical properties in the Cavities of Mine- rals; with additional Observations on the New Fluids in which they occur. (With a Plate.) .....	497
Mr. L. Thompson's Observations on Chloric Acid and the Chlorates .....	510

	Page
Prof. Sir W. R. Hamilton on Quaternions ; or on a New System of Imaginaries in Algebra ( <i>continued</i> ) . . . . .	511
Mr. J. H. Gladstone's Contributions to the Chemical History of Gun-Cotton and Xyloidine . . . . .	519
Proceedings of the Royal Astronomical Society . . . . .	528
On Osmiamic Acid, by MM. J. Fritzsche and H. Struve . . . .	534
On the Preparation and Properties of some Osmiamates, by MM. Fritzsche and Struve . . . . .	535
On Sulphato-chloride of Copper,—a New Mineral, by Arthur Connell, Esq. . . . .	537
On the Formation of Valerianic Acid, by M. Therault . . . . .	538
Note on the Measurement of the double Sulphates of Zinc and Soda, and of Magnesia and Soda, by W. H. Miller. . . . .	540
Native Carbonate of Nickel . . . . .	541
An Examination and Analysis of the "Nadelerz," or needle ore of Bismuth, by E. J. Chapman, Esq. . . . .	541
Action of Anhydrous Phosphoric Acid on Ammoniacal Salts, by M. Dumas . . . . .	544
Meteorological Observations for October 1847 . . . . .	545
————— Table . . . . .	546
Index . . . . .	547

## PLATES.

- I. Illustrative of Sir D. Brewster's Paper on the Modification of the Doubly Refracting and Physical Structure of Topaz, by Elastic Forces emanating from Minute Cavities.
- II. Illustrative of Prof. Wartmann's Fourth Memoir on Induction.
- III. Illustrative of Sir D. Brewster's Paper on the Existence of Crystals with different primitive forms and physical properties in the Cavities of Minerals.

*Erratum* in Mr. SYLVESTER's paper, p. 189

Line 14, for *D'xyz* read *D'uvw*.

*Errata* in Sir GRAVES C. HAUGHTON's paper, vol. xxx. p. 437.

- P. 445, in the thirteenth line from the bottom, for 1·1024 read 1,1024.  
 — — ninth line from the bottom, for 256° read 256.  
 — 456, in the third line from the bottom, for Hare hair read Horse hair.  
 — 518, in the thirteenth line from the bottom, for oxide of hydrogen read protoxide of hydrogen.  
 — 522, in the fifteenth line from the top, for in fault read at fault.



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- I. *On the Perturbations of Planets moving in Eccentric and Inclined Orbits.* By Sir J. LUBBOCK, Bart., F.R.S.\*

THE accuracy of the tables which give for an indefinite time the places of the older planets, is at present sufficient for the purposes of astronomy, and is commensurate with the accuracy of observation; or if this statement appears to be exaggerated, it will at least be admitted that the sensible errors which remain are owing rather to inadvertence in the numerical computation than to the imperfection of the method itself. Such a result is owing to the uninterrupted labours of the greatest mathematicians from the time of Newton, and is justly regarded as one of the greatest triumphs of human intelligence. But it must be recollected that these methods, by which the perturbations of the older planets have been obtained, are applicable only to the case of orbits nearly circular, and little inclined to each other; so that the general solution of the problem of the three bodies, as it is called, remains to the present day imperfect. The methods in use for the older planets are founded, as is well known, upon the development of the disturbing function in terms of the mean anomalies. M. Binet has indeed carried this development to quantities of the seventh order inclusive; but such a development is quite insufficient in the case of comets or planets moving in highly eccentric and inclined orbits, which problem presents far greater difficulty; while the nature of the expression is such, that it is evidently impracticable to carry further such a mode of development, even if the expressions were sufficiently convergent when the eccentricity passes a certain limit.

The only memoir with which I am acquainted which pro-

\* Communicated by the Author.

fesses to give a general solution of the problem otherwise than by mechanical quadratures, is due to M. Hansen. This important work is translated in the *Conn. des Temps* for 1847. That great mathematician has considered the case when  $r < r'$ , that is, when the disturbed body is inferior; and has illustrated the question by the numerical calculation of the perturbations of the comet of Encke by Saturn. M. Hansen develops the disturbing function according to multiple angles of the eccentric anomaly of the disturbed planet *literally*; and first, according to multiple angles of the true anomaly of the disturbing planet; M. Hansen next converts the cosines and sines of the multiple angles of the true anomaly of the disturbing planet into sines and cosines of multiple angles of the mean anomaly of that planet; so that finally the disturbing function is exhibited in terms of the eccentric anomaly of the disturbed planet and the mean anomaly of the disturbing planet; but those series which serve to give the sines and cosines of the multiples of the true anomaly, in terms of sines and cosines of the mean anomaly, are not very convergent; and the process becomes extremely laborious, even in the case which M. Hansen has considered, in which, in consequence of the great distance of Saturn, the approximation does not require to be carried nearly so far as in the case of the perturbations of the same comet by Jupiter, and in many others which may require consideration. Moreover, in this as in every other mode which can be devised of developing the disturbing function *literally*, all quantities must be retained of a given order; although when they are of a different sign, in many instances they destroy each other; but such reductions cannot be foreseen. The numerical substitutions are also extremely laborious, in consequence of the multitude of terms which have to be considered.

As the disturbing function, and others which require to be integrated, are finally exhibited by M. Hansen in terms of two variables, such that direct integration is impossible, M. Hansen has recourse to the integration *par parties*, in which each term by integration gives rise to a series of other terms, the nature of which is complicated.

The method which I propose differs from that suggested by M. Hansen in every particular. Instead of attempting a *literal* development, I insert the numerical values of the elliptic constants in the earliest possible stage: by this means the radical, which expresses the mutual distance of the planets, is explicitly a function of sines and cosines of various angles with numerical coefficients. When  $r < r'$ , I develop in terms of the eccentric anomaly of  $m$ , after having obtained expres-

sions for the co-ordinates of  $m'$  in terms of the eccentric anomaly of  $m$ . Such expressions are very easy to obtain, and are very convergent. It will be recollected that before I endeavoured to develop the disturbing function in the lunar theory in terms of the mean motions of the sun and moon, the inviolable practice had been (see *Mécanique Céleste*, vol. iii. p. 189) to express the co-ordinates of the sun in terms of the true longitude of the moon; but the equation which connects the eccentric anomalies of two bodies is far simpler than that which connects the true anomalies, or  $v'$  and  $v$ , and therefore the conversion which I employ is made with greater facility. The quantity under the radical sign in  $R$  may thus be considered as a function, of which the general term can be represented by

$$a \frac{\sin}{\cos} \left( i v + i' \frac{n'}{n} v \right),$$

$a$  being a numerical quantity. The development of this quantity to the power  $-\frac{1}{2}$  or  $-\frac{3}{2}$ , may be facilitated by the use of tables, which give the numerical coefficients in the development of  $\{1 - A \cos \alpha\}^{-\frac{1}{2}}$ ,  $\{1 - A \cos \alpha\}^{-\frac{3}{2}}$ , &c. Such tables have been calculated for me by Mr. Farley. By proceeding in this way, no term is ever introduced which affects the final result beyond a given place of decimals. For the development of the radical admits of being exhibited in the form

$$A + B + C + D + \&c.;$$

such that

$$B = \alpha A \mathcal{Q}, \quad C = \beta B \mathcal{Q}, \quad D = \gamma C \mathcal{Q},$$

so that each term is deducible from the one which precedes it, by the multiplication of that term by  $\alpha \mathcal{Q}$ ,  $\beta \mathcal{Q}$ , &c.,  $\alpha$ ,  $\beta$ ,  $\gamma$ , &c. being proper fractions. If therefore the terms in the two quantities which form those products, such, for instance, as

$$\alpha A \text{ and } \mathcal{Q}$$

which form  $B$ , are sorted and arranged in the order of their numerical magnitude, as soon as any one partial product sinks below any limit that may be assigned, all the succeeding terms are necessarily of inferior magnitude; and the approximation stops, as it were, of itself, without any exercise of thought on the part of the computer.

When  $r > r'$ , that is, when the planet disturbed is superior to the disturbing planet, I am not able to suggest any other course than to develop in terms of the true anomaly of the disturbed planet, and the mean anomaly of the disturbing planet,

and to integrate *par parties*. I have obtained the law of the coefficients in the series which results in this process, and they are highly convergent. I am confident that, by the processes which I have attempted thus so briefly to describe, the perturbations of planets moving in orbits, however eccentric and inclined, may be calculated with nearly as great facility as they are given by existing methods, in orbits nearly circular and in the same plane, and may be exhibited in tables, giving their values for an indefinite period, if required. If these methods, which I have described in detail elsewhere, possess the advantages which I ascribe to them, I hope the time is not distant when the perturbations of Pallas and of some of the comets may be reduced to a tabular form; but as the labour will be very considerable, it will be necessary to limit the inquiry in the commencement to the cases of the greatest emergency.

Although my methods are specially adapted to the determination of the perturbations of bodies moving in eccentric orbits which cannot be developed in terms of the mean motions, yet they embrace also the case of a planet moving in an orbit nearly circular; and it is easy to show in what manner the labour is increased by the greater eccentricity. If the reciprocal of the radical which expresses the mutual distance of the planets be called

$$\frac{\eta}{r} \{1 + P\}^{-\frac{1}{2}}, \quad \eta = \frac{a}{\sqrt{a^2 + a'^2}},$$

the chief difficulty arises in developing  $\{1 + P\}^{-\frac{1}{2}}$ . If the numerical values of the elliptic constants are introduced,

$$1 + P = 1 - A_1 \cos \alpha_1 - A_2 \cos \alpha_2 + \&c.,$$

$A_1, A_2, \&c.$  are numerical coefficients, which I here suppose ranged in the order of their numerical magnitude. I make

$\{1 - A_1 \cos \alpha_1\} \{1 - A_2 \cos \alpha_2\} \dots \{1 - A_i \cos \alpha_i\} = 1 + P + Q,$   
including a limited number of terms in  $1 + P + Q$ .

$$\{1 - A_1 \cos \alpha_1\}^{-\frac{1}{2}}, \quad \{1 - A_2 \cos \alpha_2\}^{-\frac{1}{2}} \dots \&c.,$$

can be obtained at once by means of a table. But as the coefficients given by such a table do not readily furnish, by interpolation, the values required unless it be considerably extended, I take for  $A_1, A_2, \&c.$  the nearest value given by the table, and I leave the residue to form part of  $Q$ . In this way it will generally be found sufficient to include not more than six terms in  $1 + P + Q$ , so as to leave  $Q$  consisting of



terms of which the coefficients are each below .1 in numerical value, and the quantity  $\{1 + P\}^{-\frac{3}{2}}$  can be developed according to powers of  $Q$  in a rapidly converging series.

$$1 + P = 1 - \frac{2\eta^2}{\sigma} \frac{a'}{r'} \frac{r}{a} \cos \delta + \eta^2 \rho.$$

$$\frac{2\eta^2}{\sigma} = \frac{2aa'}{(a^2 + a'^2)}, \quad \delta \text{ is the angle included by lines drawn from the sun to } m \text{ and } m', \quad \rho = \frac{a'^2}{r'^2} \frac{r^2}{a^2} - 1.$$

The terms contained in  $\frac{2\eta^2}{\sigma} \frac{a'}{r'} \frac{r}{a} \cos \delta$  obviously exceed greatly in magnitude those contained in  $\eta^2 \rho$ ,

$$\frac{r}{a} \cos \delta = i \cos f' + k \sin f';$$

$f'$  being the true anomaly of  $m'$ , and  $v$  the eccentric anomaly of  $m$ ,

$$i = -e \mathfrak{A} + \mathfrak{A} \cos v - \sqrt{1 - e^2} \mathfrak{B} \sin v,$$

$$k = -e \mathfrak{C} + \mathfrak{C} \cos v + \sqrt{1 - e^2} \mathfrak{D} \sin v;$$

$\mathfrak{A}$ ,  $\mathfrak{B}$ ,  $\mathfrak{C}$  and  $\mathfrak{D}$  being constants, each necessarily less than unity, which depend only on the inclination of the orbits and the position of their line of intersection, and such that when they are in the same plane

$$\mathfrak{A} = \mathfrak{D} \text{ and } \mathfrak{B} = \mathfrak{C}.$$

The process is precisely the same in substance, whether the orbit of  $m$  is highly eccentric and inclined, or circular, and in the same plane with that of  $m'$ ; the only difference being that, while in the former case it may be necessary to detach as many as six terms to form the quantity  $1 + P + Q$ , in order that  $Q$  may not contain any term of which the numerical coefficient exceeds .1 in magnitude; in the latter case, supposing  $e'$  the eccentricity of  $m'$  to be inconsiderable,  $1 + P + Q$  will only contain one factor, and therefore  $\{1 + P + Q\}^{-\frac{3}{2}}$ , &c. can be calculated with greater facility. Thus, for example, in the perturbations of

Pallas by Saturn, it is convenient that  $1 + P + Q$  should contain three terms.

Pallas by Jupiter, it is convenient that  $1 + P + Q$  should contain four terms.

Encke's comet by Saturn, it is convenient that  $1 + P + Q$  should contain five terms.

Encke's comet by Jupiter, it is convenient that  $1 + P + Q$  should contain six terms.

And then in each of these examples  $Q$  will contain no term of which the coefficient exceeds  $\cdot 1$  in numerical amount.

If 
$$\frac{Q}{1+P+Q} = Q,$$

$$\{1+P\}^{-\frac{3}{2}} = A+B+C+D,$$

$$A = (1+P+Q)^{-\frac{3}{2}}, \quad B = \frac{3}{2} Q A, \quad C = \frac{5}{4} Q B,$$

$$D = \frac{7}{6} Q C, \quad E = \frac{9}{8} Q D, \text{ \&c.}$$

The calculation of  $\{1+P+Q\}^{-1}$  is much facilitated by the use of a table which gives the values of the coefficients of

$$\{1-A \cos \alpha\}^{-1}.$$

$\frac{\eta}{r}$  and  $1+P$  contain terms multiplied by  $\frac{a'}{r'} \cos f'$ ,  $\frac{a'}{r'} \sin f'$ ,  $\frac{a'}{r'}$ , and  $\frac{a'^2}{r'^2}$ , and none others require consideration. If the eccentricity of  $m'$  is small, they may be developed in terms of  $\xi'$ , the mean anomaly of  $m'$ ; and it will be sufficient to consider the terms depending on

$$\cos \xi', \cos 2\xi', \cos 3\xi', \sin \xi', \sin 2\xi', \sin 3\xi'.$$

If  $t$  is the time reckoned from the time of the perihelion passage of  $m$ ,  $r < r'$ ,

$$nt = v - e \sin v;$$

and if  $360^\circ - n'c'$  is the mean anomaly of  $m'$  at the time of the perihelion passage of  $m$ ,

$$\xi' = n't - n'c';$$

and if

$$\frac{n'}{n} v - n'c' = n,$$

$$\cos i \xi' = \cos i \left\{ n - \frac{n'}{n} e \sin v \right\},$$

$$\sin i \xi' = \sin i \left\{ n - \frac{n'}{n} e \sin v \right\};$$

and as  $\frac{n'}{n}$  is a fraction,  $\cos i \xi'$  and  $\sin i \xi'$  can be developed in a series rapidly converging, and containing explicitly only the variable quantity  $v$ .

[To be continued.]

II. *On the Discovery of Gun-Cotton.*

By Professor SCHÖENBEIN\*.

THE substance to which I have given in German the name of *schliesswolle*, and in English that of gun-cotton, having excited a lively curiosity, it may be interesting to the scientific world to become acquainted with some details of the way in which I was first led to its discovery.

The results of my researches on ozone led me in the course of the last two years to turn my attention particularly to the oxides of nitrogen, and principally to nitric acid. The numerous experiments I have made on this subject have led me, as I have stated in detail in Poggendorff's *Annalen*, to adopt a peculiar hypothesis on the so-called hydrates of nitric acid, sulphuric acid, &c., as well as on the normal nitrates, sulphates, &c.

For a long time I had entertained doubts as to the existence of compound bodies of this nature, which cannot be isolated, and which are stated to be capable of existing only in combination with certain other substances; for a long time also I had come to the notion that the introduction of these imaginary combinations had only been an apparent progress in theoretical chemistry, and that it had even impeded its development.

It is well known that what has most contributed to the admission of the existence of these compounds has been the opinion generally received among chemists respecting the nature of nitric acid. Starting from the existence of the compound of nitrogen  $\text{NO}_5$ , as an undoubted and demonstrated fact, notwithstanding the impossibility of isolating it, they always cite nitric acid to prove the existence of compounds which cannot exist in an isolated state. In my opinion, there is no degree of oxidation which is represented by  $\text{NO}_5$ , and what these chemists designate by the formula  $\text{NO}_5 + \text{HO}$  must be considered as being really  $\text{NO}_4 + \text{HO}_2$ ; I am even inclined to regard the normal nitrates  $\text{NO}_5 + \text{RO}$ , as compounds which must be expressed by  $\text{NO}_4 + \text{RO}_2$ . Amongst other motives which induce me to admit this opinion, I will mention the fact that we can obtain hydrated nitric acid or a normal nitrate by the direct mixture of  $\text{NO}_4$  with  $\text{HO}_2$  or  $\text{RO}_2$ . Other considerations, which I have had occasion to detail elsewhere, induce me also to consider hydrated sulphuric acid to have the form  $\text{SO}_2 + \text{HO}_2$ , and not that of  $\text{SO}_3 + \text{HO}$ , and a normal sulphate that of  $\text{SO}_2 + \text{RO}_2$ . It is sufficient here to observe that  $\text{SO}_2$  placed in presence of  $\text{HO}_2$  gives rise to what is

\* From the *Archives des Sciences Physiques et Naturelles*.

called hydrated sulphuric acid, and that  $\text{SO}_2$  placed in presence of  $\text{BaO}_2$  or  $\text{PbO}_2$  gives rise to what is called sulphate of the oxide of barium or of lead. Rose's compound, to which the formula  $2\text{SO}_3 + \text{NO}_2$  has been assigned, should have, in my opinion,  $2\text{SO}_2 + \text{NO}_4$ . Admitting this, I considered it probable that the mixture of  $2(\text{SO}_2 + \text{HO}_2)$  ( $= 2(\text{SO}_3 + \text{HO})$ ) with  $\text{NO}_4 + \text{HO}_2$  ( $= \text{NO}_5 + \text{HO}$ ) yields  $2\text{SO}_2 + \text{NO}_4$ , and that at the same time  $3\text{HO}_2$  is disengaged, or enters into a loose combination with what is called the bisulphate of deutoxide of nitrogen. In other words, I conjectured that a mixture formed with the hydrates of nitric acid and sulphuric acid would possess a very great power of oxidation, and would form a kind of *aqua regia*, in which the combination  $\text{HO}_2$  would act the part of the chlorine. On this hypothesis, and abstracting  $\text{HO}_2$  from the acid mixture by means of a proper oxidable body, there ought to remain Rose's compound.

Guided by these suppositions, which, I admit, may be as little founded as they are contrary to the ideas received among chemists, I commenced in December 1845 a series of experiments with a view to put my hypothesis to the proof: it will be seen in the sequel whether the results at which I arrived tend to confirm it.

I mixed some flowers of sulphur and a certain quantity of the acid mixture of which I have spoken: immediately, even at the temperature of  $32^\circ \text{F.}$ , a lively disengagement of sulphurous acid gas took place without the production of deutoxide of nitrogen. After the reaction, which was accompanied by a development of heat, there remained a colourless liquid, which, mixed with water, disengaged a considerable quantity of deutoxide of nitrogen, and acted generally as a solution of Rose's compound in hydrated sulphuric acid would have done.

I should add here, that a mixture of four ounces of hydrated sulphuric acid with a single drop of nitric acid, on the addition of flowers of sulphur, disengages a sensible quantity of sulphurous acid. To assure himself of the presence of the latter, the operator has only to hold over the liquid a strip of paper which has been covered with iodide of potassium paste, and tinged slightly blue by exposure to chlorine. The liberated sulphurous acid will soon dissipate this blue colour.

Selenium and phosphorus are oxidized in the same manner at low temperatures in the acid mixture in question; and this latter is modified to such an extent, that, on the addition of water, an abundant disengagement of deutoxide of nitrogen gas takes place.

Iodine even, in the state of powder and shaken up with the



acid mixture, rapidly absorbs oxygen, when exposed to a low temperature; and there is formed, besides iodic acid, the compounds to which Millon has lately drawn attention. After the reaction a liquid remains, which, diluted with water, gives an abundant disengagement of deutoxide of nitrogen and liberates iodine.

My experiments on ozone having shown that this body, which I consider to be a distinct peroxide of hydrogen, forms, as well as chlorine, at the ordinary temperature, a peculiar compound with olefiant gas, without apparently oxidizing in the least either the hydrogen or the carbon of this gas, I had the idea that it would not be impossible that certain organic matters, exposed to a low temperature, would likewise form compounds, either with the peroxide of hydrogen alone, which, on my hypothesis, occurs in a state of combination or of mixture in the acid mixture, or with  $\text{NO}_4$ . It was this conjecture, doubtless very singular in the eyes of chemists, which principally led me to commence experiments with common sugar.

I made a mixture of one part (volume) of nitric acid, of 1.5 spec. grav., and two parts of sulphuric acid of 1.85, at the temperature of  $36^\circ \text{F.}$ ; I then added some finely powdered sugar, so as to form a very fluid paste. I stirred the whole, and, at the end of a few minutes, the saccharine substance formed itself into a viscous mass entirely separated from the acid liquid, without any disengagement of gas. This pasty mass was washed with boiling water, until this last no longer exercised any acid reaction; after which I deprived it, as much as possible, at a low temperature, of the water it still contained. The substance now possessed the following properties:—Exposed to a low temperature, it is compact and brittle; at a moderate temperature, it may be moulded like jalap resin, which gives it a beautiful silky lustre. It is semi-fluid at the temperature of boiling water; at a higher temperature, it gives off red vapours; heated still more, it suddenly deflagrates with violence, without leaving any perceptible residue. It is almost insipid and colourless, transparent like the resins, almost insoluble in water, but easily soluble in the essential oils, in æther and concentrated nitric acid, and in most cases it acts in general like the resins in a chemical and physical point of view: thus friction renders it very electro-negative. I will add, that the acid mixture, by means of which this resinous body was obtained, has an extremely marked bitter taste.

I wished to make experiments also with other organic substances; and I soon discovered, one after another, all those about which there has been so much said of late, especially in the Academy of Paris. All this passed in December 1845,

and the first few months in 1846. In March, I sent specimens of my new compounds to some of my friends, in particular to Messrs. Faraday, Herschel and Grove. It is necessary to note expressly that the gun-cotton formed part of these products; but I must add, that hardly was it discovered when I employed it in experiments of shooting, the success of which encouraged me to continue them. Accepting the obliging invitation which I received, I went in the middle of April to Wurtemberg, and made experiments with gun-cotton both in the arsenal of Ludwigsburg, in the presence of artillery officers, and in Stuttgart, before the king himself. In the course of May, June and July, with the kind cooperation of the Commandant de Mechel, of M. Burkhardt, captain of artillery, and other officers, I subsequently made in this city (Bâle) numerous experiments with arms of small calibre, such as pistols, carbines, &c., and afterwards with mortars and cannon,—experiments at which Baron de Krüdener, the Russian ambassador, was several times present. I may be allowed to mention, that I was the person who fired the first cannon loaded with gun-cotton and shot, on the 28th of July, if I remember aright, after we had previously ascertained, by experiments with mortars, that the substance in question was capable of being used with pieces of large calibre.

About the same time, and indeed previously, I employed gun-cotton to blast some rocks at Istein in the Grand Duchy of Baden, and to blow up some old walls at Bâle; and in both cases I had opportunities of convincing myself in the most satisfactory manner, of the superiority of this new explosive substance over common gunpowder\*.

Experiments of this kind, which took place frequently and in the presence of a great number of persons, could not long remain unknown; and the public journals soon gave, without participation on my part, descriptions, more or less accurate, of the results which I had obtained. This circumstance, joined to the short notice which I inserted in the May number of Pogendorff's *Annalen*, could not fail to attract the attention of German chemists: in the middle of August I received from M. Bœttger, Professor at Frankfort, the news that he had succeeded in preparing gun-cotton and other substances. Our two names thus became associated in the discovery of the substance in question. To M. Bœttger the gun-cotton must have been particularly interesting, as he had previously discovered an organic acid which deflagrates readily.

In the month of August I went to England, where, assisted

\* In the month of June I made also the first capsules, and employed them with success for muskets, in the presence of the above-named officers.

by the able engineer, Mr. Richard Taylor of Falmouth, I made numerous experiments in the mines of Cornwall, which were entirely successful, in the opinion of all competent witnesses. Experiments on the action of gun-cotton were also made in several parts of England, under my direction, both with small fire-arms and with pieces of artillery, and the results obtained were very satisfactory.

Until that time there had been little or nothing said of gun-cotton in France; and it will appear that the short notices which Mr. Grove gave at Southampton at the meeting of the British Association, and the experiments with which he accompanied them, served first to attract the attention of French chemists to this substance. At Paris, the thing was at first considered hardly credible, and jokes even were passed upon it; but when there could no longer remain any doubt as to the reality of the discovery, and when several chemists in Germany and other countries had published the processes which they employed to prepare the gun-cotton, then a lively interest was manifested in a subject which had just before excited derision, and it was soon pretended that the new explosive substance was an old French discovery. It was declared to be nothing more than the xyloïdine first discovered by M. Braconnot, and afterwards investigated anew by M. Pelouze, and the only merit left me was to have conceived the happy idea of putting this substance into a gun-barrel. The knowledge of the composition of xyloïdine ought to have sufficed to convince those who put forward that opinion, that it is not suited for fire-arms, on account of its containing too much carbon and too little oxygen for the chief part to be converted into gaseous matters during the combustion. It was moreover very easy to discover the essential differences which exist between the xyloïdine of Braconnot and gun-cotton. Nevertheless the error was kept up for some months.

Matters stood thus, when, on the 4th of last November, a Scotch chemist, Mr. Walter Crum of Glasgow, published a memoir, in which he showed that gun-cotton is not the same product as xyloïdine, but that it presents an essentially different composition; and towards the end of the same month, the French Academy received a communication of the same nature. The gun-cotton was then no longer xyloïdine, it was called pyroxyloïdine, and the first was admitted to be unsuitable for fire-arms.

If, therefore, it is proved that from the commencement of 1846 I prepared gun-cotton, and applied it to the discharge of fire-arms, and that M. Bœttger did the same in the month of August,—if it be admitted that xyloïdine cannot serve the

same purposes as this cotton, and if it be notoriously known that what is now called pyroxyloidine was not brought before the French Academy and the scientific world until towards the middle of last November, the idea of attributing to France the discovery of gun-cotton cannot be seriously entertained, or of assigning to me merely a practical application of that which another would have discovered.

I appeal to the justice of Frenchmen, to decide the point to whom belongs the honour of not only being the first to apply the new substance in question, but also of having first prepared it—to MM. Braconnot and Pelouze, or myself. I must, moreover, add expressly, that it was not xyloidine even which led to my discovery, however intimate may be its relation with gun-cotton; it was theoretical ideas, possibly very erroneous ones, but which are peculiarly my own, as well as some facts which I was also the first to discover. *Suum cuique* is a principle of morality on which society at large rests; why should it not be strictly respected in the republic of science? M. Pelouze is a distinguished chemist, and already possesses a sufficiently high reputation not to require to elevate his pretensions on the merits of others; and I am fully persuaded that this estimable chemist, of well-known truth of character, will, appreciating with impartiality the circumstances which have occurred, freely render me the justice to which I consider myself entitled.

Bâle, Dec. 28, 1846.

### III. On the *Inverse Calculus of Definite Integrals.*

By the Rev. BRICE BRONWIN\*.

THIS paper contains several very simple and easy methods in the inverse calculus of definite integrals; and they show that the function under the sign of integration may have more than one form. The exponents  $n$  and  $p$  are always positive, and  $n + p = i$  an integer.

First, let  $\phi(x) = \Sigma A_m x^m$ , an ascending series. Then

$$\begin{aligned} \int_0^a x^{n-1} dx \phi(a-x) &= \Sigma A_m \int_0^a x^{n-1} dx (a-x)^m \\ &= \Sigma A_m a^{m+n} \int_0^1 x^{n-1} dx (1-x)^m \\ &= \Gamma(n) \Sigma A_m a^{m+n} \frac{\Gamma(m+1)}{\Gamma(m+n+1)} = \psi(a) \text{ suppose.} \end{aligned}$$

\* Communicated by the Author.



Then also

$$\Gamma(n) \Sigma A_m (a-x)^{m+n} \frac{\Gamma(m+1)}{\Gamma(m+n+1)} = \psi(a-x);$$

and

$$\begin{aligned} \Gamma(n) \Sigma A_m \frac{\Gamma(m+1)}{\Gamma(m+n+1)} \int_0^a x^{p-1} dx (a-x)^{m+n} \\ = \int_0^a x^{p-1} dx \psi(a-x), \end{aligned}$$

or

$$\Gamma(n) \Gamma(p) \Sigma A_m a^{m+i} \frac{\Gamma(m+1)}{\Gamma(m+i+1)} = \int_0^a x^{p-1} dx \psi(a-x).$$

Operate with  $\left(\frac{d}{da}\right)^i$  on both members, and we have

$$\begin{aligned} \Gamma(n) \Gamma(p) \Sigma A_m a^m = \Gamma(n) \Gamma(p) \phi(a) = \left(\frac{d}{da}\right)^i \int_0^a x^{p-1} dx \psi(a-x) \\ = \left(\frac{d}{da}\right)^i a^p \int_0^1 dv (1-v)^{p-1} \psi(av), \end{aligned}$$

by making  $a-x=av$ . Therefore

$$\left. \begin{aligned} \int_0^a x^{p-1} dx \phi(a-x) = \psi(a), \quad \phi(a) = \frac{1}{\Gamma(n) \Gamma(p)} \\ \left(\frac{d}{da}\right)^i a^p \int_0^1 dv (1-v)^{p-1} \psi(av). \end{aligned} \right\} \dots \dots (1.)$$

Next, let  $\phi(x) = \Sigma \frac{A_m}{x^m}$ , a descending series. Then

$$\phi(a+x) = \Sigma \frac{A_m}{(a+x)^m},$$

and

$$\begin{aligned} \int_0^\infty x^{n-1} dx \phi(a+x) &= \Sigma A_m \int_0^\infty \frac{x^{n-1} dx}{(a+x)^m} \\ &= \Sigma \frac{A_m}{a^{m-n}} \int_0^\infty \frac{x^{n-1} dx}{(1+x)^m} = \Gamma(n) \Sigma \frac{A_m}{a^{m-n}} \frac{\Gamma(m-n)}{\Gamma(m)} = \psi(a) \end{aligned}$$

suppose. Therefore

$$\Gamma(n) \Sigma \frac{A_m}{(a+x)^{m-n}} \frac{\Gamma(m-n)}{\Gamma(m)} = \psi(a+x);$$

and

$$\Gamma(n) \Sigma A_m \frac{\Gamma(m-n)}{\Gamma(m)} \int_0^\infty \frac{x^{p-1} dx}{(a+x)^{m-n}} = \int_0^\infty x^{p-1} dx \psi(a+x);$$

or

$$\Gamma(n) \Gamma(p) \Sigma \frac{A_m}{a^{m-i}} \frac{\Gamma(m-i)}{\Gamma(m)} = \int_0^\infty x^{p-1} dx \psi(a+x).$$

Operate with  $\left(\frac{d}{da}\right)^i$  on both members; then

$$(-1)^i \Gamma(n) \Gamma(p) \Sigma \frac{A_m}{a^m} = \left(\frac{d}{da}\right)^i \int_0^\infty x^{p-1} dx \psi(a+x);$$

or

$$\left. \begin{aligned} \varphi(a) &= \frac{(-1)^i}{\Gamma(n) \Gamma(p)} \left(\frac{d}{da}\right)^i \int_0^\infty x^{p-1} dx \psi(a+x), \\ \int_0^\infty x^{n-1} dx \varphi(a+x) &= \psi(a). \end{aligned} \right\} \quad . \quad . \quad (2.)$$

We may put  $\varphi(a)$  under a different form by making  $a+x = \frac{a}{v}$ . The forms of  $\varphi(a)$  obtained in (1.) and (2.) differ from those given by Mr. Boole in the Cambridge Mathematical Journal, No. 20; but by varying the process a little, we might obtain his results. We may observe that the least value of  $m$  in (1.) must be greater than  $(-1)$ , and in (2.) greater than  $n+p$  or  $i$ .

In  $\varphi(x) = \varepsilon^D \varphi(0)$ , which is Taylor's theorem ( $D$  standing for  $\frac{d}{do}$ ), change  $\varphi(x)$  into  $\varphi(\varepsilon)$ , and then  $x$  into  $\log x$ ; we have

$$\varphi(x) + x^D \varphi(\varepsilon^0). \quad . \quad . \quad . \quad . \quad . \quad (a.)$$

Therefore, also,

$$\varphi(a-x) = (a-x)^D \varphi(\varepsilon^0),$$

and

$$\begin{aligned} \int_0^a x^{n-1} dx \varphi(a-x) &= \left\{ \int_0^a x^{n-1} dx (a-x)^D \right\} \varphi(\varepsilon^0) \\ &= \frac{\Gamma(n) \Gamma(D+1)}{\Gamma(D+n+1)} a^{D+n} \varphi(\varepsilon^0) = \psi(a). \end{aligned}$$

Consequently

$$\frac{\Gamma(n) \Gamma(D+1)}{\Gamma(D+n+1)} (a-x)^{D+n} \varphi(\varepsilon^0) = \psi(a-x),$$

and

$$\begin{aligned} \frac{\Gamma(n) \Gamma(D+1)}{\Gamma(D+n+1)} \left\{ \int_0^a x^{p-1} dx (a-x)^{D+n} \right\} \varphi(\varepsilon^0) \\ = \int_0^a x^{p-1} dx \psi(a-x); \end{aligned}$$

or

$$\Gamma(n) \Gamma(p) \frac{\Gamma(D+1)}{\Gamma(D+i+1)} a^{D+i} \varphi(\varepsilon^0) = \int_0^a x^{p-1} dx \psi(a-x).$$

Operating with  $\left(\frac{d}{da}\right)^i$  on both members, we find

$\Gamma(n)\Gamma(p)a^D\phi(\varepsilon^0)=\Gamma(n)\Gamma(p)\phi(a)=\left(\frac{d}{da}\right)^i\int_0^ax^{p-1}dx\psi(a-x),$   
the same result as in (1.).

In (a.) change  $\phi(x)$  into  $\phi\left(\frac{1}{x}\right)$ , and then  $x$  into  $\frac{1}{x}$ ; we have

$$\phi(x)=x^{-D}\phi(\varepsilon^{-0}); \quad . \quad . \quad . \quad . \quad . \quad (b.)$$

and therefore

$$\phi(a+x)=(a+x)^{-D}\phi(\varepsilon^{-0}),$$

and

$$\begin{aligned} \int_0^\infty x^{n-1}dx\phi(a+x) &= \left\{ \int_0^\infty x^{n-1}dx(a+x)^{-D} \right\} \phi(\varepsilon^{-0}) \\ &= \frac{\Gamma(n)\Gamma(D-n)}{\Gamma(D)} a^{n-D}\phi(\varepsilon^{-0}) = \psi(a). \end{aligned}$$

Hence

$$\frac{\Gamma(n)\Gamma(D-n)}{\Gamma(D)} (a+x)^{n-D}\phi(\varepsilon^{-0}) = \psi(a+x),$$

and

$$\begin{aligned} \frac{\Gamma(n)\Gamma(D-n)}{\Gamma(D)} \left\{ \int_0^\infty x^{p-1}dx(a+x)^{n-D} \right\} \phi(\varepsilon^{-0}) \\ = \int_0^\infty x^{p-1}dx\psi(a+x); \end{aligned}$$

or

$$\Gamma(n)\Gamma(p)\frac{\Gamma(D-i)}{\Gamma(D)} a^{i-D}\phi(\varepsilon^{-0}) = \int_0^\infty x^{p-1}dx\psi(a+x).$$

And, as before,

$$\begin{aligned} (-1)^i\Gamma(n)\Gamma(p)a^{-D}\phi(\varepsilon^{-0}) &= (-1)^i\Gamma(n)\Gamma(p)\phi(a) \\ &= \left(\frac{d}{da}\right)^i\int_0^\infty x^{p-1}dx\psi(a+x), \end{aligned}$$

the same result as in (2.).

We might by this method derive the forms of  $\phi(a)$  given by Mr. Boole; but my object is merely to show one use out of many which may be made of the formulæ (a.) and (b.)

If  $\Delta r=1$ , and  $E=1+\Delta$ ;  $E^k r=r+k$ ,  $E^k x^r=x^r x^k$ . Giving to  $k$  an infinity of different values, multiplying the results by any constants, and taking the sum, we have

$$x^r\phi(x)=\phi(E)x^r. \quad . \quad . \quad . \quad . \quad . \quad (c.)$$

It is plain that we may give to  $k$ , not only integer values, but fractional ones also, and any values whatever, and negative as well as positive ones; for the operation  $E^k$  performed on  $r$ , or on  $x^r$ , merely changes them into  $r+k$ , and  $x^{r+k}$  respectively. The function  $\phi(x)$  is therefore very general.

Change  $x$  into  $a-x$ , and we have

$$(a-x)^r \phi(a-x) = \phi(E)(a-x)^r.$$

Therefore

$$\begin{aligned} \int_0^a x^{n-1} dx (a-x)^r \phi(a-x) &= \phi(E) \int_0^a x^{n-1} dx (a-x)^r \\ &= \phi(E) a^{r+n} \int_0^1 x^{n-1} dx (1-x)^r = \Gamma(n) \phi(E) a^{r+n} \frac{\Gamma(r+1)}{\Gamma(r+n+1)} = \psi(a) \end{aligned}$$

suppose. Change  $a$  into  $a-x$ , and we have

$$\Gamma(n) \phi(E) (a-x)^{r+n} \frac{\Gamma(r+1)}{\Gamma(r+n+1)} = \psi(a-x),$$

and

$$\Gamma(n) \phi(E) \frac{\Gamma(r+1)}{\Gamma(r+n+1)} \int_0^a x^{p-1} dx (a-x)^{r+n} = \int_0^a x^{p-1} dx \psi(a-x);$$

or

$$\Gamma(n) \Gamma(p) \phi(E) a^{r+i} \frac{\Gamma(r+1)}{\Gamma(r+i+1)} = \int_0^a x^{p-1} dx \psi(a-x),$$

and

$$\Gamma(n) \Gamma(p) \phi(E) a^r = \Gamma(n) \Gamma(p) a^r \phi(a) = \left( \frac{d}{da} \right)^i \int_0^a x^{p-1} dx \psi(a-x).$$

Change  $x^r \phi(x)$  into  $\phi(x)$ , and transform the second member; then

$$\begin{aligned} \int_0^a x^{n-1} dx \phi(a-x) &= \psi(a), \quad \phi(a) = \frac{1}{\Gamma(n) \Gamma(p)} \left( \frac{d}{da} \right)^i a^p \int_0^1 dv \\ &\quad (1-v)^{p-1} \psi(av), \end{aligned}$$

as before.

Resuming the equation

$$\Gamma(n) \phi(E) (a-x)^{r+n} \frac{\Gamma(r+1)}{\Gamma(r+n+1)} = \psi(a-x),$$

we have

$$\Gamma(n) \phi(E) \frac{\Gamma(r+1)}{\Gamma(r+n+1)} \int_0^a x^{p-1} dx (a-x)^{r-p} = \int_0^a \frac{x^{p-1} dx}{(a-x)^i} \psi(a-x);$$

or

$$\begin{aligned} \Gamma(n) \Gamma(p) \phi(E) a^r \frac{\Gamma(r-p+1)}{\Gamma(r+n+1)} &= \int_0^a \frac{x^{p-1} dx}{(a-x)^i} \psi(a-x) \\ &= a^{p-i} \int_0^1 v^{-i} dv (1-v)^{p-1} \psi(av). \end{aligned}$$

Multiply by  $a^{i-p} = a^n$ , and operate with  $\left( \frac{d}{da} \right)^i$ ; there results

$$\Gamma(n) \Gamma(p) \phi(E) a^{r-p} = \left( \frac{d}{da} \right)^i \int_0^1 v^{-i} dv (1-v)^{p-1} \psi(av).$$



If therefore we change  $x^r \phi(x)$  into  $\phi(x)$ , we now have

$$\left. \begin{aligned} \int_0^a x^{n-1} dx \phi(a-x) &= \psi(a), \quad \phi(a) = \frac{a^p}{\Gamma(n)\Gamma(p)} \left( \frac{d}{da} \right)^i \\ \int_0^1 v^{-i} dv (1-v)^{p-1} \psi(av). \end{aligned} \right\} \quad . \quad . \quad (3.)$$

If we put  $D$  for  $\frac{d}{dr}$ , then

$$D^k \epsilon^{rx} = x^k \epsilon^{rx}, \quad (-D)^k \epsilon^{-rx} = x^k \epsilon^{-rx};$$

and, as in (c.), we have

$$\epsilon^{rx} \phi(x) = \phi(D) \epsilon^{rx}, \quad \epsilon^{-rx} \phi(x) = \phi(-D) \epsilon^{-rx} \quad . \quad . \quad (d.)$$

Or, if we put  $\rho^k$  for the operation which converts  $\epsilon^{rx}$  into  $x^k \epsilon^{rx}$ , and  $\theta^k$  for that which changes  $\epsilon^{-rx}$  into  $x^k \epsilon^{-rx}$ , then

$$\epsilon^{rx} \phi(x) = \phi(\rho) \epsilon^{rx}, \quad \epsilon^{-rx} \phi(x) = \phi(\theta) \epsilon^{-rx}; \quad . \quad . \quad (e.)$$

and  $k$  may be positive or negative, integer or fractional, or any quantity whatever. I believe these formulæ are new, and they admit of many uses.

Changing  $x$  into  $a+x$ , we have

$$\epsilon^{-r(a+x)} \phi(a+x) = \phi(\theta) \epsilon^{-r(a+x)};$$

and

$$\begin{aligned} \int_0^\infty x^{n-1} dx \epsilon^{-r(a+x)} \phi(a+x) &= \phi(\theta) \epsilon^{-ra} \int_0^\infty x^{n-1} dx \epsilon^{-rx} \\ &= \Gamma(n) \phi(\theta) \frac{1}{r^n} \epsilon^{-ra} = \psi(a) \text{ suppose.} \end{aligned}$$

Changing  $a$  into  $a+x$ , this gives

$$\Gamma(n) \phi(\theta) \frac{1}{r^n} \epsilon^{-r(a+x)} = \psi(a+x),$$

and

$$\begin{aligned} \Gamma(n) \phi(\theta) \frac{1}{r^n} \epsilon^{-ra} \int_0^\infty x^{p-1} dx \epsilon^{-rx} &= \Gamma(n) \Gamma(p) \phi(\theta) \frac{1}{r^i} \epsilon^{-ra} \\ &= \int_0^\infty x^{p-1} dx \psi(a+x); \end{aligned}$$

or

$$\begin{aligned} (-1)^i \Gamma(n) \Gamma(p) \phi(\theta) \epsilon^{-ra} &= (-1)^i \Gamma(n) \Gamma(p) \epsilon^{-ra} \phi(a) \\ &= \left( \frac{d}{da} \right)^i \int_0^\infty x^{p-1} dx \psi(a+x). \end{aligned}$$

Change the function  $\epsilon^{-rx} \phi(x)$  into  $\phi(x)$ , and we have

$$\begin{aligned} \int_0^\infty x^{n-1} dx \phi(a+x) &= \psi(a), \quad \phi(a) = \frac{(-1)^i}{\Gamma(n)\Gamma(p)} \left( \frac{d}{da} \right)^i \\ &\quad \int_0^\infty x^{p-1} dx \psi(a+x) \end{aligned}$$

as heretofore.

The equation

$$\Gamma(n)\phi(\theta) \frac{1}{r^n} \varepsilon^{-ra} = \psi(a),$$

found in this investigation, gives

$$\Gamma(n)\phi(\theta) \frac{1}{r^n} \varepsilon^{-r\left(\frac{a}{v}\right)} = \psi\left(\frac{a}{v}\right),$$

and

$$\Gamma(n)\phi(\theta) \frac{1}{r^n} \int_0^1 v^{-p} dv (1-v)^{p-1} \varepsilon^{-r\left(\frac{a}{v}\right)} = \int_0^1 v^{-p} dv \\ (1-v)^{p-1} \psi\left(\frac{a}{v}\right).$$

Differentiate this for  $a$ , then

$$-\Gamma(n)\phi(\theta) \frac{1}{r^{n-1}} \int_0^1 v^{-p-1} dv (1-v)^{p-1} \varepsilon^{-r\left(\frac{a}{v}\right)} = \frac{d}{da} \int_0^1 v^{-p} dv \\ (1-v)^{p-1} \psi\left(\frac{a}{v}\right).$$

Make  $\frac{1}{v} - 1 = x$ , and

$$\int_0^1 v^{-p-1} dv (1-v)^{p-1} \varepsilon^{-r\left(\frac{a}{v}\right)}$$

will be transformed into

$$\varepsilon^{-ra} \int_0^\infty x^{p-1} dx \varepsilon^{-rax} = \Gamma(p) \frac{a^{-p}}{r^p} \varepsilon^{-ra};$$

and the preceding will become

$$-\Gamma(n)\Gamma(p)\phi(\theta) \frac{a^{-p}}{r^{p-1}} \varepsilon^{-ra} = \frac{d}{da} \int_0^1 v^{-p} dv (1-v)^{p-1} \psi\left(\frac{a}{v}\right).$$

Multiply by  $a^p$ , and operate with  $\left(\frac{d}{da}\right)^{i-1}$ , there results

$$(-1)^i \Gamma(n)\Gamma(p)\phi(\theta) \varepsilon^{-ra} = (-1)^i \Gamma(n)\Gamma(p) \varepsilon^{-ra} \phi(a) \\ = \left(\frac{d}{da}\right)^{i-1} a^p \frac{d}{da} \int_0^1 v^{-p} dv (1-v)^{p-1} \psi\left(\frac{a}{v}\right).$$

After changing the function  $\phi$ , as before, we now have

$$\left. \begin{aligned} \int_0^\infty x^{n-1} dx \phi(a+x) &= \psi(a), & \phi(a) &= \frac{(-1)^i}{\Gamma(n)\Gamma(p)} \\ \left(\frac{d}{da}\right)^{i-1} a^p \frac{d}{da} \int_0^1 v^{-p} dv (1-v)^{p-1} \psi\left(\frac{a}{v}\right) & \end{aligned} \right\} \dots (4.)$$

The formulæ (3.) and (4.) are the same as those given by Mr. Boole in the paper before referred to. From the last

method of investigation, it appears that the functions  $\phi$  and  $\psi$  may be any whatever, consistent with the required relation between them. But if we are obliged to integrate by series, they will in general be subject to the restrictions mentioned in (1.) and (2.); I say, in general, for infinite quantities may vanish by the operations  $\left(\frac{d}{da}\right)^i$ .

To give an example in each of the theorems: in (1.) let

$$n = \frac{1}{2}, p = \frac{1}{2}, \psi(x) = \sqrt{x}.$$

We find

$$\left\{\Gamma\left(\frac{1}{2}\right)\right\}^2 \phi(a) = \frac{d}{da} \left(\frac{\pi a}{2}\right),$$

and

$$\phi(a) = \frac{1}{2};$$

then

$$\psi(a) = \sqrt{a},$$

as it should be.

In (2.) let

$$n = \frac{1}{2}, p = \frac{1}{2}, \psi(x) = \frac{1}{\sqrt{x}}$$

We find

$$\left\{\Gamma\left(\frac{1}{2}\right)\right\}^2 \phi(a) = \frac{d}{da} (\log a - \log 0), \quad \phi(a) = \frac{1}{\pi a};$$

and then

$$\psi(a) = \frac{1}{\sqrt{a}}.$$

In the last example  $n$  and  $p$  are not conformed to the restrictions, but the infinite quantity goes out by differentiation. The theorems (3.) and (4.) are likewise satisfied by these examples. It must not be supposed that the values of  $\phi(a)$ , given in (1.) and (3.), or in (2.) and (4.), are necessarily equal; for they will not reduce the one to the other. Yet we may have

$$\int_0^\infty x^{n-1} dx \phi(a+x) = \psi(a), \quad \int_0^a x^{n-1} dx \phi(a-x) = \psi(a)$$

in both cases; since we know from examples that the integrals of different functions may be of the same form.

Gunthwaite Hall, near Barnsley,  
May 24, 1847.

IV. *On certain Phænomena of Voltaic Ignition and the Decomposition of Water into its constituent Gases by Heat.*  
By W. R. GROVE, Esq., M.A., F.R.S.\*

IN the Philosophical Magazine for August 1841, I recommended for eudiometrical purposes, the use of a platinum wire ignited by a voltaic battery. In fig. 1 is presented a form of apparatus for this purpose; it consists of a tube of Bohemian glass, with a loop of platinum wire  $\frac{1}{80}$ th of an inch diameter sealed into its upper end; the size of the glass tube may be adapted to the quantity of gas sought to be analysed, and may when necessary be reduced to extremely small dimensions, one-eighth of an inch being ample; into this the gas may readily be made to ascend, by the insertion of a wire of copper, platinum, or glass, as may be suitable to the gas: two cells of the nitric-acid battery are sufficient fully to ignite the wire, and the same battery supplies, by electrolysis, pure oxygen and hydrogen for the analysis. Since the period when I first proposed this, I have seldom used any other apparatus for such gaseous analyses as are performed by combining the gas to be examined with oxygen or hydrogen. This eudiometer possesses the advantage of enabling the operator either to detonate or slowly to combine the gases, by using different powers of battery, by interposing resisting wires, or by manipulation alone,—a practised hand being able by changing the intervals of contact to combine or detonate the gas at will. My general practice has been to produce a gentle heat in the wire until the gases contract, and then gradually to increase the heat until a full ignition takes place, by which means all the objects of the eudiometer of Volta are fulfilled, without detonation, without dependence on the fickle electric spark, and without thick tubes, any danger of explosion, or of the gases being projected from the eudiometer.



I have commenced with a description of this eudiometer, as it has been indirectly the means of my undertaking the experiments detailed in this lecture; and as its very great convenience has never been generally understood, I think that in strongly recommending it, I shall be of service to chemists.

In a paper honoured by insertion in the Philosophical Transactions for 1845, p. 358, I have shown another method of eudiometry also performed by voltaic ignition; in that experiment the vapour of camphor was decomposed into carbonic oxide and carburetted hydrogen; it was an application

\* From the Philosophical Transactions for 1847, part i.; having been received by the Royal Society September 3, and read November 19, 1846.



of voltaic ignition to effects analogous to those produced by Priestley and others, by passing compound gases through ignited tubes of porcelain.

But the voltaic process has this immense advantage, that the heat can be rendered incomparably more intense; that the quantity of vapour or gas to be operated on may be indefinitely small; that there are no joints, stop-cocks or ligatures; and that there is no chance of endosmose, which takes place through all porcelain vessels. I therefore determined to examine by these means several gases, both with a view of verifying, under different circumstances, known results, and seeking for new effects by this new and advantageous application. I used an eudiometer (fig. 1) of 8 inches long and 0·4 inch internal diameter, exposing the gases to intense heat, and subsequently analysed the residues in one of the same length, but 0·2 inch diameter.

I will first consider the physical effects of different gases on the ignition of the wire itself.

In a paper on the Application of Voltaic Ignition to lighting Mines\*, I have mentioned the striking effects of hydrogen in reducing the intensity of ignition of a platinum wire, so much so that a wire voltaically ignited to incandescence in atmospheric air, is apparently extinguished by inverting over it a jar of hydrogen; with other gases the effects are not so striking, and with them these differences are best shown by including a voltameter in the circuit. Davy found that the conducting power of a wire diminished in proportion to the degree to which it was heated: assuming the accuracy of this position, the amount of gas in the voltameter would be inverse to the intensity of ignition in the wire. The following is the result I obtained with different gases, employing the same battery (the nitric-acid combination at its most constant period), the same wire, and the same vessel:—

Gases surrounding the wire.	Cubic inches of gas evolved in the voltameter, per minute.
Hydrogen . . . . .	7·7
Olefiant gas . . . . .	7·0
Carbonic oxide . . . . .	6·6
Carbonic acid . . . . .	6·6
Oxygen . . . . .	6·5
Compressed air, 2 atmospheres	6·5
Nitrogen . . . . .	6·4
Atmospheric air . . . . .	6·4
Rarefied air . . . . .	6·3
Chlorine . . . . .	6·1

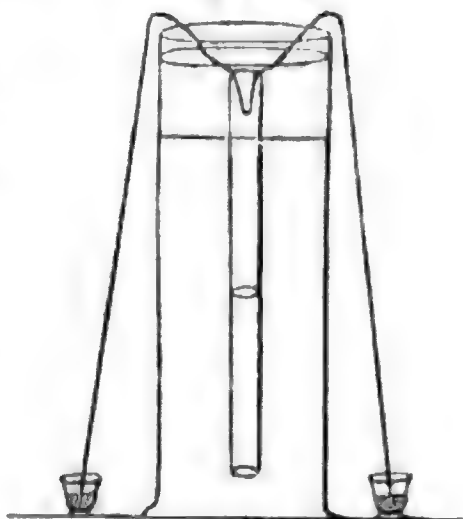
\* Phil. Mag. Dec. 1845.

To ascertain the relation between the amount of radiant heat generated by the same battery and wire in gases which presented striking differences as to the luminous effects of the platinum wire, an apparatus was prepared in which the bulb of a thermometer was retained at a certain distance from the coil of wire ignited by a battery of four cells, and exposed, first, to an atmosphere of hydrogen, and then to one of atmospheric air, at the same temperature and pressure; the thermometer rose  $7\frac{1}{2}^{\circ}$  in five minutes in the hydrogen, and  $15^{\circ}$  in the air in the same time. Both the heating and luminous effects appear therefore to be greater in atmospheric air than in hydrogen. I cannot satisfactorily account for the differences shown in the above table; there appears a general tendency to greater ignition in the electro-negative than in the combustible gases, but the facts are far too few to found a generalization. I was at first inclined to regard the difference of effect in hydrogen as analogous to the peculiarity mentioned by Leslie\* respecting its convection of sound, but the parallel does not hold; sound is transmitted imperfectly through rarefied air, and also through hydrogen; on the contrary, the heat of the ignited wire is most intense in the former, and least so in the latter; the heat is also very much reduced in intensity in the compounds of hydrogen, ammonia and olefiant gas, or even by a small admixture of hydrogen with another gas, such as nitrogen; hydrogen, therefore, appears to have a peculiar and specific action in this respect.

I now pass to the consideration of the effects of the ignited wire on different gases. The ignition was in every case raised to the fullest extent, and the gases after exposure to it were carefully cooled down to their original temperature.

When the experiments were made over water, the whole eudiometer was immersed in a vessel of distilled water, occasionally having an inch depth of oil on the surface (see fig. 2†); when over mercury, and a long-continued exposure was required, a bent tube was employed, as at fig. 3, the closed end being immersed in water or oil, to prevent the fusion of the glass which would otherwise have ensued.

Fig. 2.

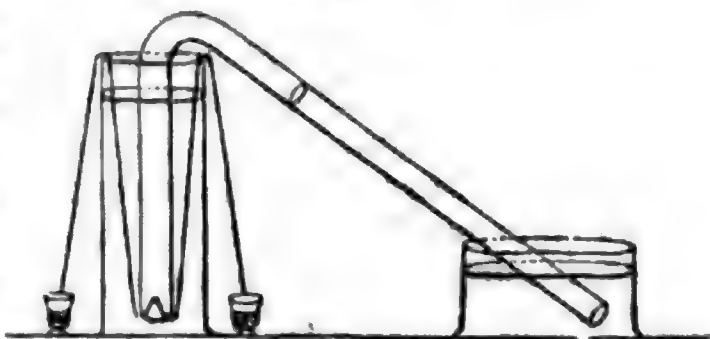


\* Transactions of the Cambridge Philosophical Society, vol. i. p. 267.

† In this and in figs. 3 and 5, the lines leading from the platinum loop to the mercury cups represent copper wires.

The tubes are much more easily preserved from cracking, and the ignition better kept up with oil on the exterior than with water, but as in many of these experiments I might have been considerably misled by a crack in the glass, or a bad sealing of the wire, allowing a portion of oil to enter the tube, I used water in the greater number of them until I was assured of the phenomena.

Fig. 3.



The apparatus, fig. 3, is superior in one respect to fig. 2, even for experiments over water, as the wire being situate below the volume of gas, the circulation is more rapid. This object may also be effected by employing the form of eudiometer, fig. 4, in which the loop of wire is near the centre of the tube, so as to be just above the surface of water in the tube; there are, however, some difficulties of manipulation with this form, which render it practically of less value than fig. 1.

Fig 4.



*Binoxide of nitrogen* over distilled water contracted differently in proportion to the heat of the wire; in the best experiment it contracted to one-third of its original volume; the residual gas was nitrogen. Nitric acid was found in solution in the water.

Over mercury the effects were nearly the same; the mercury was attacked, and the orange fumes of nitrous acid were visible.

*Protoxide of nitrogen* was decomposed into nitrogen and oxygen; the volume increased by 0.35 of the original volume; I could not get the full equivalent proportion, or 0.5 of oxygen.

*Carbonic acid* underwent no perceptible alteration.

*Ammonia* increased to double its original volume; it was now no longer absorbable by water, and gave three volumes of hydrogen, plus 1 nitrogen.

*Olefiant gas* contracted slightly, deposited carbon, the residue being hydrogen and olefiant gas, more of the former in proportion to the heat, but I could not succeed in entirely decomposing it.

*Nitrogen* suffered no change.

*Oxygen* gave a very slight contraction, amounting to  $\frac{1}{30}$ th of its volume; the oxygen employed was very pure, obtained from chlorate of potash and manganese, and also from water

by electrolysis : no change in properties was perceptible in the oxygen after its exposure to the ignited wire. This contraction I incline to attribute to a slight portion of hydrogen present, which view will, I think, be considered as strengthened by the effect of the ignited wire on hydrogen, to be presently detailed. I at one time thought that the contraction might be due to a slight oxidation of the wire, but it never went beyond a very limited point; nor was the wire altered in size or weight, though it was kept ignited for many hours.

*Chlorine* over water gave dense white fumes; a grayish-yellow insoluble powder accumulated on the sides of the tube near the platinum wire, which appeared of the same nature as the vapours; the deposit was insoluble in cold nitric, sulphuric, or muriatic acid, but dissolved by the last when boiled. The fumes did not, as far as I could judge, affect litmus paper; a barely perceptible tinge of red was indeed communicated to it, but this, I had every reason to believe, was attributable to a slight portion of muriatic acid not absorbed by the water. I have not yet worked out this result, as it is probable, considering the number of experiments that have been made on heated chlorine, that it is a known product, though I cannot find, in several books to which I have referred, any substance answering to it in description, and the field opened by voltaic ignition is so new that each result demands a separate and prolonged examination; if I find that this is an unknown compound I shall probably resume its investigation\*.

*Cyanogen* gave, though in very minute quantities, a somewhat similar deposit, but at its then very high temperature it began to act rapidly on the mercury, and I was obliged to give up the experiment after an hour's ignition. Both these gases require peculiar and novel apparatus for examination by voltaic ignition. It will presently be seen that my whole attention and disposable time were necessarily occupied with certain phænomena to which this class of experiments ultimately led me.

*Hydrogen* gave a very notable contraction, amounting in some cases to one-tenth of its volume. This was an unexpected result, and I examined it with care. It took place both over water and over mercury; rather more with the former than with the latter. It obtained equally with hydrogen procured by electrolysis from carefully distilled water and pure sulphuric acid; with that procured from common zinc and pure sulphuric acid diluted with distilled water; and with that obtained from distilled zinc and pure diluted sulphuric acid. The contraction was less when the water from which

\* See Supplemental paper.



the hydrogen was obtained was carefully purged of air by boiling and the air-pump, but yet there was a notable contraction even when the water had been freed from air to the utmost practicable extent. In the numerous experiments which I made on this subject, the contraction varied from the  $\frac{1}{10}$ th to the  $\frac{1}{30}$ th of the whole volume.

After many fruitless experiments I traced it to a small quantity of oxygen which I found hydrogen to contain under all circumstances in which I examined it. Phosphorus placed in hydrogen, obtained with the utmost care, gives fumes of phosphorous acid, shines in the dark and produces a slight contraction, but there is after this a further contraction by the use of the ignited wire.

I may cite the following as an easy experiment and simple illustration of the rapidity with which hydrogen appropriates oxygen. Let hydrogen be collected over water well-purged of air; let a piece of phosphorus remain in it until all combustion has ceased, the hydrogen will then be full of phosphoric vapour; fill another tube with water, and pass the hydrogen rapidly into it, the second tube will instantly be filled with a dense white fume of phosphorous acid; the hydrogen having instantly carried with it oxygen from the stratum of water it has passed.

A very careful experiment was made as follows:—distilled water was boiled for several hours, to this was added one-fortieth part by measure of pure sulphuric acid, and it was cooled under the receiver of an air-pump; it was now placed in two test-glasses, connected by a narrow inverted tube, full of the same liquid: platinum electrodes were placed in each glass, and the hydrogen caused to ascend immediately into the eudiometer tubes; the whole was completed within two or three minutes after the water had been removed from the air-pump. Here the ordinary sources of impurity in hydrogen were avoided; no zinc was used, the sulphuric acid was pure, and the quantity was so small, that, had it not been pure, the error could have been but very trifling. The hydrogen so obtained, contracted in volume  $\frac{1}{28}$ th; hydrogen prepared in the same way, and exposed to phosphorus, gave dense white fumes; the phosphorus was luminous in the dark for more than an hour, and the contraction (temperature and pressure being carefully examined) was  $\frac{1}{90}$ th; the amount of contraction by the wire would of course equal three times the volume of oxygen mixed with the hydrogen, consequently the oxygen would be  $\frac{1}{78}$ th of the whole volume; the platinum wire induces therefore a greater absorption of oxygen than the phosphorus, unless the volume of hydrogen is increased by the phosphoric

vapour; the sequel of this paper will render it probable that even the ignited wire does not and cannot induce combination of all the oxygen existing in the hydrogen.

I have looked into the papers of MM. Berzelius and Dulong, and of M. Dumas on the equivalent weight of hydrogen. The latter contains a most careful experimental investigation, and is by far the best determination we have; although it is not there mentioned that hydrogen contains oxygen, yet a correction is made for the air contained in the sulphuric acid employed. M. Dumas does not state how the quantity of that air is calculated. There can be no question that nothing approaching in elaborate care to these experiments has been yet performed on the subject; but with the fullest consciousness of M. Dumas' skill, I have, in all my experiments, perceived such an inveterate tendency of hydrogen to possess itself of oxygen, that I cannot help entertaining some doubts whether we have yet the real weight of hydrogen within the assigned limits of error.

It is difficult to see how hydrogen can be absolutely deprived of oxygen which has once existed in it; neither an oxidable metal as zinc, or an ignited inoxidable metal as platinum, getting rid of all the oxygen, and phosphorus, if it does so, replaces it by its own vapour. The near approach, however, of the equivalent of hydrogen, as determined by M. Dumas, to the ratio of whole numbers, renders it probable that it is a very close approximation to the truth.

I have not been able to detect nitrogen in the hydrogen, but the probability is that a slight quantity also exists in it. Whether the oxygen proceeds from portions of air still remaining in solution in the liquid from which the air is exhausted, or whether it is a part of the water actually decomposed, but of which the oxygen is not absorbed by the zinc, is a question to resolve which further experiments are necessary.

*Hydrogen and carbonic acid* mixed in equal volumes were readily acted on by the ignited wire; they contracted to 0.48 of the original volume; the residue was carbonic oxide; one equivalent of oxygen had therefore united with the hydrogen; and the slight additional contraction was probably due to the further combination of hydrogen with oxygen, as above stated.

*Carbonic oxide* exhibited a remarkable effect, and one which, coupled with the last experiment, gave rise to considerations which mainly led to the results to be detailed in the body of this paper. Carbonic oxide, very pure and carefully freed from carbonic acid, was exposed to the ignited wire over distilled water; the gas increased in volume in one experiment to one-

third of its original volume, in the greater number of instances to one-fifth: this increase depended upon the intensity of ignition, which it was very difficult to maintain at its maximum on account of the frequent fusions of the platinum wires.

Here again I had a long research and many erroneous guesses, which I need not detail. The effect did not take place with perfectly dry gas over mercury, and I thence was led to attribute it to some combination with aqueous vapour; the increase turned out to be occasioned by the formation of carbonic acid. By agitation with caustic potash or lime water the gas was reduced to exactly its original bulk, but it was now found to be mixed with a volume of hydrogen equal to the volume of carbonic acid by which it had been increased; it was thus perfectly clear that half a volume or one equivalent of oxygen derived from the vapour of the water, had combined with one volume or equivalent of carbonic oxide, and formed one volume or equivalent of carbonic acid, leaving in place of the carbonic oxide with which it had combined, the one volume or equivalent of hydrogen with which it had been originally associated.

Comparing the last experiment, viz. that of mixed carbonic acid and hydrogen with this, I was naturally struck with the curious reversal of affinities under circumstances so nearly similar; in the one case, hydrogen taking oxygen from carbonic acid to form water and leaving carbonic oxide; in the other, carbonic oxide taking oxygen from water to form carbonic acid and leaving hydrogen.

I thought much upon this experiment; it appeared to me ultimately that the ignited platinum had no specific effect in producing either composition or decomposition of water, but that it simply rendered the chemical equilibrium unstable, and that the gases then restored themselves to a stable equilibrium according to the circumstances in which they were placed with regard to surrounding affinities; that if the state of mixed oxygen and hydrogen gas were, at a certain temperature, more stable than that of water, ignited platinum would decompose water as it does ammonia.

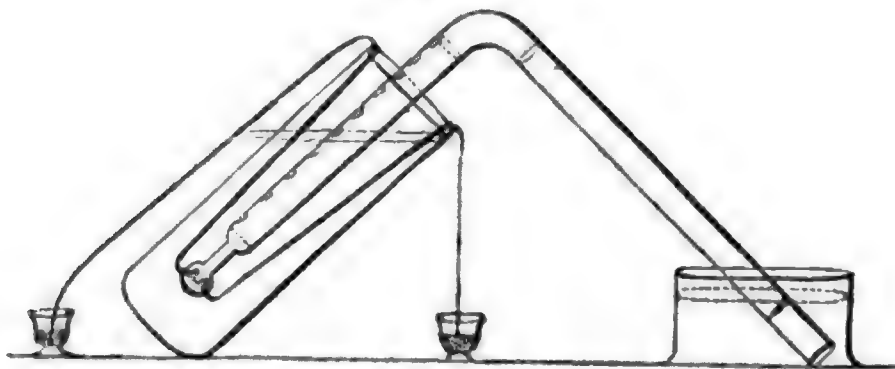
This is a very crude expression of my ideas, but we have no language for such anticipatory notions, and I must adapt existing terms as well as I am able.

It now appeared to me that it was possible to effect the decomposition of water by ignited platinum; that, supposing the atmosphere of steam in the immediate vicinity of ignited platinum were decomposed, or the affinities of its constituents loosened, if there were any means of suddenly removing this atmosphere I might get the mixed gases; or secondly, if, as

appeared by the last two experiments, quantity had any influence, that it might be possible so to divide the mixed gases by a quantity of a neutral ingredient as to obtain them by subsequent separation (or as it were filtration) from the neutral substance. Both these ideas were realized.

To effect the first object, after, as usual in such circumstances, much groping in the dark, I cemented a loop of platinum wire in the end of a tube retort similar to fig. 3, and covered it with asbestos, ramming this down so as to form a plug at the closed extremity of the tube, the platinum wire being in the centre. My object was, by igniting the platinum wire, to drain the water out of the asbestos, and the ignited wire being then in an atmosphere of steam, I hoped the water would by capillary attraction keep constantly oozing down to the platinum wire as the steam or decomposed water ascended. The experiment did not succeed; the water established a current through the asbestos by washing away fine particles, and the phænomena of ordinary ebullition took place, unless the intensity of the battery was very much exalted, when a very slight decomposition was perceptible, which I attributed to electrolysis. This experiment, however, suggested another which did succeed. In one or two cases the asbestos plug became compressed above the platinum and so choked up the tube that the wire suddenly fused. It now occurred to me that by narrowing the glass tube above the platinum wire I had the result at my command, as the narrow neck might be made of any diameter and length, so as just to allow the water to drip or run down as the steam forced its way up; a tube was so formed, and is shown with its accompaniments at fig. 5.

Fig. 5.



The result of this experiment was very striking: when two cells of the nitric-acid battery were applied the air was first expanded and expelled, the water then soon boiled, and at a certain period the wire became ignited in the steam. At this instant a tremulous motion was perceptible, and separate bubbles of permanent gas of the size of pin-heads ascended,



and formed a volume in the bend of the tube. It was not a continuous discharge of gas as in electrolysis, but appeared to be a series of rapid jerks; the water, returning through the narrow neck, formed a natural valve which cut off by an intermitting action portions of the atmosphere surrounding the wire; the experiment presented a novel and indescribably curious effect. The gas was oxyhydrogen. It will occur at the first to many of those who hear this paper read, that this effect might be derived from electrolysis. No one seeing it would think so for a moment; and although I shall by my subsequent experiments, I trust, abundantly negative this supposition, yet as this was my first successful experiment on this subject, and is *per se* an interesting and striking method of showing the phænomenon of decomposition by heat, I will mention a few points to prove that the phænomenon could not be occasioned by electrolysis.

To account for it by electrolysis, it must be supposed that the wire offered such a resistance to the current that this divided itself, and the excess of voltaic power passed by the small portion of water which trickled down, instead of by the wire.

In the first place, the experiment was performed with distilled water, and only two cells of the battery employed, which will not perceptibly decompose distilled water.

2ndly. No decomposition took place until the instant of ignition of the wire, though there was a greater surface of boiling water exposed to the wire before than after the period of ignition.

3rdly. A similar experiment was made, but with the wire divided in the centre so as to form two electrodes, and the water boiled by a spirit-lamp; here the current had no wire to conduct any part of it away, but the whole was obliged to pass across the liquid, and yet no decomposition took place, or if there were any it was microscopic.

4thly. When, instead of oil, distilled water was used in the outer vessel\*, even the copper wires, one of which would form an oxidable anode, gave no decomposition across the boiling water outside, while the ignited wire inside was freely yielding mixed gases.

5thly. To prevent the water from being the shortest line for the current, I repeated the experiment with a perfectly straight wire (fig. 6). The result was precisely the same, but the experiment is more difficult; as a certain length of

\* January 8.—I have since found that the exterior tube of oil or water may be dispensed with in this experiment, as the water which trickles down prevents the fusion of the glass.

wire is necessary, the sealing is more troublesome, and the size of the bulb is much more difficult to adapt to the production of steam in exactly the requisite quantity; the straight wire being more suddenly extinguished and more easily fused: with careful manipulation however it succeeds equally well with the former experiment.

I might add other experiments and arguments, but I believe when the remainder of this paper has been read, that the above will be thought scarcely necessary.

I now directed all my efforts to produce the effects by heat alone without the battery. I will mention a few of my unsuccessful attempts, as it will save trouble to future experimenters. I sealed a platinum wire into the extremity of a curved tube, filled the latter with water, and applied a strong heat by the blowpipe to the projecting end of the wire, hoping that the conducting power of the platinum, although inferior to that of most other metals, was sufficiently superior to that of glass to enable me to ignite the portion of the wire within the tube, and thus surround it with an atmosphere of steam; the water however all boiled off from the glass; nor could I succeed in igniting the platinum by heat from without. A similar failure occurred when, on account of its superior conducting power, a gold wire was substituted for that of platinum.

I sealed spongy platinum and bundles of platinum wire into the ends of Bohemian glass tubes, closing the glass over them, and then filling the tubes with water and heating the whole extremity; but the water boiled off from the glass, and the platinum could not be made to attain a full incandescence.

After many similar trials I returned to the battery, and sought to apply it in a manner in which electrolysis could not possibly take place. I had hoped, as I have above stated, to obtain a residual decomposition of water by masking or diluting the gases by a neutral substance. I therefore tried the following experiment: a tube similar to fig. 1 was filled with water which had been carefully freed from air by long boiling and the air-pump; it was then inverted in a vessel of the same water, and a spirit-lamp applied to its closed extremity, until the upper half was filled with va-

Fig. 6.

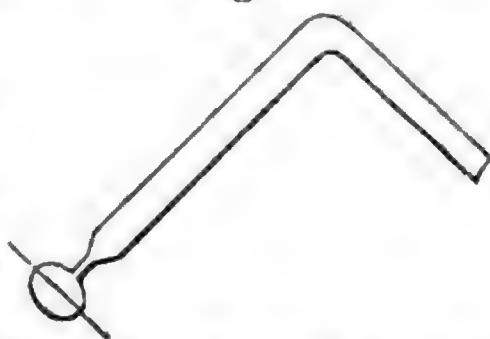
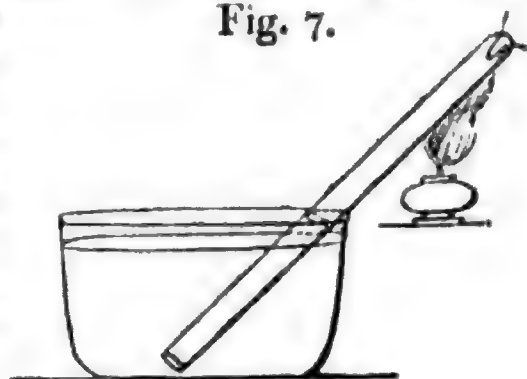


Fig. 7.



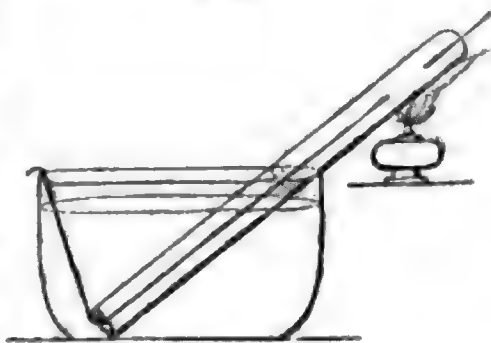
pour (fig. 7). The wire was brought to a full ignition by the battery, and kept ignited for a few seconds; connexion was then broken and the lamp removed, so that the water gradually ascended. A bubble of the size of a large mustard-seed was left in the extremity of the tube, and I was much gratified at finding that when this was caught by a lighted match at the surface of the water-trough it detonated. The experiment was then repeated, continuing the ignition for a longer time, but the gas could not be increased beyond a very limited quantity; indeed it was not to have been expected, as supposing it to be mixed gas, recombination of the excess would have taken place, and the fact of any uncombined gas existing when exposed to incandescent platinum, will doubtless surprise those who hear it for the first time.

The experiment was repeated as at first and the bubble transferred to another tube; the wire was then again ignited in vapour, another bubble was instantly formed and transferred, and so on, until after about ten hours' work sufficient gas was collected for analysis; this gas was now placed in an eudiometer, it detonated and contracted to 0.35 of its original volume; the residue being nitrogen. The experiment was repeated several times with the same general results, the residue sometimes containing a trace of oxygen.

Here electrolysis was out of the question; the wire was ignited in (if I may use the expression) dry steam, the upper part of the tube being far above the boiling-point, and of course perfectly transparent; if not an effect of heat, it must have been a new function of the electric current, at least one hitherto unknown.

As the voltaic arc and electric spark afford heat of the greatest intensity, I tried a succession of electric sparks from platinum wires through steam in the apparatus fig. 8, the water, as in all my experiments, having been previously purged of air (to save circumlocution I will in future call it prepared water). The sparks were taken from the hydro-electric machine of the London Institution; they had in the steam a beautiful crimson appearance; on cooling the tube a bubble was perceptible, which detonated by the match.

Fig. 8.



As in the previous experiments, a whole day's work did not increase the bubble, but when it was transferred another instantly formed; the gas was similarly collected; it detonated

and contracted to 0.4 of its original volume; the residue was nitrogen with a trace of oxygen.

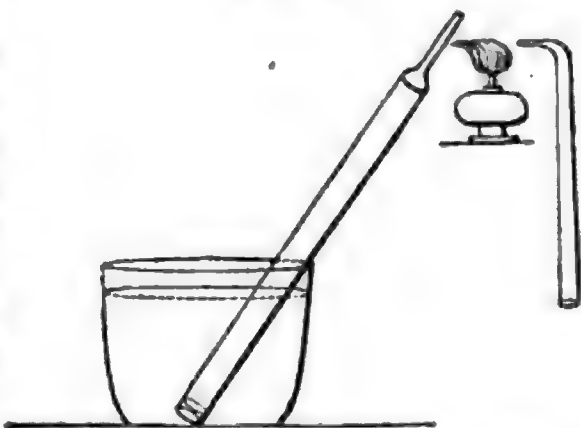
This experiment will again surprise by its novelty; the very means used in every laboratory to combine the mixed gases and form water, here decompose water\*. From a vast number of experiments which I have made on the voltaic and electric disruptive discharges (which are I believe similar phenomena, differing only in quantity and intensity), I believe the decompositions produced by them are the effects of heat alone, and this experiment was therefore to my mind a repetition of the last under different circumstances; others however may think differently. This experiment also I several times repeated.

By counting the globules given off, and comparing a certain number of them with the average volume of steam in the last two experiments, an attempt was made to ascertain what proportion of water could be decomposed by an ignited platinum wire in aqueous vapour, or, which amounts to a corollary from this proposition, what degree of dilution would enable mixed gas to exist without combustion in an atmosphere of steam exposed to an ignited platinum wire. The proportion in an experiment in which the globules were so counted, was 1 to 2400; the probability is however that different temperatures of the platinum wire would give different volumes of gas so decomposed, the volume being greater as the wire is more intensely ignited.

Although there was no known effect of electricity which could produce the phenomenon exhibited by the last two experiments, and it was in any event new, still, firmly convinced that it was an effect of heat, I again determined to attempt its production by heat alone, and without the use of the battery. I procured a tube of silver 9 inches long and 0.4 inch diameter; at the extremity of this was a platinum cap

Fig. 9.

to which a smaller tube, also of platinum, was soldered. This platinum tube was closed at the end and soldered with gold solder. The apparatus was filled with prepared water; the water was boiled in the tube to expel the air from the narrow tube and any which might have adhered to the vessel;



\* I need scarcely point out the distinction, in fact, between this experiment and those in which liquid water has been decomposed by the electric spark. See Supplemental Paper.



the tube was then, when full of hot water, inverted into water, and the flame of a common blowpipe made to play against the platinum tube (fig. 9) until a white heat was obtained. Upon inverting it under water, a bubble of the size of a mustard-seed rose to the surface, which gave a very feeble detonation with the match. Similar bubbles were collected as before, and the gas in an eudiometer contracted to 0.7. On repetition the experiment did not succeed so well, and upon several repetitions it sometimes succeeded and sometimes failed, and I should not mention it but that it was the first experiment which gave me, although not very satisfactorily, the effect of decomposition by heat alone. The reason of its uncertainty I believe to have been the want of a sufficiently intense heat, as I dared not venture on account of the gold solder to push the ignition very far; in fact, I subsequently fused the extremity and spoiled the apparatus by applying the oxyhydrogen flame to it; had the platinum tube been welded instead of gold-soldered, it would doubtless have succeeded better. I should state that the object of the silver tube was to prevent the chance of recombination by the catalytic effect of a large platinum surface; to have, in short, a small portion of platinum exposed to the steam, and that at a high temperature: economy was also no indifferent consideration. This experiment, although, coupled with the previous ones, tolerably conclusive, did not satisfy me, and I attacked the difficulty in another manner. The experiment (fig. 5) induced me to believe that if I could get platinum ignited under water so as to be in an atmosphere of steam, decomposition would take place; and M. Boutigny's experiments on the spheroidal state of water led me to hope I might keep platinum for some time under conditions suitable for my purpose.

After a few failures I succeeded perfectly by the following experiment. The extremity of a stout platinum wire was fused into a globule of the size of a peppercorn, by a nitric-acid battery of thirty cells; prepared water was kept simmering by a spirit-lamp, with a tube filled with water inverted in it; charcoal being the negative terminal, the voltaic arc was taken between that and the platinum globule until the latter was at the point of fusion; the circuit was now broken, and the highly incandescent platinum plunged into the prepared water: separate pearly bubbles of gas rose into the tube, presenting a somewhat similar effect to experiment (fig. 5). The process was repeated, the globule being frequently plunged into the water in a state of actual fusion; and when a sufficient quantity of gas was collected it was examined, it detonated,

*Phil. Mag. S. 3. Vol. 31. No. 205. July 1847.*

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leaving 0.4 residue; this was as usual nitrogen with a trace of oxygen. A second experiment gave a still better result, the gas contracting to 0.25 of its original volume.

On making the platinum negative and the charcoal positive, a very different result followed: the carbon was, as is known to electricians, projected upon the platinum; and the gas in this case was mixed with carburetted hydrogen and carbonic oxide. I know no experiment which shows so strikingly the different effects at the disruptive terminals as this; when the platinum is negative it gives much carbonic gas, when it is positive, not a trace (the gas was delicately and carefully tested for it); nay, more, by changing the platinum from negative to positive the carbon is instantly removed, and in a single experiment the platinum becomes perfectly clean.

Here then I produced very satisfactorily decomposition by heat; it is true, the battery was used, but used only as a means of fusing the platinum, as this was, as soon as fused, entirely separated from the circuit and could have no possible voltaic action. Wishing however altogether to avoid the use of the battery, I repeated this experiment, employing as my means of fusing the platinum the oxyhydrogen blowpipe; the experiment was equally successful, perhaps more so, as the manipulation was more easy.

I could readily by this means collect half a cubic inch or more of the gas; when detonated, the residue of nitrogen averaged 0.35 of the original volume.

In carefully watching this experiment, I observed that at first a rapid succession of bubbles ascended into the tube from the incandescent platinum, it then became quiescent; the spheroidal state was assumed by the water and no gas ascended; on losing the spheroidal state a sudden hiss was heard, and a single bubble ascended into the tube. I determined to examine separately the gas from the platinum before and after the quiescent state; to effect this I placed two inverted tubes in the capsule with the orifices near each other; the platinum at the point of fusion was immersed under one tube, say tube A, and as soon as the ascent of bubbles ceased, it was removed across to tube B, and the last bubble then entered that tube; the gases from each tube were separately analysed, and tube A gave nearly all detonating gas, the residue being only 0.2; tube B gave none; the gas collected in it was nitrogen, with a trace of oxygen.

In order to examine the effect of an oxidable metal under similar circumstances, I fused by the oxyhydrogen blowpipe the end of a stout iron wire, plunged it into prepared water and collected the globules of gas; no oxygen was given off,

or at least no more than I have always found to accompany hydrogen, which with a small residue of nitrogen was the gas given off in this experiment.

[To be continued.]

V. *Invention of Fluxions.* By CHARLES RICHARD WELD, Esq.  
To the Editors of the *Philosophical Magazine and Journal*.

GENTLEMEN,

IN the course of my researches in the archives of the Royal Society, with reference to a history of the Society which I am compiling, I have been much struck with a very remarkable discrepancy on a most important point, connected with the celebrated dispute of the invention of fluxions, between the original Minutes of the Society and the statements of writers on this subject.

Sir David Brewster and Professor De Morgan, following others, state that at a meeting of the Society held on the 20th of May 1714, a resolution was inserted in the Minutes, that "it was never intended that the report of the committee was to pass for a decision of the Society\*." This alludes to the report presented by the committee appointed by the Society to determine the question of the invention of fluxions. Now the exact words of the minute are these:—

"It was not judged proper (since this letter was not directed to them†) for the Society to concern themselves therewith, nor were they desired so to do; but that if any person had any material objection against the *Commercium* or the report of the committee, it might be reconsidered at any time‡."

There is nothing here to show that the Society *resolved* (and this is the word Mr. De Morgan uses) upon repudiating the report of their committee; so far from this, the opposite conclusion is at once obvious, which is in keeping with the original resolution of the Society *adopting* the report of their Committee, *nemine contradicente*. The point is of great moment; for had the Society come to the resolution as represented, a strong case would be made out against Newton. I have examined the Minutes of the meeting in question with the greatest care, and confidently assert that there is no other allusion to the dispute between Leibnitz and Newton. In conclusion, I wish to state that it is at the request of some of

\* See Life of Newton by Brewster, p. 211, and Life by De Morgan, p. 93.

† Alluding to a letter of Leibnitz to Chamberlayne, complaining of the report of the committee.

‡ Journ. Book, vol. xi. p. 431.

our most eminent philosophers that I send you this "correction," which they conceive ought to be made public through the medium of the Philosophical Journal.

I am, Gentlemen,

Your humble Servant,

Royal Society, Somerset House,  
June 11, 1847.

CHARLES RICHARD WELD.

VI. *Researches on the Composition and Characters of certain Soils and Waters belonging to the Flax districts of Belgium, and on the Chemical Constitution of the Ashes of the Flax Plant.* By Sir ROBERT KANE, M.D., M.R.I.A.\*

ABOUT two years since, I had the honour to submit to the Royal Irish Academy the results of some inquiries into the chemical composition of the flax and hemp plants, and into the chemical phænomena of the treatment which they undergo in the preparation of the ligneous fibre for the purposes of the arts. The main object of that memoir was to point out that, whilst the plant, as a whole, was rich in alkalis, earths, sulphuric and phosphoric acids, &c., the fibre, as ultimately purchased in the market, was practically destitute of all these materials, which therefore remained amongst the substances removed from the plants during their preparation, and hitherto rejected as of no use. Those results being published in the Proceedings of the Royal Irish Academy, and copied thence into various agricultural books and journals, have in some degree led to the œconomising of those valuable residues; and it is to be hoped that, according as the attention of farmers becomes more definitely fixed upon the real and philosophical principles of the growth and composition of various crops, the utilization of the different parts of plants will be still more carefully attended to.

The researches to which I have referred, involved the determination of the elementary composition of the plants, only so far as it was necessary to prove the presence and proportional quantity of certain materials in the plant as it grows, and their absence in the fibre as prepared; but it was not my design therein at all to discuss the very important questions, so fundamental to vegetable chemistry and physiology, of the degree within which the composition of the ashes of a plant may vary; or whether there is any general expression within which the constitution of the mineral elements of a plant is necessarily contained; or finally, whether there can be traced

\* Read at the Agricultural Evening Meeting of the Royal Dublin Society, held on the 6th of April 1847.



any positive relation between the composition of the plant and the composition of the soil upon which it grows. To answer these questions even approximatively, will require investigations frequently repeated, and the concurrent labours of many different investigators; and although my present inquiries may serve to furnish certain grounds for arriving at an opinion upon these points, I would not in any way be understood as putting them forward with that view.

My main object, in the inquiry which forms the subject of the present paper, was to ascertain, if possible, whether there existed any difference between the composition of the ashes of the ordinary flax of Ireland and the flax grown in those localities in Belgium, where that plant is known to yield a fibre of so much commercial value. Further, to ascertain the composition of the soils of those districts, in order to compare them with the soils of the localities in Ireland where flax is, or may be, successfully cultivated. Finally, as it is known that in the preparation of the fibre the most important stage consists in the steeping or retting of the plant, I considered it of the greatest interest to trace, if possible, whether the superior qualities of some rivers or ponds in Belgium could be connected with any peculiarity of chemical constitution. For the materials and specimens necessary for these investigations, I am indebted to the kindness and liberality of Mr. Marshall of Leeds; who was anxious also to connect therewith the discussion of some most important points of special technical application, for which, however, the pressure of other avocations did not allow me time. I therefore publish the results contained in the present paper, solely under their scientific relations to agricultural chemistry and physiology, and shall not enter upon any considerations belonging to manufacturing practice.

Before entering into the description of the numerical results of the analyses, I think it better to premise a succinct notice of the modes of analyses adopted for the different classes of substances, as I shall thereby be enabled to avoid a great deal of repetition.

#### *1. Of the Modes of Analyses used for the Ashes, Soils and Waters.*

The preparation of the flax-ashes was effected by chopping up the plant stems into moderately small bits, and then carbonizing them gently in a Hessian crucible. The material so obtained was further incinerated by very gentle ignition in a platinum capsule over a gas flame; but it was not in any way sought to burn off all charcoal, or to obtain the ash perfectly

white, as such would require a temperature capable of materially altering the constitution of the ash, a fact of which I have been long aware, and which has latterly fixed the attention of several chemists. The ash so prepared was carefully dried in a stove, and then treated in the following manner:—

Dilute muriatic acid having been poured over the quantity of ash selected for analysis, the whole was heated in a water-bath until it dried completely down; water was then added, and when the soluble materials had been completely taken up, the whole was thrown upon a weighed filter and the liquor separated; there remained upon the filter such particles of sand or soil as had been adherent to the plants, the unburned charcoal of the ash, and the silica which had existed in the ash, either free or in combination with alkaline or earthy bases.

The weight of this insoluble residue having been properly determined, it was boiled in a strong solution of caustic potash, by which all the proper silica of the ash was taken up, and the residue then remaining being weighed, gave the sand and charcoal, the silica being thus determined by difference.

The muriatic solution was then divided into three parts for the determination—

1. Of the alkaline constituents.
2. Of the phosphoric acid, manganese, alumina, magnesia, and lime.
3. Of the sulphuric acid and oxide of iron.

The first portion of solution was rendered slightly alkaline by carbonate of ammonia, and then mixed with solution of caustic barytes in excess, and allowed to stand for some hours. By this means the sulphuric and phosphoric acid were perfectly removed, as well as the earthy constituents, except a small quantity of lime, which remained dissolved in a caustic state, and which was then perfectly removed by the addition in excess of a mixture of caustic and carbonated ammonia. The liquor, after filtration, was evaporated to dryness, and the residue gently ignited, when the ammoniacal salts were perfectly expelled: there remained the alkalies of the ash as chlorides. This residue was weighed, then dissolved in water, and a solution of bichloride of platinum added. The liquor and precipitate were then evaporated nearly to dryness, the potash platinum salt washed by a mixture of alcohol and æther, and the amount of platinum determined in the usual way. The soda was ascertained by subtracting the weight of the chloride of potassium from the weight of the mixed chlorides, as given in the first instance.

To the second portion of the liquor was added so much ammonia as nearly neutralized it without producing any per-

manent precipitate. A quantity of perchloride of iron was then added, and acetate of potash, until a deep wine-red colour was produced; the liquor was then boiled until all odour of acetic acid ceased, and a copious brown precipitate formed, which was separated by the filter. This precipitate was then redissolved in muriatic acid, boiled until all odour of acetic acid ceased, and the liquor then precipitated by ammonia. The precipitate, collected on a filter, was dried, ignited, and weighed, then redissolved by muriatic acid. A quantity of tartaric acid was added to the liquor, and ammonia then added in such excess as to redissolve the precipitate which first forms. To the solution thus got, hydrosulphuret of ammonia was added in excess, the sulphuret of iron collected on a filter, and, when washed, redissolved in aqua regia. The peroxide of iron, precipitated from the liquor by ammonia, collected, dried, ignited and weighed, and its weight subtracted from the weight of the basic phosphate previously given, determines in an absolute manner the quantity of phosphoric acid.

To the liquor from which the phosphoric acid had been separated by the means described above, hydrosulphuret of ammonia was added, by which a precipitate was formed, which was collected, and, while moist, boiled with caustic potash liquor; the undissolved matter was dissolved in muriatic acid nearly neutralized, and treated with benzoate of ammonia; by this a trace of iron, generally remaining from the preceding process, was removed, and the manganese was then precipitated by carbonate of ammonia, collected, ignited and weighed. The potash liquor was then acidulated by muriatic acid, and the alumina which it had dissolved was precipitated, and its quantity determined in the usual way.

The solution, from which the iron, alumina, and manganese had thus been separated by hydrosulphuret of ammonia, was next boiled until all odour of sulphuretted hydrogen ceased, and then treated with oxalate of ammonia, the oxalate of lime collected was gently ignited with carbonate of ammonia, and the quantity of lime determined. The liquor was then very much concentrated by evaporation, and treated with phosphate of soda and ammonia, set aside until the ammoniacomagnesian phosphate had perfectly deposited, and the quantity of magnesia determined from that of the latter salt.

The third portion of the ash liquor was treated with nitric acid, so as perfectly to peroxidize the iron; it was then decomposed by chloride of barium, by which all the sulphuric acid was separated as sulphate of barytes, collected and weighed. To the filtered liquor there was added a great excess of phosphate of soda and ammonia, and then an excess of acetic acid.

By boiling, the whole of the iron separated as perphosphate, and was collected, ignited, and the quantity of oxide of iron calculated from its weight.

For the determination of the chlorine, a totally distinct portion of ash was taken, digested with water, acidulated with nitric acid, and then precipitated with nitrate of silver in the usual way.

It will be observed that, in all its main features, this plan of examination of the ash coincides with that employed by Will and Fresenius, and proposed by them in their memoir on the Composition of certain Ashes. It is, however, that which I had followed in all my former ash analyses, except in regard to the determination of the phosphoric acid, for which I had previously made use of the method proposed by Schulze, but now replaced with so much advantage by that invented by Will.

It is necessary, however, to remark, that the composition of the perphosphate of iron, given by Will, and upon which he founds his mode of determining the quantity of oxide of iron, has been contested very recently by Wittstein, who has not succeeded in preparing that salt with the composition assigned to it by Will. According to the latter chemist, it consists, in its anhydrous form, of  $2\text{Fe}_2\text{O}_3 + 3\text{PO}_5$ , that is,

$2\text{Fe}_2\text{O}_3$	. . .	160	42.89
$3\text{PO}_5$	. . .	213	57.11
		<hr/> 373	<hr/> 100.00

whilst the salt uniformly obtained by the other chemists was  $\text{Fe}_2\text{O}_3 + \text{PO}_5$ , or

$\text{Fe}_2\text{O}_3$	. . .	80	52.98
$\text{PO}_5$	. . .	71	47.02
		<hr/> 151	<hr/> 100.00

But the circumstances of preparation of the different salts do not appear to have been quite identical; and I do not, therefore, reject Will's numbers, which have been, moreover, verified by some trials made in my own laboratory. I have consequently employed his formula in calculating the amount of iron in the different materials; but it is easy to calculate, for each analysis, the change (in most cases trifling) which the employment of Wittstein's formula for the perphosphate should introduce.

In the examination of the soils, the process consisted, first, in mechanically separating the sandy and gravelly materials from the finely-divided portion, by careful elutriation with the smallest possible quantity of pure water. This having been



done, and the quantity of sand determined by direct weighing, the finely-divided earthy material was carefully dried at the highest temperature it would bear without its organic constituents being injured, and then weighed. It was then carefully but gently ignited in a current of air, until the organic materials were burned out, and was then again weighed. The loss of weight gave the quantity of organic substance, together, however, with some traces of water, from which the soil could not be previously perfectly freed.

The soil was then subjected, for the determination of its chemical constituents, to precisely the same general plan of treatment which I have described in the case of the ash. The matter, insoluble in muriatic acid, was however found to be (the sand and organic matters having been previously separated) ferruginous clay, which it was not necessary further to examine, as all the materials of importance, in studying the chemical nature of the soil, had been taken up by the different solvents used.

In the case of the waters, the quantity employed for analysis was, with one exception, about two gallons; in that case, owing to a vessel having leaked (No. 3), but one gallon was employed. The waters were, in the first instance, very carefully filtered; and where any sensible quantity of sediment was found upon the filter, its nature and quantity observed.

The water was then evaporated, at first upon the sand-bath, but finally upon a water-bath, to perfect dryness, and the residue having been collected and dried at  $212^{\circ}$ , was weighed. It was then incinerated; the residue, moistened with carbonate of ammonia, again gently ignited and weighed. By the difference of weight, the quantity of organic matter present was ascertained in the state in which it exists when dried at  $212^{\circ}$  Fahrenheit.

The solid material thus obtained was treated with water, until all soluble salts were taken up, and the alkalies, lime, magnesia, with sulphuric and muriatic acids, therein determined. The undissolved residue was next treated with muriatic acid, and the amount and nature of the earthy substances taken up, as well as oxides of iron, &c., and phosphoric acid, if any, ascertained. The material insoluble in muriatic acid, when present, was of course determined.

The detailed modes of analyses pursued in these cases were precisely the same as in those of the ashes and soils.

In carrying out the greater part of the practical details of these analytical methods, I derived valuable aid from Mr. William Sullivan, then my private assistant, but now first chemical assistant in the Museum of Irish Industry, founded by Her Majesty's Government in Dublin.

## 2. Results of the Analyses of the Soils.

The general character of all the soils submitted to examination was that of light, sandy loams, in some cases almost purely sandy; excessively loose in texture, non-coherent and permeable: usually rich in organic matters containing nitrogen. These soils all coloured water boiled upon them, and gave to it a sensible, though very small, quantity of alkaline and earthy salts.

## A. Soil from Heestert, in the Courtrai district:—

	Composition per cent.
Potash . . . . .	0·160
Soda . . . . .	0·298
Peroxide of iron . . . . .	3·298
Oxide of manganese . . . . .	a trace
Alumina . . . . .	2·102
Lime . . . . .	0·357
Magnesia . . . . .	0·202
Sulphuric acid . . . . .	0·025
Phosphoric acid . . . . .	0·121
Chloride of sodium . . . . .	0·017
Organic matter and water } not driven off at 212°	3·123
Clay . . . . .	14·920
Sand . . . . .	75·080
	<hr/> 99·703
Loss . . . . .	0·297
	<hr/> 100·000

## B. Soil from Escamaffles, some of the very best flax lands of the Courtrai district:—

	Composition per cent.
Potash . . . . .	0·123
Soda . . . . .	0·146
Peroxide of iron . . . . .	1·663
Oxide of manganese . . . . .	a trace
Alumina . . . . .	1·383
Lime . . . . .	0·227
Magnesia . . . . .	0·153
Sulphuric acid . . . . .	0·017
Phosphoric acid . . . . .	0·152
Chloride of sodium . . . . .	0·030
Organic matter and water } not driven off at 212°	2·361
Clay . . . . .	9·280
Sand . . . . .	84·065
	<hr/> 99·600
Loss . . . . .	·400
	<hr/> 100·000

C. Soil from Hamme Zog, the best flax land in the Antwerp district :—

Composition per cent.	
Potash . . . . .	0·068
Soda . . . . .	0 110
Peroxide of iron . . . . .	1·202
Oxide of manganese . . . . .	a trace
Alumina . . . . .	1·125
Lime . . . . .	0·481
Magnesia . . . . .	0·140
Sulphuric acid . . . . .	0·013
Phosphoric acid . . . . .	0·064
Chloride of sodium . . . . .	0·067
Organic matter and water } not expelled at 212°	4·209
Clay . . . . .	5·760
Sand . . . . .	86·797
	<hr/>
	99·975
Loss . . . . .	·025
	<hr/>
	100·000

D. Soil from a district producing coarse flax and poor crops generally :—

Composition per cent.	
Potash . . . . .	0·151
Soda . . . . .	0·206
Peroxide of iron . . . . .	1·543
Oxide of manganese . . . . .	no trace
Alumina . . . . .	0·988
Lime . . . . .	0·366
Magnesia . . . . .	0·142
Sulphuric acid . . . . .	0·026
Phosphoric acid . . . . .	0·193
Chloride of sodium . . . . .	0·009
Organic matter and water } not expelled at 212° .	3·672
Clay . . . . .	4·400
Sand . . . . .	88·385
	<hr/>
	100·081

E. Soil from a district in Holland, where flax is well-grown :—

	Composition per cent.
Potash . . . . .	0·583
Soda . . . . .	0·306
Peroxide of iron . . . . .	6·047
Oxide of manganese . . . . .	a trace
Alumina . . . . .	5·626
Lime . . . . .	3·043
Magnesia . . . . .	0·105
Sulphuric acid . . . . .	0·023
Phosphoric acid . . . . .	0·159
Chloride of sodium . . . . .	0·023
Organic matter and water } not expelled at 212° . }	5·841
Clay . . . . .	17·080
Sand . . . . .	60·947
	<hr/> 99·783
Loss . . . . .	0·217
	<hr/> 100·000

Mr. Marshall was also kind enough to forward to me a specimen of the kind of soil which is found deposited in the Humber, and the gradual silting up of which has formed the extensive flat districts reclaimed along that eastern coast. This specimen of soil, or *warp*, as it is termed, from the operation by which the ground becomes permanently gained from the sea, had not yet borne any crop. It is from Crowle, in Lincolnshire.

Its composition per cent. was found to be as follows :—

Potash . . . . .	0·534
Soda . . . . .	0·083
Peroxide of iron . . . . .	4·500
Oxide of manganese . . . . .	a considerable trace
Alumina . . . . .	3·065
Lime . . . . .	5·538
Magnesia . . . . .	0·052
Sulphuric acid . . . . .	0·113
Phosphoric acid . . . . .	0·222
Chloride of sodium . . . . .	0·067
Organic matter and water } not expelled at 212° . }	5·328
Sand . . . . .	80·702
	<hr/> 100·204



By these analytical results, it is abundantly evident how completely due to artificial means is the fertility of those different Belgian soils; the large quantity of azotized organic matter, the proportionally large quantities of phosphoric acid and magnesia, and of the alkalies, being evidently the result of the copious treatment with animal manures, to which, as all persons conversant with Flemish agriculture are aware, the soil in Belgium is subjected. This will become still more evident, when hereafter I have to notice the course of cultivation which those soils are made to undergo. The duty, so important in the preparation of our Irish soils for flax, of dividing the soil to the finest possible state, and rendering it perfectly friable and porous, is seen by the above results to be naturally effected in the Belgian soils, of which a well-manured, incoherent sand, might be more correctly the title; none of them containing, except that marked A, and that from Holland, E, as much clay as would even justify the title of a light loam. There is, therefore, no doubt but that the soils most adapted for the successful growth of flax are of this very light and porous character; and that, in the selection of districts in this country into which the flax culture may be extended, this quality of lightness and permeability of soil is of the first importance.

The quantity of lime contained in the Belgian soils will be observed to be extremely small; but in that from Holland and from the Lincolnshire warped land it is much larger, indeed so as to constitute the most dominant earthy material. This has evidently had its cause in the source from whence these soils were derived, the silt deposited in shallow, quiescent waters by the sea, and which contains, mixed with sand, a proportion of comminuted shells or chalk. There is no positive evidence that this amount of lime is connected with any decided inferiority in the flax; but it is still worthy of attention, that the soil of the districts which have been longest and best known for the production of good flax have but a mere trace of lime in their constitution.

The comparatively large quantity of magnesia which the Belgian soils contain, and which is so remarkably contrasted with its inferior proportion in the warp soil, is, in my opinion, produced by the artificial manuring by animal liquids; and to this source also I attribute the great richness of these soils in phosphoric acid.

[To be continued.]

VII. *On the Colouring Matters of Madder.*  
By Dr. SCHUNCK\*.

THE organic colouring matters present such a wide field for inquiry, that it would require the labour of years to enable one person fully to elucidate their properties, or even to bring this department of organic chemistry into a state of development proportionate to the present condition of the science. The substances included under the name of colouring matters by no means agree in their chemical characteristics; they merely coincide in being possessed of certain vivid colours, or in giving rise to coloured compounds. Strictly considered, some of them ought to be classed among the resins and others among the extractive matters; and on the other hand, if we attempt a definition of the class according to their chemical characteristics, we shall find it impossible to exclude a large number of bodies, which, like tannin and catechin, are capable of giving rise under peculiar circumstances to brown substances, which in nowise differ in their general properties from the bright red colouring matters of archil, logwood, &c. Some colouring matters are presented to us ready formed in the different parts of plants and animals; others are produced artificially from colourless substances, which undergo very complex changes during the process; others arise spontaneously during the first stages of oxidation or putrefaction following the extinction of organic life. In the investigation of substances thus widely differing in properties and formation, it would be vain to expect at present anything approaching to general results in regard to the class as a whole. I must therefore content myself on this occasion with giving a short account of the results of some experiments which I have made on one branch of the subject, at the same time apologising for their present vague and undefined nature.

I have directed my attention in the first instance to madder, partly because the colouring matters contained in it are almost unknown, or rather worse than unknown, viz. known in such a manner as merely to mislead those who wish to inform themselves by the accounts given of them, and partly because madder is an article of such an immense importance in the art of dyeing that every discovery in relation to it acquires immediately a practical bearing.

It will be unnecessary for me to allude to the former numerous investigations of madder, except so far as to mention that Robiquet discovered in it a crystallized volatile colouring matter, which he called *Alizarin*, and that Runge described five colouring matters which he obtained from it, viz. *madder purple*, *madder red*, *madder orange*, *madder yellow* and *madder brown*. I may here state as one result of my investigation, that I agree with Runge in thinking that there is more than one colouring matter in madder, though I am of opinion that the substances which he enumerates and describes are not pure. Before however entering on this part of the subject, I shall first give the results at which I have arrived in regard to ali-

\* From Report of British Association for 1846.

zarin. Alizarin is doubtless the most interesting and the most definite in its nature of all the substances contained in madder. It also presents itself the most easily to the observer even on the most superficial examination. If we heat madder spread out in a thin layer on a metal plate without carrying the heat far enough to char the woody parts of the root, we shall in the course of a few hours find its surface covered with small red or orange-coloured crystals, which consist of alizarin. In the same way any extract of madder, whether with water, alcohol or alkalies, evaporated to dryness and gently heated, gives a crystalline sublimate of alizarin, which is variously coloured from a light yellow to a dark red or brown. Now one of the first points to be ascertained in regard to this body was whether it exists as such in the root, or whether it is formed by the process of sublimation. Robiquet, the discoverer, states that it pre-exists in the plant. He considered alizarin as the colouring principle of madder, and merely subjected it to sublimation for the purpose of purifying it. But his investigation presents us with no convincing proof of this opinion, for the extract of madder with water, alcohol, &c., from which he prepares his alizarin by sublimation, shows no trace of anything crystalline; and many chemists have asserted in consequence that it is a product of decomposition, being formed by the action of heat in the same way as pyrogallic, pyrotartaric acid, and many other bodies. I have however no hesitation in affirming that it exists in the plant as such, having in more than one way obtained it in a crystallized state without the intervention of heat. If we make an extract of madder with cold water, we obtain a brown fluid which produces no reaction on test-paper. After being exposed however to the action of the atmosphere for some hours, it acquires a distinctly acid reaction; and if it be now examined carefully, there will be found floating about in it a number of long hair-like shining crystals: these crystals are alizarin. If the fluid be still further exposed to the influence of the atmosphere, a yellow amorphous substance begins to separate, which I shall mention afterwards. This is succeeded by a gelatinous substance, and after some days a complete state of putrefaction ensues. It seems as if the alizarin in madder, or at all events that part which dissolves in the water, exists in combination with lime. On exposure to the atmosphere, there is formed, from some constituent of the root dissolved in the fluid through the instrumentality of the oxygen, some acid, which seizes hold of the lime in the solution and separates the bodies which are combined with the lime. Now the alizarin, being a body of very slightly acid properties, is separated first, and the other substances follow in succession. The fresher the madder is, the purer will be the alizarin, which separates on exposure to the atmosphere; in some instances it forms on the surface of the fluid a thick light yellow scum; but in most cases it is mixed with brown or red substances, from which it is separated with difficulty. It is therefore most advisable to separate the crystals which are deposited after twelve hours' standing, by filtration. These crystals are then washed from the filter and boiled with very dilute nitric acid until they have be-

come of a bright yellow colour. They are then dissolved in boiling alcohol, from which they separate on cooling in yellow transparent plates and needles having a strong lustre. Alizarin prepared in this way has the following properties:—It has a pure yellow colour without any admixture of red. It may be volatilized without leaving any residue. The vapour crystallizes on cooling in beautiful yellow plates and needles. It suffers hardly any change if exposed to the action of the most powerful reagents. It dissolves without change in cold concentrated sulphuric acid. Concentrated nitric acid hardly affects it even on boiling. It is not changed by chlorine. It is insoluble in water, but soluble in alcohol with a yellow colour. It dissolves in alkalies with a beautiful purple colour. Its compounds with the alkaline earths are red and slightly soluble in water. Its compounds with the earths and metallic oxides are insoluble in water and exhibit different shades of red. It imparts no colour to cloth mordanted with acetate of alumina or oxide of iron, on account of its insolubility in water. Very little alizarin is obtained in this way; perhaps one 1 gr. from 1 lb. of madder, though there is more of it contained in the root.

I shall now shortly describe two other colouring matters which I have obtained from madder. If an extract of madder be made with hot or cold water, and a strong acid, such as muriatic or sulphuric acid, be added to the fluid, a dark reddish-brown flocculent precipitate is produced. This precipitate was separated by filtration and washed until the acid was removed. On being treated with boiling water, a part of it dissolves with a brown colour. On adding a few drops of acid to the filtered solution a dark brown precipitate is produced, which seems to me to be a peculiar colouring matter similar in its properties to orcein, hematin and other soluble colouring matters. It dissolves in alkalies with a red colour, and is capable of imparting very lively colours to mordanted cloth. As far as I am aware it has not been described in the former investigations of this subject, though it seems to be the principal substance concerned in the production of the colours for which madder is used in the arts. I have however only examined it very slightly as yet. The residue left behind by the boiling water was treated with dilute boiling nitric acid, by which every trace of the preceding substance is destroyed, and the residue itself acquires a bright yellow colour and a more powdery consistence. This yellow powder contains alizarin, as is shown by its giving crystals of that substance on being gently heated; in fact it contains all the alizarin of the root, but mixed with another substance of an amorphous nature but very similar properties, from which it is difficult to separate it. By crystallising from alcohol no separation can be effected, as they are both about equally soluble in that menstruum. They also behave in a similar manner towards the alkalies, the earths and most of the metallic oxides. I have hitherto only succeeded in discovering one method of separating them, which is as follows:—The mixture of the two is dissolved in a little caustic potash. To the solution is added perchloride of iron, which produces a dark reddish-brown precipi-



tate consisting of peroxide of iron in combination with the two substances. Now on boiling this precipitate with an excess of perchloride of iron, the alizarate of iron dissolves, forming a dark brown solution, while the iron compound of the other substance remains behind. On adding muriatic acid to the filtered solution, the alizarin separates in yellow flocks and may be purified by crystallization from alcohol. The other substance, to which I have not yet given a name, is obtained by decomposing its iron compound, which remains behind on treating with perchloride of iron, with muriatic acid, and washing till all the oxide of iron is removed. It seems also to be a colouring matter, as it dissolves with a red colour in alkalies and gives red compounds with the earths and metallic oxides. It is insoluble in water, but soluble in alcohol with a yellow colour. It therefore resembles the resins in its general properties. It cannot be obtained in a crystallized state. From a hot concentrated solution in alcohol it separates on cooling as a yellow powder. It imparts no colour to mordanted cloth.

VIII. Comparative Analysis of the Urine of the Calf and the Sheep\*.

**M.** BRACONNOT finds that the urine of the calf, nourished by the milk of the mother, consists of—

	grs.
Ammoniac-magnesian phosphate . . . . .	0·18
Chloride of potassium . . . . .	3·22
Sulphate of potash . . . . .	0·44
Urinary animal matter } . . . . .	2·36
Urea . . . . .	
Phosphate of iron . . . . .	} . . traces
Phosphate of lime . . . . .	
Phosphate of potash . . . . .	
Combustible acid combined with potash . . . . .	
Silica . . . . .	
Mucus . . . . .	
Chloride of sodium ? . . . . .	
Water . . . . .	993·80
	1000·00

A litre of the urine of the sheep yielded—

	grs.
Chloride of potassium . . . . .	6·13
Sulphate of potash . . . . .	3·74
Carbonate of magnesia . . . . .	1·40
Urea . . . . .	} quantities undetermined.
Urinary animal matter . . . . .	
Hippurate of potash . . . . .	
Bicarbonate of potash . . . . .	
Carbonate of lime . . . . .	
Mucus . . . . .	
Oxide of iron . . . . .	

\* From the *Annales de Chimie et de Physique*, Juin 1847.  
*Phil. Mag. S. 3. Vol. 31. No. 205. July 1847.* E

IX. On the expected Reappearance of the celebrated Comet of 1264 and 1556. By Mr. HIND\*.

THE time is now near at hand when the return of the comet of 1264 and 1556, signalled by Mr. Dunthorne and M. Pingré, may be expected to take place. It is therefore desirable that observers should be in possession of everything that may tend to facilitate their search for the comet; and I venture to communicate to the Society the results of some recent calculations of my own on the subject, preceded by a very brief view of the principal circumstances connected with former appearances of the comet, and a short notice of calculations already published.

“The great and celebrated comet” of 1264, as Pingré terms it, is mentioned by nearly all the European historians of the time, and was observed by the astronomers of the dynasties then reigning in the north and south of China. It is described as presenting a most imposing appearance, with a tail  $100^{\circ}$  in length, stretching from the east part of the “mid-heaven.” The comet was of “surprising magnitude,” far exceeding any remembered by those who beheld it. Contemporary writers generally considered it the precursor of the death of Pope Urban IV., and many of them relate that it disappeared on the same night that the pope died, or on October 2; thus, in the words of Thierry de Vaucouleurs,

“Quo (Urbano) moriente, velut mortem cognosceret ejus,  
Apparens minimè stella comata fuit.”

In 1556 the appearance of the comet was not on the same scale of splendour as in 1264, but still was sufficiently imposing to call forth from historians the epithets “ingens et lucidum sidus.” It was observed by Paul Fabricius, a mathematician and physician at the court of the emperor Charles V. of Austria. M. Pingré, the celebrated cometographer, sought in vain for the original observations; the only information he could find on the subject was contained in a small rough chart found in Lycosthenes and other authors. I have before† suggested the probability that these observations were given by Fabricius in his work upon the comet, published at Nürnberg in 1556, and mentioned by Lalande in his *Bibliographie*; but, as far as I am aware, this work has not been discovered in any library. M. Pingré would have at his command the splendid collections of St. Geneviève and the Royal Library at Paris; and his ineffectual search for the observations in these libraries makes it at least doubtful whether they are now in existence. The chart just mentioned enables us to form a tolerably definite

\* From the Proceedings of the Royal Astronomical Society, No. 14.

† *Ast. Nach.* 493.

idea of the path followed by the comet, and we have ample information for a rough determination of the elements.

When Halley published his *Synopsis of Cometary Astronomy*, he gave a set of parabolic elements for the comet of 1556, founded upon the observations made by Paul Fabricius; but he remarks that these elements are not so certain as those of other comets he had computed, the observations being made "neither with sufficient instruments nor due care," and by no means to be reconciled with any regular calculation.

The elements of the comet of 1264 were first computed by Mr. Dunthorne. His discussion of the observations and circumstances relating to the comet's apparition are published in vol. xlvii. of the *Philosophical Transactions*. The elements are chiefly founded on the authority of a manuscript preserved in the library of Pembroke Hall College, Cambridge, entitled *Tractatus fratris Ægidii de Cometis*. But it must be observed there are manifest contradictions in this account not easily set right. The other authorities consulted were the *Chronicon Sampetrinum Erphurtense* and the *Chronicle of John Vitoduranus*. The orbit deduced by Mr. Dunthorne much resembles that calculated by Halley for the comet of 1556.

In the *Memoirs of the Royal Academy of Sciences at Paris* for 1760, appears a valuable memoir by M. Pingré on the comet of 1264. After collecting together a great number of accounts from different chronicles and histories of the day, he proceeds to the discussion of the elements. The contradiction in the Cambridge manuscript which relates to the comet's motion in longitude is pointed out; and since this manuscript was Mr. Dunthorne's chief authority, it might be supposed that his orbit would differ entirely from M. Pingré's. This, however, was not the case; for although there are differences of some moment in one or two of the elements, there is still a striking similarity between the two orbits taken as a whole, and M. Pingré's approaches much nearer than Mr. Dunthorne's to the orbit of the comet of 1556. A closer agreement might have been produced if he had not wished to preserve the path laid down by *Thierry de Vaucouleurs* with as little alteration as possible. M. Pingré concludes from his researches that there is little doubt of the identity of the comets of 1264 and 1556, and, therefore, that the return to perihelion may be expected to take place in the year 1848. In No. 493 of the *Astronomische Nachrichten* will be found the results of my first calculations relating to this comet. I have there deduced elements from the observations by Fabricius in 1556, and computed an ephemeris for comparison with the comet's observed path. The agreement, though not so close as could be wished, was the best that could be obtained from the data

given by M. Pingré in his *Cometography*. I then reduced the elements to the year 1264, and with the assistance of a passage in 'Thierri's poem, I fixed the time of perihelion for July 9·9 (old style). The passage alluded to is as follows:—

“Undecimumque gradum Phœbo superante Leonis,  
Ter deno Cancri restitit illa loco.”

With M. Pingré, I have understood by “*Ter deno Cancri*,” the 120th degree of longitude; but I am not quite sure that this is the true interpretation.

With perihelion and node reduced as before stated, and the other elements as for 1556, an ephemeris of the comet's geocentric path in 1264 was computed. During the month of July, calculation and observation agree pretty well; but after the beginning of August the theoretical places entirely differ from the positions of the comet, as deduced from the accounts. Instead of traversing Orion towards the end of its appearance, as some historians relate, it would take a higher declination, passing through Auriga and Taurus.

Since the publication of this paper in the *Astronomische Nachrichten*, I have made some further investigations on the subject, and with more success than in my first calculations. A closer comparison of data showed pretty clearly that the observation of March 5, on which I had chiefly relied, must be erroneous as it is given by M. Pingré. In tome i. of his *Cometography*, p. 503, we learn that on March 5 the comet was almost in the right line joining the stars  $\gamma$  and  $\theta$  Virginis, and was equidistant from the stars. A trigonometrical calculation from these data gives the place of the comet in longitude  $188^{\circ} 1'$ , and latitude  $+2^{\circ} 19'$ , and this position was employed in my earlier investigations. But I have recently satisfied myself, that the observation as given above cannot be reconciled with those of March 3 and 4, and on subsequent days, by any set of elements. The cause of this anomaly is, I believe, an error in the name of the star. If instead of  $\gamma$  and  $\theta$  Virginis we read  $\delta$  and  $\theta$ , then the place of the comet would be in longitude  $188^{\circ} 41'$ , and latitude  $+5^{\circ} 13'$ , which agrees very well with the track which the comet ought to have followed, according to the other observations.

A recalculation of the elements from an interpolated position for March 5, and from those of March 9 and 14, gives the following values:—

Passage through perihelion, 1556, April 22·0233, G. M. T. [Old style.]

Longitude of perihelion .....	274 14·9	} Equinox of 1556.
Ascending node .....	175 25·8	
Inclination .....	30 12·2	
Log. least distance.....	9·70323	
Motion direct.		



The following ephemeris of the comet for the appearance in 1556, Greenwich mean midnight, old style, is deduced from these elements:—

1556. Old style.	Geo. long.	Geoc. lat.	Log. r.	Δ.
March 3	188° 13'	+ 1° 9'	0.0732	0.193
4	188 0	3 40	0.0670	0.175
5	187 44	6 45	0.0606	0.157
6	187 22	10 36	0.0541	0.140
7	186 54	15 29	0.0476	0.124
8	186 14	21 43	0.0409	0.109
9	185 18	29 49	0.0341	0.096
10	183 49	40 12	0.0272	0.085
11	181 11	52 50	0.0201	0.078
12	175 21	67 5	0.0130	0.075
13	153 35	80 29	0.0057	0.078
14	55 19	82 30	9.9983	0.085
15	27 16	73 26	9.9908	0.095
16	20 37	65 30	9.9831	0.108
17	17 44	59 16	9.9753	0.122
27	12 19	34 58	9.8903	0.302
April 6	12 7	27 1	9.7959	0.505
16	14 13	20 30	9.7178	0.733
26	19 12	+13 52	9.7130	0.974

If this ephemeris be compared with the descriptions of the comet's apparent path in the heavens, we shall find the agreement as close as could be expected, considering the uncertainty and irregularity of the data.

With the above elements reduced to 1264, the time of perihelion was found to be July 13.42, *i. e.* assuming with Pingré, that the comet was in longitude 120° when the sun had reached the 11th degree of Leo, according to the narration of Thierri de Vaucouleurs. The geocentric places of the comet, Greenwich mean midnight, old style, would then be as follows:—

1264. Old style.	Geo. long.	Geoc. lat.	r.	Δ.
July 7	138° 10'	+18° 14'	0.53	0.82
17	132 36	22 9	0.51	0.62
22	126 29	21 54	0.55	0.55
27	118 36	20 14	0.61	0.48
Aug. 6	101 14	+10 17	0.75	0.41
16	85 23	— 3 47	0.92	0.39
26	70 47	17 10	1.09	0.42
Sept. 5	56 39	27 8	1.26	0.48
15	43 11	33 4	1.43	0.57
25	31 35	35 26	1.59	0.69
Oct. 5	22 47	—35 30	1.75	0.84

If we are to depend solely on the European accounts of this comet's path, the above is liable to two objections: first, too high a declination in August; and secondly, that the posi-

tions are in Eridanus during the latter part of the comet's apparition; historians generally contenting themselves with stating that the comet "finally traversed *Orion*." M. Pingré's elements, which are not open to these objections, do not agree so well as mine with the more circumstantial details left us in the Chinese annals. The two orbits differ chiefly in the longitude of the node and perihelion distance, but the discordances are by no means great.

The results of my calculations have satisfied me that the comet of 1264 was, in all probability, the same as that of 1556, and consequently, that its return to perihelion must be very near at hand. The nodes of the comet's orbit lie very close to the earth's path. The ascending node is passed fifty days before perihelion, the radius vector being 1.193, and consequently the distance outside the earth's orbit about 0.197. The passage through descending node occurs  $31\frac{1}{2}$  days after perihelion, and the distance of the point from the earth's orbit inside is 0.126. However, the nearest approach of the comet to the earth will not happen at the nodes, but soon after its passage through them; thus in 1556 the least distance between the two bodies was 0.074, nine days after the transit through ascending node. The effect of this close proximity to our globe on the period of revolution of the comet has been investigated by Professor Mädler, of the Dorpat Observatory, as detailed in No. 501 of the *Astronomische Nachrichten*; it amounted to  $14\frac{1}{2}$  days only, and the return of the comet to perihelion was fixed for the end of February 1848.

The following table contains the heliocentric co-ordinates referred to the equator and the log. radii vectores of the comet in my last orbit, reduced to 1848, for every tenth day, from ninety days before to 90 days after perihelion.

Time from perihelion pass.	<i>x</i> .	<i>y</i> .	<i>z</i> .	Log. <i>r</i> .
Days.				
—90	—1.7430	+0.5750	—0.0603	0.2640
80	1.6231	0.4370	0.0445	0.2257
70	1.4931	0.2963	0.0284	0.1826
60	1.3504	0.1533	—0.0122	0.1333
50	1.1917	+0.0084	+0.0041	0.0762
40	1.0120	—0.1363	0.0206	0.0092
30	0.8039	0.2770	0.0363	9.9300
20	0.5570	0.4031	0.0501	9.8385
—10	—0.2611	0.4907	0.0592	9.7474
0	+0.0738	0.4961	0.0583	9.7032
+10	0.3929	0.3951	0.0450	9.7474
20	0.6507	0.2266	0.0239	9.8385
30	0.8503	—0.0352	+0.0004	9.9300
40	1.0086	+0.1590	—0.0233	0.0092
50	1.1385	0.3490	0.0463	0.0762
60	1.2484	0.5331	0.0685	0.1333
70	1.3433	0.7109	0.0900	0.1826
80	1.4268	0.8828	0.1107	0.2257
+90	+1.5015	+1.0492	—0.1307	0.2640

With the above values for  $x$ ,  $y$  and  $z$ , and those of  $X$ ,  $Y$ ,  $Z$ , taken from the Nautical Almanac, the position of the comet for different suppositions as to the time of passage through perihelion may be readily obtained. If we suppose March 0, which is about the epoch fixed by Professor Mädler, we shall have the following ephemeris for facilitating the discovery of the comet, mean noon at Greenwich:—

1847—8.	R.A.	Decl.	$\Delta$ .
Dec. 1	187° 16'	—11° 22'	2.16
11	193 55	12 56	1.92
21	201 52	14 29	1.68
31	211 43	15 52	1.46
Jan. 10	224 16	16 50	1.26
20	240 18	16 47	1.11
30	259 53	15 3	1.02
Feb. 9	281 23	11 24	1.03
19	302 15	7 1	1.13
29	321 18	3 23	1.29
Mar. 10	338 5	— 0 54	1.48
20	352 11	+ 0 50	1.66
30	3 50	2 9	1.84
April 9	13 32	3 11	2.01
19	21 46	3 59	2.17
29	28 52	4 35	2.32
May 9	35 5	4 58	2.46
19	40 36	5 11	2.59
29	45 31	+ 5 12	2.69

It appears from this ephemeris, that according to the most probable supposition we can make respecting the time of perihelion without actual calculation of the perturbations, the position of the comet in the heavens during the approaching reappearance will be extremely unfavourable for observation; and it is therefore the more desirable that those who look out for comets should be on the alert. Nearly the whole of the vast trajectory of this comet lies below the plane of the ecliptic, and *far from the paths of the larger planets*, but it extends into space more than twice the distance of Neptune; and surely we are not yet able to say what causes may operate, at this immense distance from the sun, to affect the time of the next return to perihelion. If however the comet can be detected and observed, we shall then have the means of ascertaining something more on these points

X. *Analysis of the Water of the Thermal Spring of Bath (King's Bath).* By Messrs. GEORGE MERCK and ROBERT GALLOWAY\*.

THE water of this celebrated spring, the efficacy of which was known in the time of the Romans, has been analysed repeatedly by various chemists at different periods. Richard Phillips†, Scudamore‡, Walker§, and more recently Noad||, have occupied themselves in the investigation of this water. In their several analyses, the whole amount of the fixed ingredients of the water agrees very closely; but in regard to the composition of these substances there are considerable discrepancies, as may be seen in a table which we have annexed at the end of this paper.

Besides great differences in the quantitative analysis, we find discrepancies even in regard to the presence and absence of certain constituents. Among the chemists that have been mentioned, Walker is the only one who has recognised the presence of potash. The same chemist corroborated Scudamore's statement as to the presence of magnesia, overlooked by their predecessors; but he states also that he detected alumina, which none of the others found. In all these analyses iodine has been omitted. Mr. Cuff¶ however has indicated the presence of this element in the spring.

These discrepancies made another investigation of the mineral water of Bath very desirable; the following analysis was performed at the suggestion of Dr. A. W. Hofmann.

To obtain the water genuine, and especially for the purpose of ascertaining the amount of free carbonic acid it contained, we collected the water ourselves, an operation in which we were kindly assisted by Messrs. Green and Simms, lessees of the establishment.

The water was taken from the principal well, which supplies the King's and Queen's baths, which are the most esteemed and valued in the city. Of the two other wells, one supplies the Hot Bath and the other the Cross Bath, which are in the neighbourhood of those first mentioned.

\* Communicated by the Chemical Society; having been read Nov. 16, 1846.

† An Analysis of the Bath Water, by Richard Phillips. London, 1806.

‡ A Chemical and Medical Report of the properties of the Mineral Waters of Buxton, Matlock, &c., by Ch. Scudamore, M.D. 1820.

§ Quarterly Journal of Science, Literature and Arts, vol. xxvii. 78. 1829.

|| Pharmaceutical Journal, vol. iii. 526.

¶ Memoir on the occurrence of Iodine and Bromine in certain Mineral Waters of South Britain, by Charles Daubeny; Transactions of the Royal Society of London, 1830, ii. p. 223.



The King's Bath is an oblong cistern, 65 feet long and 40 feet broad, in which the water stands at the height of 46 inches. It is supplied from the bottom by means of twelve large and about twenty smaller apertures. By far the largest amount of water rises however from an opening made in the centre of the bath, 18 inches in diameter. Although the water flows under the influence of a very small pressure, the quantity is such, that the two reservoirs, the King's and the Queen's bath, are entirely filled in about nine hours. The quantity of water entering each minute is 126 gallons, upon the authority of Dr. Daubeny\*.

### I. *Qualitative Analysis.*

The water as it issues from the well has a temperature of  $46^{\circ}$  C. ( $115^{\circ}$  Fahr.), the temperature of the air being  $20^{\circ}$  C. ( $68^{\circ}$  Fahr.); it is clear and without odour, and has no effect upon vegetable colours; it has a saline and slight iron taste; the iron is deposited as sesquioxide in rather large quantities in the pipes leading from the well.

The following experiments gave the qualitative composition of the mineral water; on boiling for some time a white crystalline precipitate formed. The qualitative analysis was therefore divided into two parts.

- a. The analysis of the precipitate formed on boiling.
- b. The analysis of the substances remaining dissolved.

#### a. *Analysis of the Precipitate formed on boiling.*

1. The precipitate was treated with hydrochloric acid; a small portion of it dissolved with effervescence, indicating the presence of carbonic acid. The portion insoluble in hydrochloric acid dissolved on the addition of a large quantity of water:—*Indicating sulphate of lime.*

Another portion of the water was boiled some time, with the precaution of replacing the evaporated water, in order that all the sulphate of lime should remain in solution; in this case only a very small precipitate was formed, which was entirely soluble in hydrochloric acid.

2. On heating this solution and adding ammonia, a very slight flocculent precipitate of a yellowish-white colour was produced after some time:—*Indicating oxide of iron.*

3. In the filtrate from the sesquioxide of iron (2.), on the addition of oxalate of ammonia, a white precipitate was formed:—*Indicating salts of lime.*

\* On the Quantity and Quality of the Gases disengaged from the Thermal Spring which supplies the King's Bath in the City of Bath, by Charles Daubeny; Transactions of the Royal Society of London, 1834, I. i.

4. In the liquid filtered off from the oxalate of lime (3.), phosphate of soda produced an exceedingly slight crystalline precipitate:—*Showing the presence of magnesia.*

*Note.*—This precipitate could only be distinctly seen in testing a large quantity of the water.

*b. Analysis of the substances remaining dissolved.*

The liquid which was filtered from the precipitate (a.) formed on boiling had no alkaline reaction; a portion of it was evaporated nearly to dryness and treated with hydrochloric acid; no carbonic acid was evolved, from which comportment the absence of alkaline carbonates could with safety be concluded.

1. A portion of the liquid gave on addition of chloride of barium a copious white precipitate, insoluble in hydrochloric acid:—*Indicating sulphuric acid.*

2. In another portion of the liquid nitrate of silver produced a copious white precipitate, easily soluble in ammonia:—*Evidencing the presence of chlorine.*

3. The entire solubility of the silver precipitate seemed to indicate the absence of iodides. To make ourselves perfectly certain of the absence of these salts, 30 or 40 pounds of the water were evaporated to 2 or 3 pounds, and the liquid filtered off from the precipitate which had been formed; a part of this fluid was evaporated with precaution to dryness, the residue was mixed with some starch paste, and a few drops of nitric acid being added, feeble but distinct violet spots were observed: this experiment was repeated several times with the same success:—*Indicating the presence of iodine.*

4. Another portion of the liquid (b.) was treated with hydrochloric acid, evaporated to dryness, and gently ignited: on treating the residue with a large quantity of water an insoluble portion remained:—*Showing the presence of silicic acid.*

5. Another portion of the liquid (b.) gave, on addition of chloride of ammonium and oxalate of ammonia, a white precipitate:—*Indicating lime.*

6. On adding, to a portion of the filtrate, ammonia and phosphate of soda, a slight crystalline precipitate was formed:—*Indicating magnesia.*

7. For the discovery of the alkalies, the remaining portion of the filtrate from the lime precipitate was evaporated to dryness, and the residue ignited until the ammoniacal salts had been expelled. The ignited residue was then dissolved in water, the sulphuric acid and magnesia precipitated by baryta water, and after separation of the excess of baryta by means of carbonate of ammonia, the filtrate evaporated to dryness

and ignited. The residue imparted a yellow colour to the blowpipe flame:—*Evidencing the presence of soda.*

An alcoholic solution of the residue gave with a concentrated solution of bichloride of platinum a yellow crystalline precipitate:—*Indicating potassa.*

The precipitate which had formed on evaporating for the iodine determination, was treated with hydrochloric acid, the filtrate saturated with ammonia and precipitated by sulphide of ammonium; this precipitate was re-dissolved in nitro-hydrochloric acid mixed with chloride of ammonium, and the sesquioxide of iron separated by ammonia. The filtrate, evaporated and fused with nitrate of potash and carbonate of soda, gave a green mass:—*Showing traces of manganese.*

Lithia, alumina, bromine and phosphoric acid were found to be absent.

In regard to the presence of gases in the water, it was scarcely necessary to test for the presence of free carbonic acid. On mixing a solution of lime with the mineral water a precipitate was formed, which dissolved in an excess of the mineral water. The quantity of free carbonic acid however is not very large; the water has no reaction on blue vegetable colours; hydrosulphuric acid is not contained in the water. Acetate of lead gave only a white precipitate of sulphate of lead free from all trace of brown colour, which might indicate the presence of sulphur.

A large quantity of gas is continually disengaged from the chief spring as well as from the secondary ones. Dr. Daubeny\* paid particular attention to the composition of this gas. He found that it consists principally of nitrogen, together with small quantities of carbonic acid and oxygen.

He employed a peculiar apparatus, constructed on purpose for these experiments, by which he was enabled to collect the whole of the gases from the principal well, as well as from those adjoining it. The experiments of Daubeny are so numerous and accurate as to preclude any other researches on the subject.

## II. Quantitative Analysis.

### *Determination of the Specific Gravity.*

A small bottle, which contained at the temperature of  $16^{\circ}\cdot5$  C. ( $60^{\circ}$  Fahr.) 10 grms. of distilled water, contained at the same temperature 10·025 grms. of the mineral water; from this the specific gravity of the water is calculated as 1·0025.

\* Vide Memoir mentioned.

1. *Estimation of Sulphuric Acid.*

The mineral water was heated with a little hydrochloric acid and chloride of barium added.

I. 534.199 grms. of water gave 1.340 gm. of sulphate of baryta = 0.4605 gm., or 0.08620 per cent. of sulphuric acid.

II. 475.003 grms. of water gave 1.1791 gm. of sulphate of baryta = 0.4050 gm., or 0.08526 per cent. of sulphuric acid.

Mean of the results, 0.08573 per cent.

2. *Estimation of Chlorine.*

The water was treated with nitric acid and precipitated by nitrate of silver; the precipitated chloride of silver was washed by decantation, fused and weighed.

I. 101 grms. of water gave 0.1137 gm. of chloride of silver = 0.2811 gm., or 0.02778 per cent. of chlorine.

II. 100.006 grms. of water gave 0.1093 grms. of chloride of silver = 0.02702 grms., or 0.02701 per cent. of chlorine.

Mean of the results, 0.02739 per cent.

3. *Estimation of Silicic Acid.*

To the water was added nitric acid in excess; it was then evaporated to dryness and the residue for some time heated on the sand-bath. On treating this residue with water and hydrochloric acid, the silicic acid remained behind; it was collected, washed and weighed.

I. 765.325 grms. of the water gave 0.0342 gm., or 0.00446 per cent. of silicic acid.

II. 732.015 grms. of water gave 0.0289 gm., or 0.00407 per cent. of silicic acid.

Mean of the results, 0.00426 per cent.

4. *Estimation of Iron.*

The iron was estimated,—

A. In the precipitate formed on boiling the mineral water.

B. In the water which had not been boiled.

Both estimations gave the same results.

A. Estimation of the iron in the precipitate :—

A certain quantity of the water was boiled for some time; the precipitate which had formed was washed, dissolved in hydrochloric acid and precipitated by an excess of ammonia.

I. 777.215 grms. of water gave 0.0079 gm., or 0.00101 per cent. of sesquioxide of iron.

B. Estimation of the iron in the water which had not been boiled :—



The liquid filtered off from the silicic acid (3.) was concentrated and precipitated by an excess of ammonia.

II. 765.325 grms. of water gave 0.0078 grm., or 0.00101 per cent. of sesquioxide of iron.

III. 732.015 grms. of water gave 0.0086 grm., or 0.00116 per cent. of sesquioxide of iron.

Mean of the results, 0.00106 per cent., corresponding to 0.00153 per cent. of carbonate of oxide of iron.

### *5. Estimation of Lime.*

The estimation of the lime was divided into—

A. Estimation of the lime contained in the water in the state of carbonate.

B. Estimation of the lime contained in the water in the state of sulphate.

C. Estimation of the total amount of lime for control.

A. Estimation of the lime combined with carbonic acid:—

The ammoniacal liquor filtered off from the precipitate of sesquioxide of iron was precipitated by oxalate of ammonia; the oxalate of lime was converted in the known way into carbonate.

I. 712.747 grms. of water gave, on boiling, a precipitate containing 0.0904 grm. of carbonate of lime = 0.05062 grm., or 0.00712 per cent. of lime.

II. 623.881 grms. of water gave, on boiling, a precipitate containing 0.0782 grm. of carbonate of lime = 0.0437 grm., or 0.00700 per cent. of lime.

Mean of the results, 0.00706 per cent.

B. Estimation of the lime combined with sulphuric acid:

The mineral water was kept boiling for one or two hours, replacing the water which evaporated; the precipitate formed was filtered off, washed, and to the filtrate was added chloride of ammonium, ammonia, and oxalate of ammonia; the oxalate of lime was converted into carbonate.

I. 710.747 grms. of water gave in this way 0.6072 grm. of carbonate of lime = 0.3400 grm., or 0.04783 per cent. of lime.

II. 623.881 grms. of water gave 0.5165 grm. of carbonate of lime = 0.2892 grm., or 0.04635 per cent. of lime.

Mean of the results, 0.04709 per cent.

C. Estimation of the total amount of lime for control:—

The ammoniacal liquid which was filtered off from the precipitate of sesquioxide of iron was precipitated after the addition of chloride of ammonium by oxalate of ammonia, and the oxalate of lime converted into carbonate.

I. 765.325 grms. of water gave 0.7211 gm. of carbonate of lime = 0.403816 gm., or 0.05276 per cent. of lime.

II. 732.015 grms. of water gave 0.6981 gm. of carbonate of lime = 0.3909 gm., or 0.05340 per cent. of lime.

Mean of the results, 0.05308 per cent.

Mean of the lime combined with carbonic acid 0.00706

Mean of the lime combined with sulphuric acid 0.04709

Total amount found by addition 0.05415

Mean of the total amount found by direct estimation . . . . . } 0.05308

### 6. *Estimation of Magnesia.*

The estimation of the magnesia was divided in the same manner as the estimation of lime into—

A. Estimation of the magnesia combined with carbonic acid.

B. Estimation of the magnesium combined with chlorine.

C. Estimation of the total amount of magnesia for control.

A. Estimation of magnesia contained in the water as carbonate:—

To the liquid filtered off from the oxalate of lime was added phosphate of soda; on stirring, after some time a precipitate of phosphate of magnesia and ammonia was formed, which was converted by ignition into pyrophosphate of magnesia.

I. 777.215 grms. of water gave, on boiling, a precipitate which contained 0.0046 gm. of pyrophosphate of magnesia = 0.001685 gm., or 0.00021 per cent. of magnesia.

II. 623.881 grms. of water gave, on boiling, a precipitate which contained 0.0044 gm. of pyrophosphate of magnesia = 0.00016 gm., or 0.00025 per cent. of magnesia.

Mean of the results, 0.00023 per cent.

B. Estimation of the magnesia contained in the water as chloride of magnesium.

The liquid filtered off from the oxalate of lime was concentrated by evaporation, ammonia added filtered off from a small portion of silicic acid which separated, and the magnesia precipitated by phosphate of soda.

I. 414.279 grms. of water gave in this way 0.1007 gm. of pyrophosphate of magnesia = 0.03689 gm., or 0.008906 per cent. of magnesia.

II. 427.1 grms. of water gave 0.1050 gm. of pyrophosphate of magnesia = 0.03846 gm., or 0.009004 per cent. of magnesia.

Mean of the results, 0.008955 per cent.

C. Estimation of the total amount of magnesia for control:—

The liquid filtered off from the precipitate of oxalate of lime was concentrated, ammonia and phosphate of soda added.

I. 765·325 grms. of water gave 0·1936 gm. of pyrophosphate of magnesia = 0·070929 gm., or 0·00926 per cent. of magnesia.

II. 732·015 grms. of water gave 0·1837 gm. of pyrophosphate of magnesia = 0·0673 gm., or 0·00919 per cent. of magnesia.

Mean of the results, 0·00922 per cent.

Mean of the magnesia combined with carbonic acid 0·00023

Mean of the magnesia contained in the water as }  
chloride of magnesium . . . . . } 0·00895

Total amount found by addition 0·00918

Mean of the total amount found by direct estimation 0·00922

### 7. *Estimation of the Alkalies.*

For the estimation of the alkalies the mineral water was evaporated to one-third of its volume and baryta water added in excess, the precipitates of sulphates of baryta, lime, magnesia and sesquioxide of iron were filtered off, and the excess of baryta precipitated by means of carbonate of ammonia. To get rid of the silicic acid the filtrate was evaporated to dryness with hydrochloric acid, gently ignited, dissolved in water, again filtered and evaporated to dryness; the mixed chlorides obtained in this manner were weighed.

I. 632·481 grms. of the mineral water gave 0·2937 gm. of chloride of sodium and chloride of potassium = 0·04643 per cent. of the mixed chlorides.

II. 546·032 grms. of water gave 0·2538 gm. of chlorides of sodium and potassium = 0·04648 per cent. of the mixed chlorides.

Mean of the results, 0·04645 per cent.

### 8. *Estimation of the Potassa.*

The chlorides of potassium and sodium were dissolved in a small quantity of water and an excess of bichloride of platinum added; the liquid was then evaporated to dryness in the water-bath, the residue digested with alcohol, the insoluble chloride of platinum and potassium filtered off from the soluble sodium salt and washed with alcohol; the precipitate was dried in the water-bath and weighed.

I. 632·481 grms. of the mineral water, or 0·2987 gm. of the mixed chlorides, gave 0·124 gm. of chloride of platinum

and potassium = 0.0378 grm. of chloride of potassium = 0.00597 per cent. of chloride of potassium, which equals 0.00377 per cent. of potassa.

II. 546.032 grms. of water, or 0.2538 grm. of the mixed chlorides, gave 0.0975 grm. of chloride of platinum and potassium = 0.02977 grm. of chloride of potassium = 0.00545 per cent. of chloride of potassium, which equals 0.00342 per cent. of potassa.

Mean of the results, 0.00571 per cent. of chloride of potassium and 0.0359 per cent. of potassa.

### 9. *Estimation of the Soda.*

The quantity of soda was found simply by the difference of the mixed chlorides and the quantity of chloride of potassium found by direct estimation.

Mean of the mixed chlorides . . .	0.04645
Mean of the chloride of potassium . .	0.00571
Chloride of sodium . . .	0.04074

corresponding to 0.02168 per cent. of soda.

### 10. *Estimation of Carbonic Acid.*

To find the quantity of free carbonic acid contained in the water at the moment it was taken from the well, a siphon of exactly known capacity was immersed in the well, and the water obtained in this way put in bottles, containing a mixture of ammonia and chloride of calcium. In this way the free carbonic acid as well as the carbonic acid in combination was precipitated in the form of carbonates. Four bottles were filled with mineral water by this method. The capacity of the siphon was exactly 533 cubic centimetres, therefore  $533 \times 4 \times 1.0025 = 2137$  grms. of water were taken.

The precipitate from the water contained in these four bottles was collected, washed, dried and weighed; it yielded 1.4748 grm. of carbonate mixed with some alumina from impurity in the solution of chloride of calcium.

To estimate the quantity of carbonic acid in this precipitate, two portions of it were taken and estimated separately after the method proposed by Drs. Fresenius and Will.

I. 0.66 grm. of the carbonate, &c. gave in this way 0.22 grm. of carbonic acid, therefore 1.4748 grm. of the carbonate, or 2137.0 grms. of water, gave 0.4916 grm. of carbonic acid.

II. 0.718 grm. of the carbonate, &c. gave 0.23 grm. of carbonic acid, therefore 1.4748 grm. of the carbonate, &c., or 2137.0 grms. of water, gave 0.4718 grm. of carbonic acid.



*Mean of the results.*

0·4817 grm. of carbonic acid, which equals 0·02254 per cent.

Total amount of carbonic acid . . . 0·02254

Carbonic acid existing in combination—

With oxide of iron . . . 0·00057

With lime . . . . . 0·00554

With magnesia . . . . . 0·00024

Sum total . . . . . 0·00635

Free carbonic acid remaining 0·01619

From the details contained in the preceding pages, it follows that the thermal spring in the King's Bath contains the following constituents in 100 parts:—

Carbonate of lime . . . . .	0·01260
Carbonate of magnesia . . . . .	0·00047
Carbonate of oxide of iron . . . . .	0·00153
Sulphate of lime . . . . .	0·11436
Sulphate of potassa . . . . .	0·00663
Sulphate of soda . . . . .	0·02747
Chloride of sodium . . . . .	0·01806
Chloride of magnesium . . . . .	0·02083
Silicic acid . . . . .	<u>0·00426</u>
	0·20620

Traces of manganese and iodine.

*Estimation of the total amount of the fixed ingredients in the water for control.*

The water was concentrated in a porcelain dish, and afterwards evaporated to dryness in a platinum basin. The residue was heated in an air-bath until the weight was constant. Two estimates were made.

I. 217·058 grms. of water gave 0·4540 grm., or 0·20916 per cent. of residue.

II. 319·57 grms. of water gave 0·6726 grm., or 0·21040 per cent. of residue.

Mean of the results, 0·20978 per cent.

But in this experiment the iron was obtained in the state of sesquioxide, whilst in the preceding calculation it is taken as the carbonate of the oxide, in which form it exists in the water.

On calculating the absolute weights from the above, we obtain the following numbers:—

*Phil. Mag. S. 3. Vol. 31. No. 205. July 1847.*

F

## 66 *Analysis of the Water of the Thermal Spring of Bath.*

	In a litre.	In an imperial gallon (70,000 grs.).
Carbonate of lime . . .	0·1260 grm.	8·82000 grs.
Carbonate of magnesia . .	0·0047 ...	0·32900 ...
Carbonate of oxide of iron .	0·0153 ...	1·07100 ...
Sulphate of lime . . . .	1·1436 ...	80·05200 ...
Sulphate of potassa . . .	0·0663 ...	4·64100 ...
Sulphate of soda . . . .	0·2747 ...	19·22900 ...
Chloride of sodium . . .	0·1806 ...	12·64200 ...
Chloride of magnesium . .	0·2083 ...	14·58100 ...
Silicic acid . . . . .	0·0426 ...	2·98200 ...
	0·20621...	144·01800 ...

According to our experiments, 1 litre of the water contains 95·64 cubic centimetres of free carbonic acid at the temperature of 46° C. (115° F.) and normal atmospheric pressure.

One imperial gallon contains therefore 26·45 cubic inches of free carbonic acid of 46° C., being more than double the quantity which has been determined by former experiments.

This however is not surprising, as the estimations previously made had been effected by the expulsion of the carbonic acid from the water. Besides the difficulty of avoiding a loss of carbonic acid before the operation, it is scarcely possible, as Mr. Phillips justly notices in his paper, to expel all carbonic acid by simple ebullition. Besides, we see from the experiments of Daubeny, that the gas which escapes from the well contains at different periods highly varying amounts of carbonic acid. He found by several experiments that the King's Bath evolves on an average 267 cubic inches of gas per minute, or 223 cubic feet in twenty-four hours. He further ascertained that this gas consists nearly entirely of nitrogen, mixed with a small amount of oxygen and carbonic acid, and that these gases were generally in the following proportion:—

Nitrogen . . = 91·9  
Oxygen . . . = 3·8  
Carbonic acid = 4·3

In many instances, however, he observed as much as 7·4 to 8·2, and even once 11·5 parts of carbonic acid.

From these observations there is no doubt that the quantity of carbonic acid dissolved in the water is very variable.

In the following Table we give the analyses of former experimenters, calculated in an imperial gallon (70,000 grs.).

	Phillips.	Scudamore*.	Walker.	Noad.
Carbonate of lime .....	7.680	5.280	10.667	
Carbonate of oxide of iron	0.274	0.200	0.243	0.521
Carbonate of soda .....	.....	.....	.....	5.760
Sulphate of lime .....	86.400	98.320	81.624	96.240
Sulphate of potassa .....	.....	.....	2.927	
Sulphate of soda .....	14.400	1.520	19.371	
Chloride of sodium .....	31.680	12.240	15.122	27.456
Chloride of magnesium ...	.....	15.360	13.339	7.142
Alumina .....	.....	.....	0.150	
Silicic acid .....	1.960	1.920	3.233	3.360
	142.394	134.840	146.676	140.479
Quantity directly observed	144.125	.....	147.622	149.72
Carbonic acid .....	11.52 cub. in.	.....	7.60 cub. in.	

Our analysis agrees, as may be seen, best with that of Walker. According to Professor Liebig's† arrangement of mineral waters, the thermal spring of Bath would belong to the saline waters containing carbonic acid.

## XI. Notices respecting New Books.

*On the Correlation of Physical Forces : being the substance of a Course of Lectures delivered in the London Institution, in the year 1843. By W. R. GROVE, Esq., M.A., F.R.S., Barrister-at-Law. Printed at the request of the Proprietors of the London Institution. London : Samuel Highley, 32 Fleet Street.*

THIS publication treats of subjects which might have been advantageously considered at much greater length ; but it must be acknowledged that in the brief space to which the author has confined the announcement of his views and speculations, he has done them no small degree of justice ; it may indeed be questioned whether the opinions broached are not of such a nature as to defy the test of experiment to realise or to refute them. This is certainly the case as far as experiment has yet been carried ; but although we discover great reason for doubting whether the difficulties which beset the subjects may ever be overcome, we discover no cause for despair, seeing that new modes of research and new instruments for carrying them out are of almost daily occurrence. As a proof of this we may cite the author's excellent invention of his well-known and justly-appreciated voltaic battery ; and his still more recent discovery, that water may be decomposed by heat so as to exhibit both its elements in the gaseous form.

Mr. Grove states that "the position which he seeks to establish in this Essay is, that the various imponderable agencies, or the affections of matter which constitute the main objects of experimental

\* Recalculated according to a more correct principle by Walker.

† *Handwörterbuch der Chemie*, Art. 'Analyse der Mineralwasser.'

physics, viz. heat, light, electricity, magnetism, chemical affinity and motion, are all correlative, or have a reciprocal dependence; that neither, taken abstractedly, can be said to be the essential or proximate cause of the others, but that either may, as a force, produce or be convertible into the other; thus heat may mediate or immediately produce electricity, electricity may produce heat; and so of the rest."

In further illustration of the author's views, we may quote what he states to be the sense that he has attached to the word correlation, which is, that "of a reciprocal production or convertibility; in other words, that any force capable of producing or being convertible into another, may, in its turn, be produced by it,—nay, more, can be itself resisted by the force it produces, in proportion to the energy of such production, as action is ever accompanied and resisted by reaction; thus, the action of an electro-magnetic machine is reacted upon by the magneto-electricity developed by its action."

In order to support his speculations by facts, the author appeals in the first place to the agency of electricity. "To commence, then, with electricity as an initiating force, we get *motion* directly produced by it in various forms; for instance in the attraction and repulsion of bodies, evidenced by mobile electrometers, such as that of Cuthbertson, where large masses are acted on; the rotation of the fly wheel, another form of electrical repulsion, and the deflection of the galvanometer needle, are also modes of palpable, visible motion. Electricity directly produces *heat*, as shown in the ignited wire, the electric spark, and the voltaic arc, in the latter the most intense heat with which we are acquainted, so intense, indeed, that it cannot be measured, every sort of matter being dissipated by it. Electricity directly produces *light* in the same phenomena. It directly produces *magnetism* in all ferruginous bodies placed at right angles to its line of direction, and, indeed, in the substances, of whatever nature, traversed by the electrical current, in a direction at right angles to that of the current; in this case giving us a new character of force, viz., a force acting, not in direct straight lines, but in a tangential or rather rectangular direction.

"Lastly, electricity directly produces *chemical affinity*, and by its agency we are enabled to obtain effects of analysis or synthesis, with which ordinary chemistry does not furnish us. Of these effects we have examples in the brilliant discoveries by Davy of the alkaline metals, and in the peculiar crystalline compounds made known by Crosse and Becquerel."

Having stated thus much respecting electricity in support of his peculiar views, Mr. Grove adduces additional confirmation of them from considering the action of light, in a passage which we shall quote at length. He observes that "light is, perhaps, that mode of force the reciprocal relations of which with the others has been the least traced out. Until the discoveries of Daguerre and Talbot, very little could be definitely predicated of the action of light in producing other modes of force; and, even, since these discoveries, it is doubted by many competent investigators, whether the phenomena



of photography are not mainly dependent upon a separate agent accompanying light, rather than upon light itself. It is, indeed, difficult not to believe that a picture, taken in the focus of the camera obscura, and which represents to the eye all the gradations of light and shade shown by the original luminous image, is not an effect of light; certain it is, however, that the different coloured rays exercise different actions upon various chemical compounds, and that the effects on many, perhaps on most of them, are not proportionate in intensity to the effects upon the visual organs; those effects, however, appear to be more of degree than of specific difference, and without pronouncing myself positively upon the question, hitherto so little examined, I think it will be safer to regard the action on photographic compounds as resulting from a function of light: so viewing it, we get light as an initiating force, capable of producing, mediately or immediately, the other modes of force. Thus, it immediately produces chemical action; and having this, we at once acquire a means of producing the others."

Mr. Grove then relates the following beautiful experiment, by which he conceives that he showed the production of all the other modes of force by light:—"A prepared Daguerreotype plate is inclosed in a box filled with water, having a glass front, with a shutter over it; between this glass and the plate, is a gridiron of silver wire; the plate is connected with one extremity of a galvanometer coil, and the gridiron of wire with one extremity of a Breguet's helix; the other extremities of the galvanometer and helix are connected by a wire, and the needles brought to zero. As soon as a beam of either daylight or the oxyhydrogen-light is, by raising the shutter, permitted to impinge upon the plate, the needles are deflected: thus light being the initiating force, we get *chemical action* on the plate, *electricity* circulating through the wires, *magnetism* in the coil, *heat* in the helix, and *motion* in the needles."

We have had some difficulty in selecting passages for quotation from this publication, on account of the profusion of interesting matter which it contains, though in so small a space; we believe, however, that the selections which we have given are such as will well and sufficiently illustrate the interesting views of their author.

## XII. *Proceedings of Learned Societies.*

### ROYAL SOCIETY.

[Continued from vol. xxx. p. 207.]

Feb. 11, "ON the Amount of the Radiation of Heat, at night, 1847. from the Earth, and from various Bodies placed on, or near the surface of the Earth." By James Glaisher, Esq. Communicated by G. B. Airy, Esq., F.R.S., Astronomer Royal, &c.

The author enters into a very detailed description of the construction of the thermometers he employed in these observations, and the precautions he took to ensure their accuracy; and gives tabular records of an extensive series of observations, amounting to a num-

ber considerably above ten thousand, with thermometers placed on nearly a hundred different substances, exposed to the open air, under different circumstances, and in various states of the sky, at the Royal Observatory at Greenwich.

Feb. 18.—“On the Diurnal Variation of the Magnetic Declination of St. Helena.” By Lieut.-Colonel Edward Sabine, R.A., For. Sec. R.S.

It has long been known that the diurnal variation of the magnetic needle is in an opposite direction in the southern, to what it is in the northern hemisphere; and it was therefore proposed as a problem by Arago, Humboldt and others, to determine whether there exists any intermediate line of stations on the earth where those diurnal variations disappear. The results recorded in the present paper are founded on observations made at St. Helena during the five consecutive years, from 1841 to 1845 inclusive; and also on similar observations made at Singapore, in the years 1841 and 1842; and show that at these stations, which are intermediate between the northern and southern magnetic hemispheres, the diurnal variations still take place; but those peculiar to each hemisphere prevail at opposite seasons of the year, apparently in accordance with the position of the sun with relation to the earth's equator.

Feb. 25.—“On certain Properties of Prime Numbers.” By the Right Hon. Sir Frederick Pollock, M.A., F.R.S., Lord Chief Baron of the Exchequer, &c.

The author of this paper, after noticing Wilson's Theorem, (published by Waring about the year 1770, without any proof), which theorem is that, if  $A$  be a prime number,  $1. 2. 3. \dots (A-1) + 1$  is divisible by  $A$ ; refers to Lagrange's and Euler's demonstrations, and mentions Gauss's extension of the theorem, to any number, not prime; provided that instead of  $1, 2, 3, \&c. (A-1)$ , those numbers only be taken which are prime to  $A$ , and  $1$  be either added or subtracted. This theorem was published by Gauss without a proof in 1801, with a rule as to the cases in which  $1$  is to be added or subtracted, the correctness of which is questioned by the author, who proceeds to propound the following theorem, which he had previously, for distinctness, divided into three.

If any number, prime or not, be taken, and the numbers prime to it, and less than one half of it be ascertained, and those be rejected whose squares  $\pm 1$  are equal to the prime number, or some multiple of it (which may be more than one), then the product of the remaining primes (if any),  $\pm 1$  shall be divisible by the prime number.

He gives as examples,  $14$ , the primes to which, and less than one half, are  $1, 3, 5$ , and  $1. 3. 5 = 15$ ; therefore  $1. 3. 5 - 1 = 14$ ; also  $15$ , the primes to which and less, are  $1, 2, 4, 7$ ; but  $4 \times 4 = 16 = 15 + 1$ ; therefore  $4$  is to be rejected, and  $1. 2. 7 + 1 = 15$ . The author adds another theorem, that if  $A$  be a prime number, all the odd numbers less than it (rejecting as before); also, all the even numbers (making the same rejection except  $A-1$ ) will, multiplied together, be equal to  $A \pm 1$ .

The author then proceeds to prove Gauss's extension of Wilson's

theorem, and to give the cases in which 1 is to be added or subtracted; and in the course of the proof, he mentions that the numbers prime to any number not only are found in pairs, one greater and one less than one-half of the number, but that they associate themselves in sets of four, with an odd pair in certain cases. Thus, the primes to 7 are 1, 2, 3, 4, 5, 6,—

$$2 \times 4 = 8 = 7 + 1.$$

Put the complementary numbers underneath crosswise, thus,—

$$\begin{array}{ccc} 2 & \times & 4 \\ & \diagdown & \diagup \\ & \times & \\ & \diagup & \diagdown \\ 3 & \times & 5 \end{array}$$

so that  $2 + 5$  and  $4 + 3$  may equal 7; and then

$$3 \times 5 = 15 = 2 \times 7 + 1$$

$$2 \times 3 = 6 = 7 - 1$$

$$4 \times 5 = 20 = 3 \times 7 - 1$$

Multiplied together one way the product exceeds 7, or a multiple of it, by 1; multiplied the other way, the product is less than 7, or some multiple of it, by 1. By assuming the prime number to be  $A$ , and the two primes to it to be  $p, q$ , and that  $p + q$  be not equal to  $A$ , but  $pq = nA \pm 1$ , it is shown that the complementary primes  $(A - q)$  and  $(A - p)$  will have a product  $= n'A \pm 1$ , and that, instead of 1, the number may be any other prime to  $A$ . Upon this foundation the author proceeds to show that Wilson's theorem, and also Gauss's, may be made much more general; that if  $A$  be a prime number, as 7, the numbers less than it may be arranged in pairs, not only with reference to 1, but to any number less than 7. Take 4 as an example:—

$$\begin{array}{ccc} 1 & \times & 3 = 7 - 4 \\ & \diagdown & \diagup \\ & \times & \\ & \diagup & \diagdown \\ 4 & \times & 6 = 4 \times 7 - 4 \\ 2 & \times & 5 = 2 \times 7 - 4 \end{array}$$

therefore  $1 \cdot 2 \cdot 3 \cdot 4 \cdot 5 \cdot 6 = 7n - 4^3$ ;

therefore  $1 \cdot 2 \cdot 3 \cdot 4 \cdot 5 \cdot 6 + 4^3 = 7n$ ; that is, is divisible by 7.

The same is then shown as to numbers not prime, provided those numbers alone are taken which are prime to it, and the number of pairs will be half the number of primes. The general theorem therefore is this:—If  $A$  be any number, prime or not, and  $m$  be the number of primes to it, which are 1,  $p, q, r$ , &c.; then  $1 \cdot p \cdot q \cdot r$ , &c.,  $\pm Z^{\frac{m}{2}}$  will be divisible by  $A$ , provided  $Z$  be prime to  $A$ , whether it be greater or less.

It follows from this that  $z^{\frac{m}{2}} \pm 1$  must be divisible by  $A$ , and therefore that  $z^m - 1$  must be divisible by  $A$ . If  $A$  be a prime number

and  $z$  a number prime to it (which every number not divisible by it is), this is Fermat's theorem, and the author has given a new proof of it. But the theorem is true though  $A$  be not a prime number, provided  $z$  be prime to  $A$  and  $m$  be the number of primes to  $A$ , and less than it; and instead of 1, any other number prime to  $A$  raised to the  $m$ th power may be substituted: and  $z^m - y^m$  will be divisible by  $A$ , provided  $z$  and  $y$  be primes to  $A$ , and  $m$  be the number of primes to  $A$  and less than it.

The author has therefore in this paper offered a proof of Gauss's theorem, and proved that it applies in certain cases to one half of the primes, and in all cases, with certain modifications, has shown that a similar property belongs to the product of the odd numbers, and also of the even numbers which precede any prime number; and lastly, has shown the intimate connexion between Wilson's theorem and Fermat's, and shown that each is but a part of a much more general proposition, which, he observes, may itself turn out to be part only of a still more universal one.

In a postscript, the author has shown that the well-known law of reciprocity of prime numbers is an immediate corollary from his theorem; and that it may be extended thus: if  $A$  and  $B$  be any two numbers (not prime numbers but) prime to each other, and the primes to  $A$ , and less than it, are  $(m)$  in number, and the similar primes to  $B$  are  $(n)$ , then  $(A^n - 1)$  is divisible by  $B$ , and  $(B^m - 1)$  is divisible by  $A$ .

"On the reabsorption of the Mixed Gases in a Voltameter." By Professor M. H. Jacobi, in a letter to Michael Faraday, Esq., F.R.S. Communicated by Dr. Faraday.

The author found that if the mixed gases developed from the decomposition of water by a voltaic current, be allowed to remain in the voltameter in which they were collected, in contact with the fluid which produced them, they by degrees diminish in volume, and ultimately disappear by being absorbed by the fluid. He has not yet fully determined the precise conditions on which this phenomenon depends; but he is inclined to think that it is owing to a portion of the mixed gases, diffused throughout the whole liquid, coming into contact with the platinum plates, and being recombined on the surface of those plates; and this process being renewed with every fresh portion of the gases which takes the place of the former, the whole of the gases are thus reconverted into water.

March 4.—"Researches into the effects of certain Physical and Chemical Agents on the Nervous System." By Marshall Hall, M.D., F.R.S., &c.

The professed object of the author, in the present paper, is "to detail the results of an investigation of the phenomena and the laws of production and action of certain secondary or induced conditions of the nervous system, which are effected by a voltaic, and probably by any other electric current, but persistent after the influence of that current is withdrawn." This condition he designates by the new term *electrogenic*, as describing at once the origin and the independence of that condition. On the present occasion he conflates



himself to the subject of the electrogenic condition of the muscular nerves, postponing to future inquiries that of the incident nerves and of the spinal marrow; and also the modes of action of other physical and chemical agents, such as mechanical injury, heat and cold, strychnine, and the hydrocyanic acid.

The bones and muscles of the brachial lumbar and pelvic regions of a frog, being isolated from all the other parts of the body, excepting only by means of their respective brachial and lumbar nerves, which were perfectly denuded on all sides, and raised from the glass on which the limbs were laid, a voltaic current from a pair of the "couronne de tasses" was passed downwards through the nerves, in a direction from their origin in the spinal marrow towards their terminations in the muscles. Energetic muscular movements were at first excited; and the current was thus continued during the space of five, ten, or fifteen minutes, and at the end of this period was withdrawn. No sooner was the current discontinued than the muscles were affected with spasmodic contractions, and with a tetanoid rigidity, constituting the secondary, or what the author denominates the *electrogenic condition*; an effect, which as instantly subsides on the restoration of the voltaic current.

The author proceeds to state the precautions which must be taken to ensure the success of experiments on this subject; and traces the effects of desiccation of the nerves from spontaneous evaporation, and of the application of external moisture, on the phenomena; and also the modifications introduced by varying the extent of voltaic contact. Various experiments are then described, which the author instituted with a view to ascertain the nature of the electrogenic condition of the nerves, and the circumstances under which it is induced; and he is led to the conclusion that the phenomena involve some voltaic principle which has not hitherto been fully investigated.

March 11.—"On the cause of the discrepancies observed by Mr. Baily with the Cavendish Apparatus for determining the Mean Density of the Earth." By George Whitehurst Hearn, Esq., of the Royal Military College, Sandhurst. Communicated by Sir John F. W. Herschel, Bart., F.R.S.

After taking a summary review of the methods employed by Mr. Baily for determining, on the plan devised by Mr. Cavendish, the mean density of the earth, and of the anomalies, hitherto unaccounted for, which had introduced perplexity in the results obtained, the author, suspecting that these anomalies had their source in the variable magnetic states of the masses which were the subject of experiment, traces the effects which such an influence might be supposed to have on those results. He finds that, the attraction arising from gravitation between a mass and one of the balls being exceedingly minute, an almost inconceivably feeble magnetic state may be the cause of great perturbations. He then proceeds to investigate the subject by the application of mathematical analysis; from which he is led to the conclusion that the masses and balls do actually exert on one another influences which are independent of the action of gravitation. He finds that such influences are of a

very fluctuating nature; the action arising from them being either positive or negative, and its sign also changing in each revolution as the masses are turned round a vertical axis; and he observes that such action may either fall short of that arising from gravitation or exceed it many times. Such disturbing force he conceives can be no other than a magnetic influence; not however one of the ordinary kind, but that which Faraday has recently discovered as affecting all diamagnetic bodies.

The author concludes by proposing methods by which the inquiry should in future be conducted, so as to obviate or eliminate this source of error. Such an inquiry, he remarks, would, by exhibiting the magnetic and diamagnetic powers under new aspects, lead, in all probability, to important consequences.

March 18.—“Researches to determine the Number of Species and the Mode of Development of the British Triton.” By J. Higginbottom, Esq., F.R.C.S. Communicated by Thomas Bell, Esq., F.R.S.

The observations of the author, of which he gives a detailed account in the present memoir, have led him to the following conclusions:—

Two species only of the genus Triton are met with in England; namely, the *Triton verrucosus* and the *Lisso-triton punctatus*. It is three years before the animal is capable of propagating its species, and four years before it attains its full growth. In its tadpole state, it remains in the water till its legs acquire sufficient strength to qualify it for progressive motion on land. While a land animal, it is in an active state during the summer, and passes the winter in a state of hybernation; but does not then, as has been erroneously supposed, remain at the bottom of pools. Very dry, or very wet situations are incompatible with the preservation of life during the period of hybernation. At the expiration of the third year, the triton revisits the water, in the spring season, for the purposes of reproduction, and again leaves it at the commencement of autumn. Impregnation is accomplished through the medium of water, and not by actual contact. The growth and development of the triton are materially influenced by temperature, and but little by the action of light. The triton possesses the power of reproducing its lost limbs, provided the temperature be within the limits of  $58^{\circ}$  and  $75^{\circ}$  Fahrenheit; but at lower temperatures, and during the winter, it has no such power.

April 15.—“On the Proper Motion of the Solar System.” By Thomas Galloway, Esq., A.M., F.R.S.

The object of this paper is to communicate the results of a calculation for determining the direction of the proper motion of the solar system from the apparent proper motions of stars in the southern hemisphere, deduced mostly from a comparison of the observations made by Lacaille at the Cape, about the middle of the last century, with the recent observations of Mr. Johnson and the late Professor Henderson at St. Helena and the Cape respectively. After adverting to the papers of Sir William Herschel in the Philo-

sophical Transactions for 1783 and 1806, and some other investigations of the same subject, the author remarks that up to a recent period astronomers seem generally to have entertained the opinion that our knowledge of the proper motions of the stars is not sufficiently advanced to enable us to pronounce positively either on the fact or the direction of the motion of our own system. This opinion was grounded on the discrepancies which present themselves when it is attempted to explain the observed displacements of individual stars by referring them to the motion of the sun in an opposite direction; it being always found that whatever direction is assigned to the sun's motion, there are many stars whose proper motions cannot thereby be accounted for. But if the sun be in motion it is very improbable that any star is absolutely at rest; hence the proper motions deduced from a comparison of catalogues must be regarded as the effect partly of the true proper motions of the stars, and partly of the apparent systematic or parallaxic motion caused by the displacement of the point of view; and as we have no reason for supposing the true proper motion of a star to be more probable in one direction than in another, it may be expected, *a priori*, that the observed directions will form angles of all different values with the direction of the sun's motion, or any other fixed line. The observed discrepancies are therefore not incompatible with a general drifting of the stars towards a particular region of the heavens; but in order to deduce the direction of the systematic motion, it becomes necessary to take account of a very considerable number of proper motions, and to represent them by equations, involving the unknown quantities required for determining the direction of the sun's motion, and to solve the equations so as to obtain the most probable values of those quantities. The first person who investigated the subject under this point of view was Professor Argelander of Bonn, in a paper published in the Petersburg Memoirs for 1837. From the proper motions of 390 stars deduced from a comparison of Bessel's catalogue of Bradley's observations with his own catalogue of stars observed at Abo, Argelander found the direction of the sun's motion, for 1792.5, to be towards the point of the sphere whose right ascension is  $259^{\circ} 47' 6''$  and declination  $+32^{\circ} 29' 5''$ . Lundahl, subsequently, from a comparison of the places of 147 stars in the catalogues of Bessel and Pond, and not included among those considered by Argelander, found the co-ordinates of the point to be  $R=252^{\circ} 24' 4''$ , Dec.  $+14^{\circ} 26' 1''$ ; and Otto Struve, still more recently, from the comparison of about 400 of Bradley's stars with the positions determined at the Dorpat Observatory, obtained the result  $R=261^{\circ} 23' 1''$ , Dec.  $+57^{\circ} 35' 7''$ . The mean of those results taken with respect to their probable errors, was found by O. Struve to be  $R=259^{\circ} 9' 4''$ , Dec.  $+34^{\circ} 36' 5''$ .

All the stars included in the calculations of Argelander, Lundahl, and O. Struve being situated to the north of the tropic of Capricorn, it appeared to be a point of some interest to determine whether the southern stars agree with the northern in their indication of the direction of the solar motion, or afford any confirmation of



the hypothesis of the sun's translation. Unfortunately, we have no observations made in the southern hemisphere in the last century equal in precision to those of Bradley, but the catalogue given by Lacaille in his '*Astronomiæ Fundamenta*,' furnishes a means of comparison of considerable value in reference to the present inquiry. In Mr. Johnson's '*Catalogue of 606 Stars in the Southern Hemisphere*' (London, 1835), there are sixty-one which, on comparing their places in 1830 with those of Lacaille reduced to the same epoch, appear to have shifted their positions not less than  $8''$  in space in the interval of eighty years between the epochs of the catalogues, or to have an annual proper motion of not less than one-tenth of a second in space. Prof. Henderson's catalogue (Mem. R. Astron. Society, vols. x. and xv.) furnishes thirty-six stars, which, on a like comparison, appear to have an annual proper motion exceeding the same limit. Of these, however, thirty-two are contained in Mr. Johnson's catalogue, but Henderson gives the proper motions of sixteen other stars (in the southern hemisphere), from the comparison of his own places with those of Bradley. On the whole, therefore, the two catalogues furnish eighty-one different stars whose proper motions are given both in right ascension and declination. The method of investigation is the same as that of Argelander. From the differences of  $R$  and Dec. given by comparison of the catalogues, the direction of the *apparent* motion of each star is computed. It is then assumed that the sun is moving towards a point whose right ascension  $A=259^{\circ} 46'.2$  and declination  $D=+32^{\circ} 29'.6$ ; and the direction in which each star would appear to move, if it were itself at rest, is computed on this hypothesis. The difference of these two directions is treated as an error of observation, and its numerical value substituted for the differential of the angle which determines the direction of the parallactic motion; this differential being expressed by a formula containing the differentials of  $A$  and  $D$  multiplied by known coefficients. An equation is thus obtained of the form

$$0=adA+bdD+n,$$

in which  $a$ ,  $b$ , and  $n$  are known quantities. Each star furnishes a similar equation; and the equations, being first multiplied respectively by the sine of the star's distance from the point assumed as the apex of the sun's motion, in order to give them all the same weight, are solved by the method of least squares, and the resulting values of  $dA$  and  $dD$  applied as corrections to the assumed values of  $A$  and  $D$ . The results are as follows:—the whole of the eighty-one equations give (for 1790) as co-ordinates of the point towards which the sun's motion is directed,

$$R=263^{\circ} 38'.0 \pm 5^{\circ} 14'.5; \text{ Dec.} = +37^{\circ} 15'.0 \pm 6^{\circ} 17'.6.$$

But two of the stars compared with Lacaille move in a direction so nearly opposite to that of their motion on the assumed hypothesis, that (in one case especially) a slight error of observation would change the sign of  $n$  in the equations of condition. It therefore appears necessary to reject those two stars; and a further reason



for rejecting them is, that they are both situated within  $8^\circ$  of the pole, in which position Lacaille's determination of the right ascension is probably not to be depended upon. Setting aside, therefore, the two stars in question, the remaining seventy-nine equations give

$$R = 256^\circ 51' \cdot 5 \pm 4^\circ 45' \cdot 1; \text{ Dec.} = +34^\circ 14' \cdot 3 \pm 5^\circ 36' \cdot 2.$$

The author further observes, that one of the stars compared with Bradley's catalogue is also remarkable as appearing to move in a direction nearly opposite to the mean direction of the whole, and that if this star be rejected also on account of the great probability there is that the parallactic motion is in this case concealed by the larger proper motion of the star itself in an opposite direction, the co-ordinates of the solar apex become

$$R = 259^\circ 47' \cdot 4 \pm 4^\circ 31' \cdot 9; \text{ Dec.} = +34^\circ 19' \cdot 5 \pm 5^\circ 17' \cdot 7,$$

a result differing less than a degree either in right ascension or declination from the mean, as above stated, of the three previous determinations.

### XIII. *Intelligence and Miscellaneous Articles.*

#### ACTION OF CHLORINE ON ALCOHOL.—FORMATION OF ACETAL.

**M.** STAS states that he has observed that the causes which give rise to acetal are not always oxidating causes. When chlorine is made to act upon alcohol, acetal is the principal product, as long as it does not act by substitution, and it is at once a dehydrogenating and an oxidizing body. This discovery, the author is of opinion, throws great light on the hitherto obscure action of chlorine upon alcohol.

In order to obtain acetal by the action of chlorine upon alcohol, it is sufficient to pass a current of chlorine into alcohol of 80 per cent., cooled to  $50^\circ$  or  $60^\circ$  F. The action is to be discontinued when chlorinated bodies commence formation by substitution: this is readily ascertained, for the alcohol then becomes turbid on the addition of water; the liquid, which has become very acid, is to be distilled, and one-fourth of the quantity is to be preserved. This is to be neutralized by means of chalk, and by a fresh distillation one-fourth of the product is again to be obtained; in this fused chloride of calcium is to be dissolved, which immediately separates a large quantity of a very volatile fluid, containing, like common rough acetal, aldehyd, acetic æther and alcohol; by the addition of more chloride of calcium, the utmost quantity of alcohol and acetic æther are separated; the purification of the acetal is to be completed.

The analysis of the acetal thus obtained was similar to that procured in the usual way; and thus the chlorine acts, as already stated, both as a dehydrogenating and oxidizing body:  $C^{12}H^{18}O^6 + 2Ch - 2HO = C^{12}H^{14}O^4 + 2CH + 2HO$ .—*Ann. de Chim. et de Phys.*, Feb. 1847.

**BISILICATE OF IRON OR FERRUGINOUS PYROXENE.**

This new mineral is described in a memoir presented by M. Dufrénoy to the Academy in the name of M. Gruner, mining engineer, and Professor in the School of Mines at St. Etienne. It corresponds in composition to a pyroxene with a base of iron.

M. Gruner states that this mineral resembles certain varieties of asbestos, or more nearly fibrous amphibole. Its specific gravity is 3·713, which exceeds that of the densest epidotes, amphiboles or pyroxenes. By analysis M. Gruner obtained—

Silica.....	43·9
Protoxide of iron .....	52·2
Lime .....	·5
Magnesia .....	1·1
Alumina.....	1·9
	<hr/> 99·6

Admitting that the greater portion of the foreign bases is derived from a small quantity of the gangue, it will be seen that this mineral is bisilicate of iron, or ferruginous pyroxene with one base only.—*Comptes Rendus*, Mai 5, 1847.

**CHLOROSULPHURET OF SILICIUM.**

M. Isidore Pierre states that when hydrosulphuric acid and chloride of silicium in vapour are passed through a porcelain tube heated to redness, they react upon each other: much hydrochloric acid is produced, which is disengaged with excess of hydrosulphuric acid gas and a little chloride of silicium, which escapes the reaction.

If the products of this reaction be passed into a U-shaped tube immersed in cold water, a fuming liquor condenses, which has a sharp foetid odour, resembling that of hydrosulphuric acid and chloride of sulphur. The liquor thus obtained was slightly opake by sulphur suspended in it: this was deposited by being left forty-eight hours in a well-stoppered bottle. There were also deposited on the sides of the bottle, clear lemon-yellow crystals, which were sulphur in the form of oblique rhombic prisms, without any modification.

The condensed liquor has consequently the power of dissolving sulphur, and of depositing it in crystals belonging to the same system as those which are obtained in the dry way. The smallness of these crystals prevented the author from determining their angles; but he reckons upon being able soon to do so. No sensible traces of sulphuret of silicium were found in the minute deposit produced in the porcelain tube.

The liquid condensed in this operation was distilled in an oil-bath from a retort furnished with a thermometer: the more volatile portions, which usually distil from 140° to 176° F., were rejected. They consist principally of chloride of silicium mixed with a small quantity of chlorosulphuret. Afterwards there is obtained a limpid colourless liquid which fumes in the air, and has an odour resembling that of chloride of silicium and hydrosulphuric acid.

Its specific gravity at 60° F. is about 1·45; that is, a little less than that of chloride of silicium. When it is thrown into water, it occasions an abundant disengagement of sulphuretted hydrogen gas and a slight deposit of sulphur. It boils at above 212° F.; but the small quantity obtained did not allow of ascertaining its exact boiling-point.

By analysis, it yielded such proportions of its constituents as to indicate for its formula  $\text{Cl}^2 \text{S Si}$ , which would give—

Chlorine .....	65·47
Sulphur.....	14·83
Silicium.....	19·70
	<hr/> 100·00

M. Pierre proposes the name of *chlorosulphuret of silicium* for this compound.—*Ibid*, Mai 5, 1847.

#### METEOROLOGICAL OBSERVATIONS FOR MAY 1847.

*Chiswick*.—May 1. Very fine. 2. Cloudy. 3. Rain. 4. Cloudy. 5. Cloudy and fine. 6. Slight fog: fine. 7. Overcast: showery. 8. Rain. 9. Fine: cloudy: densely overcast: rain. 10. Very fine: slight showers. 11. Cloudy. 12. Very fine. 13. Cloudy and fine: showers. 14. Showery. 15. Fine: rain at night. 16. Rain: cloudy: rain at night. 17. Cloudy. 18. Fine: rain. 19, 20. Cloudy and fine. 21, 22. Very fine. 23. Very hot and sultry. 24. Cloudy and fine. 25—27. Very fine. 28. Slight haze: sultry. 29. Cloudy: thunder and heavy rain. 30. Clear and fine. 31. Cloudless: exceedingly fine.

Mean temperature of the month ..... 56°·83

Mean temperature of May 1846 ..... 56·16

Mean temperature of May for the last twenty years ... 55·01

Average amount of rain in May ..... 1·84 inch.

*Boston*.—May 1. Fine. 2. Cloudy: rain early A.M.: rain P.M. 3. Cloudy: rain A.M. and P.M. 4. Cloudy. 5. Fine: rain P.M. 6. Cloudy. 7. Fine: rain P.M. 8. Cloudy: rain P.M. 9. Cloudy. 10. Cloudy: rain early A.M. 11. Rain. 12. Fine: rain, with thunder P.M. 13. Fine: rain P.M. 14, 15. Fine: rain early A.M. 16. Rain: rain, with thunder P.M. 17. Cloudy. 18. Cloudy: rain P.M. 19, 20. Cloudy. 21—24. Fine. 25. Windy. 26, 27. Fine. 28. Fine: 1 o'clock P.M. thermometer 82°. 29. Rain: 4 o'clock A.M. thunder, hail and rain: rain all night. 30. Fine: rain early A.M. 31. Fine.

*Sandwick Manse, Orkney*.—May 1. Bright: clear. 2. Bright: drops. 3. Bright: clear. 4. Bright: damp. 5. Fine. 6, 7. Cloudy: damp. 8, 9. Drizzle: fog. 10. Clear: fine. 11. Cloudy: rain. 12. Rain: cloudy. 13. Cloudy. 14. Rain: fog. 15. Damp: rain: fog. 16. Bright: cloudy. 17, 18. Cloudy: clear. 19. Showers: drizzle. 20. Fog: cloudy. 21. Bright: rain. 22. Showers. 23. Clear. 24. Fine. 25. Bright: cloudy. 26. Bright: showers. 27. Fine: clear. 28. Fine: cloudy: fine. 29. Rain: thunder: cloudy: fine. 30. Clear: fine. 31. Cloudy: fine.

*Applegarth Manse, Dumfries-shire*.—May 1. Fine summer day. 2. Mild: showers. 3. Cloudy: keen. 4. Spring, but keen. 5. Cold: wet P.M. 6. Growing: wet P.M. 7. Dull: showers. 8. Dull: wet P.M. 9. Mild: dull: wet P.M. 10. Fine growing day. 11—14. Dull: showers. 15. Fine summer day. 16. Stormy: wet all day. 17. Wet and cold. 18. Wet and stormy. 19. Dull: wet. 20. Sunshine: fine. 21. Dry: cloudy. 22. Cloudy: showers. 23. Warm: thunder: rain. 24. Fine: clear: wet P.M. 25. High wind: clear. 26. Fine: clear: light: cloudy. 27. Fine: clear: thunder. 28. Fine: wet P.M. 29. Fine: heavy rain P.M. 30. Fine: warm. 31. Remarkably fine.

Mean temperature of the month ..... 51°·1

Mean temperature of May 1846 ..... 52·6

Mean temperature of May for twenty-five years ..... 51·1

Mean rain in May for twenty years ..... 1·69 inch.

*Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at Chiswick, near London; by Mr. Veall, at Boston; by the Rev. W. Dunbar, at Applegarth Manse, Dumfries-shire; and by the Rev. C. Clouston, at Sandwick Manse, Orkney.*

Days of Month.	Barometer.						Thermometer.				Wind.				Rain.			
	Chiswick.		Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Dumfries-shire.		Orkney, Sandwick.		Boston.		Dumfries-shire.		Orkney, Sandwick.	
	Max.	Min.	9 a.m.	9 p.m.	94 a.m.	94 p.m.	Max.	Min.	84 a.m.	Max.	Min.	84 a.m.	Max.	Min.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.
1847. May.																		
1.	29.844	29.669	29.40	29.60	29.57	29.75	58	44	49	53	39	47	41	sw.	w.	w.	calm	.03
2.	29.743	29.577	29.22	29.55	29.58	29.86	59	30	49	49	40	46½	44½	s.	s.	sw.	nne.	.03
3.	29.828	29.788	29.37	29.73	29.71	29.99	53	40	46	52	39	41½	44½	nw.	w.	w.	ne.	.15
4.	29.868	29.858	29.52	29.75	29.80	29.96	58	36	50	53	41	46	45	e.	e.	w.	se.	.02
5.	29.759	29.721	29.40	29.77	29.79	30.00	60	31	51	55	41	51	45	se.	ne.	e.	e.	.....
6.	29.744	29.710	29.32	29.63	29.55	29.86	62	40	51	54	41	49	43	s.	se.	e.	ese.	.16
7.	29.683	29.569	29.30	29.57	29.57	29.80	65	50	59.5	60	42	48	44	se.	s.	ese.	ese.	.14
8.	29.611	29.334	29.03	29.48	29.35	29.72	60	48	53	53½	45	47	47	s.	e.	e—s.	e.	.15
9.	29.830	29.757	29.26	29.38	29.60	29.53	65	44	55	53½	46	47	46	sw.	sw.	w.	se.	.04
10.	29.813	29.726	29.39	29.73	29.70	29.80	72	54	58	65	47	55	47	sw.	calm	sse.	se.	.04
11.	29.681	29.591	29.20	29.60	29.55	29.78	65	40	50	61	46	52	48	sw.	ene.	se.	e.	.32
12.	29.759	29.714	29.25	29.53	29.50	29.66	65	45	60	62	42	50	46½	sw.	se.	se.	e.	.01
13.	29.834	29.821	29.35	29.57	29.57	29.65	68	41	58	63	49	47½	43½	sw.	s.	s.	e.	.02
14.	29.892	29.845	29.37	29.58	29.60	29.66	65	46	59	59	50	47	46½	sw.	sw.	s.	e.	.08
15.	30.123	29.869	29.43	29.70	29.73	29.73	67	51	59	60	49	45	43	sw.	sw.	sw.	e.	.19
16.	29.731	29.581	29.20	29.50	29.47	29.91	68	49	55	50	48	46	41½	sw.	se.	ne.	e.	.09
17.	30.001	29.871	29.32	29.57	29.83	29.84	69	45	59.5	50	41	45½	39½	sw.	ws.	ene.	ne.	.62
18.	29.989	29.785	29.54	29.88	29.72	30.06	70	45	53	50	41	47½	42	s.	ene.	e.	se.	.02
19.	29.902	29.827	29.37	29.58	29.59	29.67	66	49	61	50	46	44	45	sw.	sw.	ssw.	ese.	.....
20.	30.048	29.841	29.30	29.55	29.80	29.66	67	42	60	54	49	49	47	sw.	w.	w.	ws.	.....
21.	30.134	30.114	29.57	29.88	29.90	29.74	69	45	60	63	46	50	48	w.	w.	ws.	sw.	.....
22.	30.048	30.004	29.50	29.75	29.82	29.51	77	50	63	61	48	47½	46	sw.	w.	sw.	w.	.....
23.	30.070	29.780	29.50	29.80	29.68	29.84	89	59	65.5	65	40	49	47	sw.	sw.	se.	se.	.29
24.	29.885	29.861	29.36	29.68	29.48	29.50	71	44	63	60	51	57	51	sw.	w.	w.	se.	.04
25.	30.250	30.112	29.50	29.70	29.93	29.46	69	35	60	...	46	51	52	w.	w.	w.	s.	.....
26.	30.242	30.103	29.19	29.93	29.98	29.81	75	36	61	...	50	52	51½	sw.	sw.	sw.	s.	.....
27.	30.084	29.968	29.62	30.00	29.90	30.05	84	50	66	69	46	55	50	e.	ssw.	w.	ese.	.....
28.	29.932	29.860	29.46	29.83	29.83	29.92	91	59	75	72½	...	55	53	s.	se.	w.	s.	.56
29.	30.157	29.746	29.34	29.66	29.84	29.82	77	45	68	...	...	52	50	s.	w.	w.	w.	.65
30.	30.410	30.351	29.81	30.07	30.30	30.09	77	44	64	...	...	55	55	sw.	calm	sw.	se.	.56
31.	30.471	30.439	29.95	30.39	30.41	30.38	80	46	68.5	...	...	63	55	ne.	calm	sw.	calm	.....
Mean.	29.946	29.832	29.39	29.707	29.730	29.803	69.06	44.61	58.3	57.5	44.8	49.61	46.70	1.59	5.41	2.50	2.28	.....



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[THIRD SERIES.]

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AUGUST 1847.

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XIV. *On a new Voltaic Battery, cheap in its construction and use, and more powerful than any Battery yet made; and on a cheap substitute for the nitric acid of Grove's Platina Battery. By the Rev. N. J. CALLAN, Professor of Natural Philosophy in the Royal College, Maynooth\*.*

SOME time ago, whilst I was reflecting on the principle of action of Grove's and Bunsen's batteries, it occurred to me that lead might be substituted for the platina of the former and the carbon of the latter. I put into the porous cell of a Grove's battery a piece of lead about  $\frac{1}{16}$ th of an inch thick, two inches broad and six inches long. I found that the voltaic current produced by the lead excited by a mixture of concentrated nitric and sulphuric acid was very powerful. I afterwards compared the power of this leaden battery with that of a platina one of the same size, by sending through the helix of a galvanometer, at the same time, but in opposite directions, the currents produced by the two batteries. Both batteries were charged with the same acids: the lead and platina were excited by concentrated nitric and sulphuric acid, and the zinc by dilute sulphuric acid. The current from the platina battery destroyed the deflection produced by the leaden one, and caused an opposite deflection, which indicated that the former current was about twice as strong as the latter. The two batteries were left working for about three hours and a half. At the end of that time the current from the lead was about twice and a half as powerful as the current from the platina. The quantity of lead dissolved during these three hours and a half was very small.

It struck me that by diminishing the action of the acids on the lead, I might increase the power of the battery. I therefore covered a leaden plate with gold leaf, and coated another

\* Communicated by the Author.

of the same size with chloride of gold, in the same way in which sheet silver is platinized for Smee's battery. These plates and a platina one of the same size were put successively into the porous cell of a Grove's battery, and the voltaic current sent through the helix of our large electro-magnet, in which the iron bar is about thirteen feet long and two and a half inches thick; the copper wire is about 500 feet long and one-sixth of an inch diameter. The magnetic power given to the electro-magnet by the leaden plate coated with chloride of gold, appeared to be equal to that which was produced by the platina plate. The magnetic effect of the current from the leaden plate covered with gold leaf was not so great. A coating of chloride of platina was afterwards found to answer as well as one of chloride of gold.

Some days after a leaden and platina battery of the same size were left working for four hours and a half. At the end of that time the lead plate acted fully as well as the platina. When the nitric acid was so much exhausted that the lead was barely capable of magnetizing the large electro-magnet so as to make it sustain a certain weight, the leaden plate was taken out of the porous cell, and a platina plate of the same size put in its stead. The platina plate was not able to make the electro-magnet sustain the weight which the lead had caused it to sustain.

The magnetizing power of the platinized or gilded lead and platina batteries was compared several times in working an electro-magnetic machine. On these occasions the power of the leaden battery was evidently superior to that of the platina one. Sometimes the platina plate was taken out of the porous cell, and a platinized or gilded lead plate of the same size put in its place: the velocity of the machine was instantly and considerably increased. The same effect was produced when the platina plate was taken out of the cell and a platinized platina one put in its stead. Hence it appears that a leaden plate coated with chloride of platina or gold, or a platinized platina plate, produces a more powerful voltaic current than a platina plate does. On the 24th of last May, a small platinized lead battery and a Grove's battery of the same size, were exhibited before the Royal Irish Academy. The power of the former was obviously superior to that of the latter. By using double leads and single zincs instead of double zincs and single leads, the power of the battery appears to be increased. When the lead plates have been used for a long time, they require to be newly gilded or platinized. After being used they should be rinsed in water, and dipped into a weak solution of chloride of gold or platina.

Seeing that the concentrated acids, by dissolving the lead, removed the gold or platina powder, and that the nitric acid was very expensive, I endeavoured to find in its stead a cheap substitute which would not act on the lead. The first that occurred to me was common nitre. I dissolved about the eighth of an ounce of it in sulphuric acid, which I diluted with nearly an equal bulk of water. I poured the mixture into the porous cell of a Grove's battery, and put into it a platinized leaden plate. I then sent the voltaic current through the helix of our large electro-magnet: the magnetic power given to the magnet appeared to be greater than that which was given to it by a Grove's battery of the same size, in which the platina was excited by concentrated nitric and sulphuric acid. I afterwards compared the heating power of the two batteries, and found the power of the platinized lead battery to be evidently superior to that of the other. I charged a platinized leaden battery with a mixture consisting of about five parts of sulphuric acid, five of solution of nitre, and one of nitric acid, and a Grove's battery with equal parts of nitric and sulphuric acid. The former fused a piece of steel wire which the latter only raised to a white heat. When a platina plate is excited by a mixture of sulphuric acid and a solution of nitre, the voltaic current appears to be as powerful as that which is produced by the plate when excited by concentrated nitric and sulphuric acid. The cost of the nitre necessary for charging a battery is about the twentieth part of that of the nitric acid. The power of the former declines sooner than that of the latter: but from the results of several experiments, I have come to the conclusion that the expense of doing a given amount of work by a platina battery excited by concentrated nitric and sulphuric acid, would be three or four times as great as if the work were done by a platinized lead battery excited by a mixture of sulphuric acid and a solution of saltpetre. I have tried nitrate of soda, or cubic nitre, and nitrate of ammonia, as substitutes for nitric acid; but although they give great power, they do not answer as well as the common nitre. A solution of common nitre and cubic nitre along with sulphuric acid, forms a mixture scarcely inferior to the solution of common nitre and sulphuric acid. The most powerful mixture for the platina or platinized lead battery consists of about four parts of sulphuric acid, two of nitric acid, and two of a saturated solution of nitre. When no nitric acid is used, at least one half of the mixture should consist of sulphuric acid, and the remainder of nitre and water: the solution need not be saturated with nitre. Four parts of sulphuric acid, two of a solution of chromate of potash, and two

of the solution of nitre, make a most powerful exciting mixture for platina, but give comparatively little power to platinized lead. I endeavoured to find among the sulphates a substitute for sulphuric acid, but did not succeed. The voltaic current from a platinized lead battery, excited by two parts of sulphuric acid, three of sulphate of soda, and three of nitrate of potash, is very powerful, but considerably inferior to that which is produced by the concentrated acids.

On finding that platinized or gilded lead and platinized platina were superior to platina, I saw that the cause of the superiority was that, in the platinized or gilded lead, and in the platinized platina batteries, the acting metals were, not lead or platina and zinc, but gold or platina powder, and zinc; and that the gold or platina powder was more negative compared with zinc than platina is. Hence I inferred, first, that a leaden plate coated with any of those substances which are more negative and cheaper than platina or gold, would act as powerfully as platinized or gilded lead; and secondly, that any other metal to which the platina or gold powder would adhere might answer as well as lead. I therefore coated, by the galvanic process, leaden plates with antimony, arsenic, chromium, molybdenum and borax. The plates coated with arsenic and molybdenum were much inferior to platina: those that were coated with antimony and borax appeared fully equal to platinized lead, but they soon lost their power. The first plate which I chromed acted as well, and retained its power nearly as long as platinized or gilded lead. I afterwards coated a great number of plates with chromium; but all of them were far inferior to the first. The power of a leaden plate is greatly increased by being coated with mercury, or even with clay boiled in aqua regia, or with any other substance which I tried; but I have not found any substance to answer as well as the chloride of gold or platina.

I have compared with platinized lead, the other cheap metals coated with gold or platina, or chromium; and with the exception of cast iron, they were all inferior to it. Platinized or chromed cast iron answers as well as platinized lead; and without being chromed or platinized, cast iron appears to act as powerfully as platina. The power of a cast iron battery in magnetizing our large electro-magnet, and in driving an electro-magnetic machine, was compared with that of a Grove's battery of the same size. In the two batteries the exciting mixture was the same. The power of the former appeared to be fully equal to that of the latter.

From the results of several experiments which I have made on the relative power of platinized silver and platinized lead,



I feel confident that the latter may, without any diminution of power, be substituted for the former in Smee's battery. Cast iron does not take the coating of platina powder (at least until the hard surface is worn away) so well as lead or silver, and on that account it does not act as powerfully as either. But I have found zinc and cast iron excited by dilute sulphuric acid as constant in their action as zinc and platinized lead. A platinized lead, or cast iron plate six inches square, may be had for the twelfth part of the cost of a platinized sheet of silver of the same size.

From the experiments which have been described, I infer, first, that a battery superior in power to Professor Grove's nitric acid battery may be made by substituting platinized platina or lead for platina, and nitrosulphuric acid and nitrate of potash for nitric and sulphuric acid; and secondly, that a battery equal in power to the nitric acid battery may be constructed by the substitution of cast iron for platina.

The advantage of what I may call the nitre platina battery over the nitric acid one is, that the expense of working the former is, as has been already stated, considerably less than that of working the latter.

The advantage of the cast iron or platinized leaden batteries over Professor Grove's is, that they are far less expensive in their construction. A plate of cast iron or platinized lead may be had for a shilling, whilst a platina plate of the same size will cost nearly three pounds. Besides, a cast iron or platinized lead battery may be worked by a mixture of nitre and sulphuric acid for one hour for about the tenth part of the expense of working a Grove's battery for the same time.

The cheapness of cast iron and platinized lead will enable every one to procure a powerful voltaic battery. A platinized lead battery is about fifteen times as powerful as a common Wollaston battery of the same size. A cast iron battery is a little less powerful than the platinized lead one; but I prefer the former, because the cast iron does not require to be chromed or platinized. I am now preparing two large cast iron batteries for the College: one will contain about thirty-three square feet of zinc and sixty-six of cast iron, the other will contain eighty square feet of zinc and a hundred and sixty of cast iron. These batteries will be more powerful than any battery ever constructed. The expense will be very moderate; for the zinc plates and Wedgwood troughs of our former batteries will answer for the new ones.

Maynooth College, July 3, 1847.

XV. *On the Perturbations of Planets moving in Eccentric and Inclined Orbits.* By Sir J. LUBBOCK, Bart., F.R.S.

[Continued from page 6.]

**I**N the last Number of the Philosophical Magazine I described tables by which the development of the disturbing function  $R$  is greatly facilitated. I shall now describe other tables which have been calculated for me by Mr. Farley, and which also facilitate the numerical solution. The advantages which the employment of tables presents wherever they can be applied are well known. Not only the march of the figures affords security against error, but the computer acquires facility in such calculations systematically undertaken, while the operations are more easy than they would be if the quantities required were not connected by a common origin, or so troublesome as they would be if undertaken by different individuals, or by the same individual at different times. The use of tables is out of the question in a literal or algebraic development; but, on the contrary, it is an important property of the numerical development that it can thus be materially facilitated.

All developments whatever may be resolved into three classes, which I call *literal*, *quasi-literal*, and *arithmetic*. *Literal* or *algebraical* are those which result when the numerical values of the constants are inserted last, and after the development is complete. *Quasi-literal* are those which result when either a part only of the constants are expressed by means of general symbols, or when the development is made up of several distinct processes, and when the numerical values are inserted after a portion of these, but not all have been accomplished. Finally, *arithmetic* or *numerical* developments are those which result when the numerical values of the constants are inserted in place of the general symbols before any step of the development is attempted.

A *literal* development is generally preferable, for this reason, that if it can be performed, the development which results serves for every possible value which can be assigned to the constants. Such, for instance, is the development of the disturbing function due to M. Binet; and if such a development in terms of the requisite variables could be accomplished and carried out to a sufficient extent, and if, being accomplished, numerical values of the constants could be easily introduced, it would be preferable to any other. M. Hansen's development, in his Memoir on the Perturbations of Encke's Comet by Saturn, is a *quasi-literal* development, because a portion only of the processes is general. The conversion of the quan-

tities  $P_{k,i}$  into explicit functions of sines and cosines of multiples of  $f$  is an *arithmetical* process; while the calculation of the quantities  $A_{n,i}$  in p. 29 of M. Hansen's paper, is literal or algebraical.

I regard as very difficult any development of the disturbing function, either *literal* or *quasi-literal*, when the eccentricity of the disturbed body is considerable and the perturbations are large, as in the case of Encke's comet disturbed by Jupiter; and if such were possible, the replacement of the numerous symbols by numbers at the conclusion, would be an operation of almost insurmountable difficulty. On the other hand, in performing an *arithmetical* development according to the rules which I have invented, not only no quantity can be introduced which has a numerical value beneath any given limit (say beneath unity in a given decimal place), but it is equally impossible, except by a numerical mistake, that any quantity which is above that limit can be omitted. The developments may also be effected by mechanical quadratures, as explained by M. de Pontécoulant (*Théor. Anal.*, vol. iii.), or by the method given by M. Le Verrier in the first number of the *Développements sur plusieurs points de la Théorie des Perturbations des Planètes*.

If  $x, y, z$  are rectangular co-ordinates of a comet or planet  $m$ , and  $f$  the true anomaly,

$$x = r\{V \cos f + G \sin f\},$$

$$y = r\{C \cos f + Q \sin f\},$$

$$z = r\{E \cos f + I \sin f\}.$$

\* $V, G, C, Q, E, I$  are constants which depend only on the elliptic constants of the planet  $m$ , and such that

$$V = \cos \pi + 2 \sin^2 \frac{i}{2} \sin (\pi - \nu) \sin \nu,$$

$$G = -\sin \pi + 2 \sin^2 \frac{i}{2} \cos (\pi - \nu) \cos \nu,$$

$$C = \sin \pi - 2 \sin^2 \frac{i}{2} \sin (\pi - \nu) \cos \nu,$$

$$Q = \cos \pi - 2 \sin^2 \frac{i}{2} \cos (\pi - \nu) \cos \nu,$$

$$E = \sin (\pi - \nu) \sin i,$$

$$I = \cos (\pi - \nu) \sin i.$$

\* I have had occasion to use so many alphabets in the course of the work from which this is extracted, that I have had recourse to this artifice of reversing the letters in order not to use the same symbol in two different significations.

Mr. Farley has calculated for me a table of the values of these quantities for all the planets, and also for the comet of Encke, the comet of Biela, and the comet of Halley.

$$\frac{aR}{\mu} = \frac{m' a'}{\mu r'} \left\{ \sigma^2 \frac{a'}{r'} \frac{r}{a} \cos \delta - \eta \{1 + P\}^{-1} \right\}.$$

$$1 + P = 1 - \frac{2\eta^2}{\sigma} \left\{ \left\{ \mathfrak{A} \frac{r}{a} \cos f - \mathfrak{B} \frac{r}{a} \sin f \right\} \frac{a'}{r'} \cos f' \right.$$

$$+ \left\{ \mathfrak{C} \frac{r}{a} \cos f + \mathfrak{D} \frac{r}{a} \sin f \right\} \frac{a'}{r'} \sin f' \left. \right\}$$

$$+ \eta^2 \left\{ \frac{a'^2}{r'^2} \frac{r^2}{a^2} - 1 \right\}.$$

$$\mathfrak{A} = VA' + CC' + EE', \quad -\mathfrak{B} = AB' + CD' + EF',$$

$$\mathfrak{C} = BA' + DC' + FE', \quad \mathfrak{D} = BB' + DD' + FF'.$$

I call the quantities  $\frac{a'}{r'} \cos f'$ ,  $\frac{a'}{r'} \sin f'$ ,  $\frac{r}{a} \cos f$ ,  $\frac{r}{a} \sin f$ ,  $\frac{r^2}{a^2}$ ,  $\frac{a'^2}{r'^2}$ , &c. the *elementary quantities*, because they are the *elements* which, by means of various combinations, form the disturbing function  $R$ ,  $\frac{rdR}{dr}$ , &c., and if the numerical values of the constants are introduced before the development is begun are alone required. Mr. Farley has calculated the coefficients of these quantities when they are developed in terms of the mean motions for the planets, and also other tables for eccentricity,  $\cdot 1, \cdot 2, \dots, \cdot 7$ , which show the convergence to be so slight, that such mode of development can only be employed when the eccentricity is small. These tables have all been constructed by means of mechanical quadratures. These tables are not wanted for the comets, because their co-ordinates cannot be developed in terms of their mean anomalies in sufficiently converging series.

When the method of mechanical quadratures is applied to the determination of the perturbations of comets, a *correction* is required; but when that method is used for the determination of coefficients of this nature, the limits of the integral are 0 and  $360^\circ$ , and the correction vanishes; so that by means of several particular values, rigorous values of the coefficients are easily obtained. Nor does the width of the interval matter, provided it is not made too large. It is difficult to give precise rules to regulate the width that should be employed; but in the formation of these tables it was easy to employ various modes of verification. As this inquiry is in its infancy, I considered it sufficient to retain only those terms which are



due to the elliptic motion; but hereafter it may be desirable to reconstruct the tables of the *elementary quantities* for each of the planets, retaining some of the principal inequalities due to the disturbing force.

Pingré, in his work on comets many years since, gave a list of comets, with their elements. At that time, however, the method of finding the orbit, or even the distance of a comet, was understood by so few persons, that, from that and other causes, the numbers contained in that table may not be accurate: many other comets have been discovered since, and such a table brought up to the present time appears to be an important desideratum in astronomy.

Mr. Hind has kindly favoured me with the following list of comets which have been made out to be periodic:—

Elements of Halley's comet, by Westphalen, for 1835. *Ast. Nach.*, No. 588.

$$e = .96739 \quad \pi = 304^\circ 31' 32''.19 \quad \nu = 55^\circ 9' 59''.34 \\ i = 17^\circ 45' 5''.13 \quad a = 17.98791 \quad \text{Retrograde.}$$

Elements of Encke's comet by Encke, for 1829. *Ast. Nach.*, No. 489.

$$e = .84462 \quad \pi = 157^\circ 17' 58''.35 \quad \nu = 334^\circ 29' 31''.62 \\ i = 13^\circ 20' 34''.49 \quad a = 2.22394 \quad \text{Direct.}$$

Elements of Biela's comet, 1846, by Prof. Plantamour. *Ast. Nach.*, No. 584.

$$e = .75700 \quad \pi = 109^\circ 2' 20''.10 \quad \nu = 245^\circ 54' 38''.8 \\ i = 12^\circ 34' 53''.47 \quad a = 3.52452 \quad \text{Direct.}$$

Elements of the comet of Faye, by M. Le Verrier, for 1844, omitting the terms multiplied by  $\mu''$ . *Ast. Nach.*, No. 541.

$$e = .55596 \quad \pi = 49^\circ 34' 19''.39 \quad \nu = 209^\circ 29' 19''.26 \\ i = 11^\circ 22' 31''.40 \quad a = 3.81179 \quad \text{Direct.}$$

Elements of DeVico's first comet, by Dr. Brünnow, for 1844. *Ast. Nach.*, No. 563.

$$e = .61765 \quad \pi = 342^\circ 30' 49''.64 \quad \nu = 63^\circ 49' 0''.11 \\ i = 2^\circ 54' 50''.33 \quad a = 3.10295 \quad \text{Direct.}$$

Elements of Brorsen's first comet, by Dr. Brünnow, for 1846. *Ast. Nach.*, No. 557.

$$e = .79362 \quad \pi = 116^\circ 28' 34'' \quad \nu = 102^\circ 39' 36''.5 \\ i = 30^\circ 55' 6''.6 \quad a = 3.15021 \quad \text{Direct.}$$

The following are the elements of the comet of Encke for 1829 used by M. Hansen: *Additions à la Conn. des Temps*, 1847, p. 54.

$$e = .844676 \quad \pi = 157^\circ 18' 24''.6 \quad \nu = 334^\circ 29' 28''.8 \\ i = 13^\circ 20' 40''.2 \quad a = 2.21997.$$

*Ast. Nach.*, No. 541.

XVI. *On the Heat of Vapours.*  
By Sir J. LUBBOCK, *Bart., F.R.S.\**

LET  $V$  be the quantity of absolute heat, considered as a function of the sensible heat or temperature  $\theta$ ,

$$\frac{dV}{d\theta} = \frac{dV}{d\rho} \frac{d\rho}{d\theta} + \frac{dV}{dp} \frac{dp}{d\theta} \quad p = k\rho(1 + \alpha\theta),$$

$\rho$  being the density,  $p$  the pressure,  $k$  and  $\alpha$  constants,

$$\frac{d\rho}{d\theta} = -\frac{\alpha\rho}{1 + \alpha\theta} \quad \frac{dp}{d\theta} = \frac{\alpha p}{1 + \alpha\theta}.$$

If  $c$  is the specific heat of a gas, the pressure being constant, and  $c_1$  its specific heat when the volume is constant, so that

$$c = \frac{dV}{d\rho} \frac{d\rho}{d\theta} \quad c_1 = \frac{dV}{dp} \frac{dp}{d\theta} \quad \gamma = \frac{c}{c_1}$$

$$\rho \frac{dV}{d\rho} + \gamma p \frac{dV}{dp} = 0.$$

Laplace evidently considered  $\gamma$  constant, and he integrated this equation upon that hypothesis, “En supposant cette quantité rigoureusement constante, &c.,” *Méc. Cél.* vol. v. p. 127. Again, Poisson, in repeating the same theory, *Traité de Méc.*, vol. ii. p. 646, “En regardant  $\gamma$  comme une quantité constante, &c.” If  $\gamma$  is constant,

$$V = A + B \frac{p^{\frac{1}{\gamma}}}{\rho} = A + B \frac{k}{\alpha} \left( \frac{1}{\alpha} + \theta \right) p^{\frac{1}{\gamma}-1},$$

(see vol. xviii. p. 507) which is identical with the equation given in the *Comptes Rendus*, Séance de 31 Mai 1847, p. 920,

$$q = m + n(a + t)p^{-z},$$

$$m = A, \quad n = \frac{Bk}{\alpha}, \quad a = \frac{1}{\alpha}, \quad t = \theta, \quad z = 1 - \frac{1}{\gamma}, \quad k = \gamma;$$

but if, as Professor Holtzmann maintains (see Taylor's Scientific Memoirs, vol. iv. part 14),  $z$  is variable, the integral of Laplace does not necessarily obtain, nor does the equation (*Comptes Rendus*, p. 920)

$$\frac{dq}{dt} = np^{-z}$$

obtain; because if  $z$  is a function of  $t$ ,

$$\frac{dq}{dt} = np^{-z} - n(a + t)p^{-z} \log p \frac{dz}{dt},$$

\* Communicated by the Author.

and

$$q - q_1 = n(a + t)p^{-\alpha} - n(a + t_1)p_1^{-\alpha}.$$

It has not, I believe, been remarked, that the integral

$$V = A + B \frac{p^{\frac{1}{\gamma}}}{\rho}$$

will however still satisfy the differential equation

$$\frac{\rho dV}{d\rho} + \gamma p \frac{dV}{dp} = 0.$$

If

$$\frac{1}{\rho} \frac{d\gamma}{d\rho} + \frac{1}{\gamma p} \frac{d\gamma}{dp} = 0$$

or

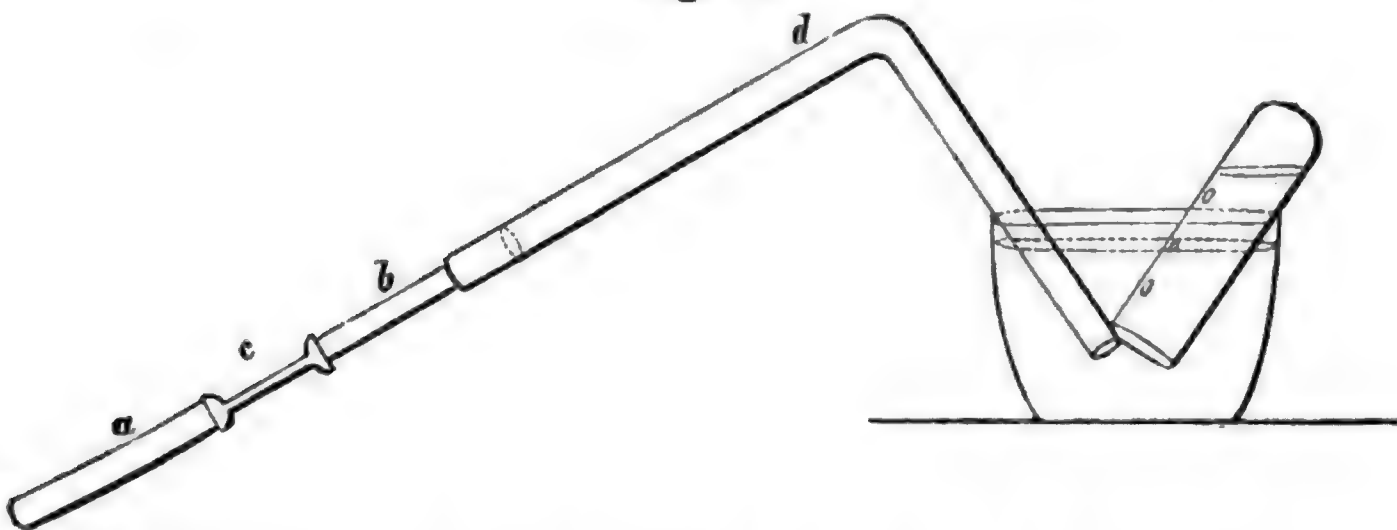
$$\gamma k(1 + \alpha\theta) \frac{d\gamma}{dp} = - \frac{d\gamma}{d\rho}.$$

XVII. *On certain Phænomena of Voltaic Ignition and the Decomposition of Water into its constituent Gases by Heat.*  
By W. R. GROVE, Esq., M.A., F.R.S.

[Continued from p. 35.]

**I** WAS now anxious to produce a continuous development of mixed gas from water subjected to heat alone, in other words, to succeed in an experiment which should bear the same relation to experiment fig. 9 as fig. 5 did to fig. 7; for this purpose the apparatus shown at fig. 10 was constructed: *a* and

Fig. 10.



*b* are two silver tubes 4 inches long by 0.3 inch diameter; they are joined by two platinum caps to a platinum tube *c*, formed of a wire one-eighth of an inch diameter drilled through its entire length, with a drill of the size of a large

pin; *a* is closed at the extremity, and to the extremity of *b* is fitted, by means of a coiled strip of bladder, the bent glass tube *d*. The whole is filled with prepared water, and having expelled the air from *a* by heat, the extremity of the glass tube is placed in a capsule of simmering water. Heat is now applied by a spirit-lamp, first to *b* and then to *a*, until the whole boils; as soon as ebullition takes place, the flame of an oxyhydrogen blowpipe is made to play upon the middle part of the platinum tube *c*, and when this has reached a high point of ignition, which should be as nearly the fusing-point of platinum as is practicable, gas is given off, which, mixed with steam, very soon fills the whole apparatus and bubbles up from the open extremity, either into the open air or into a gas collector. Although by the time I had devised this apparatus I was from my previous experiments tolerably well assured of its success, yet I experienced a feeling of great gratification when on applying a match to one of the bubbles which were ascending, it gave a sharp detonation; I collected and analysed some of it; it was 0.7 oxyhydrogen gas, the residue nitrogen, with a trace of oxygen.

Those who have endeavoured to deprive water of air, will have no difficulty in accounting for the residual nitrogen, or nitrogen mixed with a small portion of oxygen, which has occurred in all my experiments. De Luc pointed out the impossibility of practically depriving water of air, and Priestley, from observing the obstinacy with which water retained air, was led to believe that water was convertible into nitrogen (phlogisticated air). I have repeated several of Priestley's experiments under much more stringent circumstances, and have never been able to free water from air, or so to boil water that for every ebullition of vapour a minute bubble of permanent gas was not left, which appeared to have been an indispensable nucleus to the vapour.

The difficulty of boiling water increases, as M. Donny has proved, in proportion to its freedom from air, and at last the bursts of vapour become so enormous that the vessels employed are generally broken. There appears to me a point beyond which this resistance does not extend; but even at this point a minute bubble of air is left for each burst of vapour, though they are so few and distant that the aggregate amount of gas is very trifling. I have produced from water which had been previously carefully deprived of air by the ordinary methods, three-fourths of its own volume of permanent gas, which proved to be nitrogen; but as the water in this experiment was boiled under a long column of oil, it is probable that if any oxygen were present, it might have been



absorbed by the oil ; I have, however, always found the proportion of oxygen to decrease as the boiling was continued. It may be worth noticing, as having had some influence on my mind, that many months ago, when considering the experiments of Henry and Donny on the cohesion of water, I mentioned to Mr. Gassiot, and also to Mr. Bingham my assistant (to whose assiduity I am much indebted), that I was inclined to think if water could be absolutely deprived of air, it would be decomposed by heat, a result which I have now attained by a totally different series of inductions. It is a circumstance worthy of remark, that I find the greater part of the air to be expelled at a comparatively low temperature, and when the water has come in contact with the platinum, while the decomposition all takes place when the platinum is surrounded by an atmosphere of steam, if steam it may be called, for the state of this atmosphere at the first immersion of the platinum is at present very mysterious.

I think I may now safely regard it as proved, that platinum intensely ignited will decompose water, and several considerations press on the mind in reflecting on this novel phenomenon.

First of all, to those who are attached to the *cui bono* argument, and estimate physical science in proportion only to its practical applications, I would say that these experiments afford some promise of our being, at no distant period, able to produce mixed gases for purposes of illumination, &c. by simply boiling water and passing it through highly ignited platinum tubes, or by other methods which may be devised ; we in fact by this means, as it were, boil water into gas, and there appears theoretically no more simple way of producing chemical decomposition.

To pass however to more important considerations: the spheroidal state, which has lately attracted the attention of philosophers, appears to be closely connected with these results, and is rendered more deeply interesting. The last experiment but two which I have mentioned, shows that the spheroidal state is intermediate between ordinary ebullition and the decomposing ebullition ; it is probably therefore a state of polar tension, coordinate in some respects with that which takes place in the cell of a voltaic combination before decomposition, or when the power employed not being of sufficient intensity to produce actual decomposition, the state commonly called polarization of the electrodes, obtains. The phenomenon brings out also a new relation between heat, electricity, and chemical affinity ; hitherto many electrical phenomena could be produced by heat and chemical action,

the difference being that in the effects produced by the last two forces there was no polar chain, but every minute portion of the matter acted on gave rise to the phænomena which in the electrical effects are only observable at the polar extremities; thus in decomposing water by iron and sulphuric acid, or by passing steam over heated tubes of iron, parallel results are obtained to the electrolysis of water with an iron anode; but in the former cases every portion of the iron oxidated gives off its equivalent of hydrogen, in the latter the equivalent is evolved from the cathode at a point distant from that where the oxidation takes place. Hitherto electricity has been the only force by which many compounds, and particularly water, could be resolved into their constituents without either of these being absorbed by another affinity. The decomposition by ignited platinum removes this exception, and presents the parallel effect produced by heat alone.

Although there is no substance except platinum and some of the more rare metals, such as iridium, which promise much success in a laboratory experiment made for the purpose of producing the effect I have described, as the greater number of substances which will bear a sufficient heat, are fragile, oxidable, or affected by water, yet general considerations from the nearest analogies in chemistry would lead us to expect a similar effect from all matter in a state of intense ignition; even assuming the presence of solid matter to be necessary, the catalytic effects of platinum are shared in different degrees by other substances: it therefore appears probable that at a certain degree of heat water does not exist as water or steam, but is resolved into its constituent elements. If, therefore, there be planets whose physical condition is consistent with an intense heat, the probability is, that their atmosphere and the substances which compose them are in a totally different chemical state from ours, and resolved into what we call elements, but which by intense heat may be again resolved into more subtle elements. The same may be the case in the interior of our planet, subject however to the counter agency of pressure.

The experiments strongly tend to support the views of Berthollet, that chemical and physical attraction are affinal, or produced by the same mode of force. All calorific expansions appear to consist in a mechanical severance of the molecules of matter; and if heat produce effects of decomposition merely by increase of intensity, there seems no reason why we should assign to it in this case a different mode of action from its normal one. On this view physical division carried on indefinitely must ultimately produce decomposition, and

chemical affinity is only another mode of molecular attraction. Thus a high degree of rarefaction, as at the bounds of the atmosphere, or in the interplanetary spaces, may entirely change the chemical condition of matter.

In a paper published in the *Philosophical Transactions* for 1843, p. 111, I have shown that we may oppose a chemical action by a physical one (electrolysis by a vacuum), that antagonizing chemical by physical tension, they mutually oppose each other. I believe the converse of this experiment has been made by M. Babinet, who by physical compression has prevented the development of chemical action.

I have also described in the *Philosophical Magazine* for November 1845, certain phænomena which appear to me to be irreconcilable with received chemical views; and though I then believed that the theory of Grotthus would be obliged to give way, I now incline to think that some of our chemical doctrines must ere long undergo a revision.

It is rather surprising that the valuable applications of which the phænomena of voltaic ignition are capable, and the fertile field which (as I believe) it presents for discoveries, both physical and chemical, should have been so completely neglected. It is true that until a recent period the imperfection of the voltaic battery rendered accurate and continued experiment on this subject difficult of performance, but still much might have been done. Davy made several experiments on the voltaic disruptive discharge, which in many points may be regarded simply as very intense ignition; but I am only aware of two experiments of his on voltaic ignition; one, in which he employed it in an exhausted receiver to examine to what extent the radiation of heat was carried on *in vacuo*; and another, already alluded to, in which, by immersing a portion of an ignited wire in water, he observed that it conducted in some inverse ratio to its heat.

I have made a vast number of experiments on the voltaic arc or disruptive discharge, in various media\*; when this is taken in a medium incapable of acting chemically on the electrodes, the phænomena are those of intense ignition of the terminals, which are dissipated in vapour and condensed upon the interior of the vessel in which the discharge is taken. I have examined some of these deposits, and they appear to consist of the metal of the terminals in a finely-divided state; this is strikingly shown with zinc. If the arc be taken between zinc points in an exhausted receiver, a fine dark powder, nearly black, is deposited on the interior, which, when collected, proves to be pure zinc, and on the application of a

\* *Phil. Mag.*, June 1840; *Literary Gazette and Athenæum*, Feb. 7, 1845.

gentle heat, takes fire in the open air and burns into the white oxide: to casual observation the zinc would appear to be burned twice. The experiment appears to me to present an argument in favour of the dynamic theory of heat.

With charcoal, on the other hand, there is little or no deposit, but the charcoal continually yields carbonic oxide and hydrogen, and this for hours after the presence of water would be deemed impossible. I have taken the arc between pieces of well-burned charcoal for eight or nine successive hours, and there was still gas generated; indeed it appeared to be given off as long as there was any charcoal remaining, and a conversion of the carbon into inflammable gas might have been supposed. Much still remains to be done with this powerful agent, the voltaic arc: where, however, the object is simply to expose gases to an intense heat, the ignition of a conjunctive wire of platinum is more simple in its application, more uniform in its action, and instead of requiring a powerful battery, the effect can be satisfactorily produced by five or six cells, in many cases by two.

The heat is not so intense as that of the arc, but as it can be brought to within a few degrees of the fusing-point of platinum, it is far more intense than any heat usually employed in laboratories, certainly than any which can be applied to minute, I may say microscopic portions of gas or vapour.

In conclusion, I must express my sincere thanks to the managers of the London Institution, for having permitted me, as an honorary member, to carry on these experiments in the laboratory of the Institution.

London Institution, Aug. 21, 1846.

**XVIII.** *Supplementary Paper on certain Phenomena of Voltaic Ignition, and the Decomposition of Water into its constituent Gases by Heat.* By W. R. GROVE, Esq.\*

**I**N selecting the above title, I endeavoured to give as clear an enunciation of the phenomena to be described in the paper as was consistent with the brevity usual in a title.

An exception has, however, been taken to it, that as the effects of decomposition are produced by ignited platinum, the phenomena may result from that obscure mode of action called catalysis. That I did not intend to exclude from consideration any possible action of the substance employed, will be evident from the paper itself, in which I have called attention to the general production of catalytic effects by solid bodies.

\* From the Philosophical Transactions for 1847, part i.; having been received by the Royal Society November 26, and read November 26, 1846.



Whatever value or novelty there may be in the facts I have communicated, is the same whether they be regarded as resulting from catalytic or from thermic actions. If the action be catalytic, it is one absolutely the reverse of that usually produced by platinum, and therefore just as much at variance with received experience as decomposition of water by heat would be; the effect of platinum, like that of heat, on the elements of water having been hitherto known only as combining them. With regard to any theoretic views I may have advanced, I by no means attach the same importance to them as I do to the facts themselves, though I consider it necessary for the collation of facts, and desirable for the progress of science, that an author pretending to communicate new results should give with them the impressions which led to their discovery, and the inferences which he regards as immediately deducible from them. No expression can be given to facts which does not involve some theory, and admitting the difficulty (perhaps insuperable) of correctly enunciating new phænomena, and the probability of future discoveries entirely changing our views regarding them, I cannot at present see that the title of my paper could be altered without being open to greater objections. I am of this opinion, not so much because other bodies than platinum will produce the effect, as I shall presently show, nor from the fact that the electrical spark will decompose aqueous vapour, though these are arguments in its favour; but from the following considerations. The catalytic action of platinum will induce or enable combination to take place where there is already a strong affinity or tendency to combine, as with mixed oxygen and hydrogen gases; it will also induce decomposition where the affinities are extremely weak, or in a state of unstable equilibrium, as in Thenard's peroxide of hydrogen; again, where there are nicely-balanced compound affinities, it may change the chemical arrangement of the constituents of a compound, but I do not know of any case in which a powerful chemical affinity can be overcome by catalytic action; to effect this we require some natural force of greater intensity than that to be overcome. We might as well say that the platinum electrodes of a voltaic battery decompose water, as to say that platinum decomposes it in the case in question; there, the force of electricity acts only by means of matter, and matter of a peculiar description; its action also is only perceptible at the surface of this matter. I seek to use the expression in my title with reference to heat in a similar sense to that in which we use similar terms with reference to electricity, *i. e.* to regard heat as the immediate dynamic force which overcomes the

affinity; thus, as we say when employing the voltaic battery, that we decompose water by electricity, so here we should say that we decompose it by heat.

If it be said that heat so weakens or antagonizes the affinity of the elements of water as to enable catalytic action to separate them, this amounts to the same theory, as heat is then regarded as the antagonizing force, and in this case the action, both thermic and catalytic, is the reverse of the normal action. I have thought it desirable shortly to discuss this question as likely to lead to further investigation, though I have been somewhat embarrassed by the want of definite meaning in the term catalysis; I must plead guilty to have frequently used the term, but notwithstanding, or perhaps on account of, its convenience, it has I fear had an injurious effect on scientific perspicuity.

The following experiments were made to ascertain whether platinum was the only substance by which the effect could be produced. A knob or button of the native alloy of iridium and osmium of the size of a small pea was formed by the voltaic battery; to this was attached by fusion another smaller knob of the same metal one-fourth the size of the former, and to this smaller one was attached a stout platinum wire; the object of the second knob was both to prevent the fusion of the platinum wire and also to avoid the possibility of any surface of platinum being exposed to the recipient tube or alloyed with the metal to be heated. The preparation of this simple instrument was very troublesome, but when made it answered the purpose well; the larger button could be fully ignited to an intense glow, while on account of the narrow neck which united them, the smaller was barely red-hot, and the platinum wire not perceptibly ignited. An experiment having been made with this metallic button and prepared water, similar to that previously made with platinum, gas was given off which averaged 0·3 of mixed gas; the residue was nitrogen mixed with varying small quantities of oxygen. The effect, upon the whole, was decidedly inferior to that of the platinum. Indeed as platinum is the most dense and unalterable of all known substances, it would be likely, upon any received theory of heat, to produce the greatest effects.

I tried palladium in the same manner; the gas yielded was hydrogen with small quantities of oxygen, and the water was stained with the oxide of the metal.

I now tried silica and other oxides, but the results were not very satisfactory. A spheroid of silica was formed by fusing pulverized silica on to a platinum wire, so as to cover it for the length of 0·4 of an inch; when this was plunged into

the hot water and again fused in the oxyhydrogen blowpipe, it constantly became frothed with small bubbles of vapour, and after a few experiments generally separated in fissures; in the experiment which was continued for the longest time without disintegration, the gas given off contained 0.15 of oxyhydrogen gas; from the whole result I believe there is an action of the water on the silica (probably forming a hydrate decomposable by heat) which is a bar to satisfactory results. With other oxides, at least such as would bear an intense heat, the difficulties were still more insuperable. Priestley has shown that water will corrode glass, and if I mistake not, others have shown the same effect produced on silica.

Although, as applied to the facts detailed, I attached no further meaning to the title of my paper than that which I have above stated, yet in one or two theoretical inferences I have certainly gone further; for instance, when I suppose the possibility or probability of mechanical rarefaction producing the same effects as heat, here (although I do not, indeed I cannot conceive the existence of heat without matter) I certainly abstract from the proposition any consideration of solid matter. In order to ascertain how far this view might be founded on truth, I had thought of making a few experiments on the effect of mechanical rarefaction on the tendency of gases to combine, but (in addition to the interference of necessary occupations) I find that M. de Grotthus has already experimented on the point; his experiments, as far as they go, corroborate the views I have put forth.

He finds\* that mixed gases, such as chlorine and hydrogen, or oxygen and hydrogen, when rarefied either by slow increments of heat or by the air-pump, do not take fire ("ne s'enflamment pas") by the electric spark. From the context, he evidently means that the gases will not detonate or unite in volumes, as he states that a partial combination ensues. Grotthus appears to have considered the combination of gases by the electric spark as an effect of sudden compression or molecular approximation, certain particles being brought within the range of their affinities by the sudden dilatation of others. Although he did not pursue the subject far enough to ascertain whether a degree of rarefaction could be reached which would be an actual bar to combination, still his experiments strengthen those views which assimilate mechanical and thermic molecular repulsion, and regard chemical affinity as being antagonized by physical repulsion.

Pursuing the series of analogies from the decomposition of euchlorine at a low temperature, that of ammonia at a higher,

\* *Annales de Chimie*, vol. lxxxii.

that of metallic oxides at a higher, and so on to oxide of hydrogen, there appears to be an extensive series of facts which afford strong hope of a generalized antagonism between thermic repulsion and chemical affinity, and a consequent establishment of the law of continuity in reference to physical and chemical attraction.

The deposit from chlorine, to which I have alluded in my paper, I have since examined, and though it differs in colour from that described in books, I find it is a protochloride of platinum, formed at the expense of the platinum wire. The larger portion of the chlorine in the tube combines with the hydrogen of the aqueous vapour, and the muriatic acid is absorbed by the water; when the experiment terminates the gaseous volume is reduced to nearly one-half, and this residue is oxygen.

This effect induced me to try an ignited wire on other analogues of chlorine, and I tried bromine and chloride of iodine in the apparatus (fig. 5). The tube was filled with the liquid, and its extremity was in the first experiments immersed in another narrow tube of the same liquid as that which filled it. When the platinum wire was ignited, permanent gas was given off both from the bromine and from the chloride of iodine, which gas on examination proved, to my surprise, to be oxygen. In one experiment I collected half a cubic inch of gas from an equal volume of chloride of iodine. As the experiment in this form required too large a quantity of the liquid to enable me to observe any change which might take place in its character, I repeated it with a tube five feet long, bent in two angular curves. A small quantity of the liquid was placed in the extremity of the tube containing the wire, which was so arranged as to be the lowest point; the angles were placed in cold water and the experiment proceeded with; my object was to enable the dense vapour of the liquids to shelter them from the atmosphere, there being no satisfactory method of shutting them in and yet allowing room for the elimination of the liberated gas, or of absorbing the latter by combination without also absorbing the vapours.

I had hoped by the above means to proceed with the experiments until all the oxygen was liberated that could be driven off, and then to have examined the residua; but I found that after experimenting for a short time, both the platinum wire and the glass in proximity to it were attacked by the liquids; this difficulty, similar to those which have hitherto prevented the isolation of fluorine, I have not yet been able to conquer, though I hope to resume the experiments.

As chloride of iodine is decomposed by water, it cannot



FIG. 1.

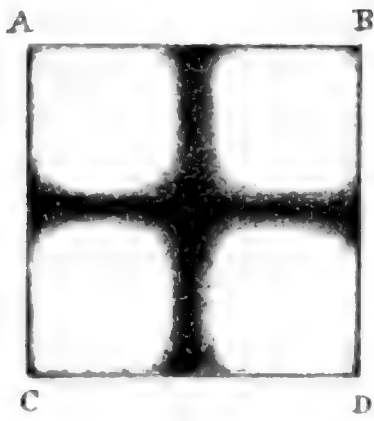


FIG. 2.

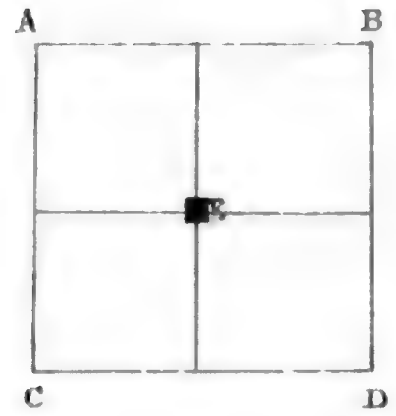


FIG. 3.

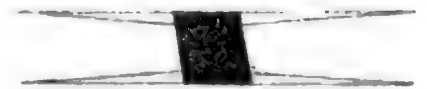
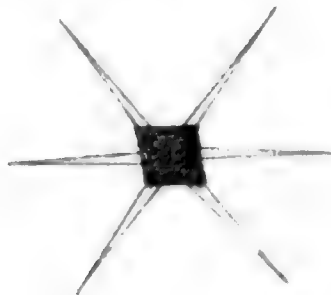
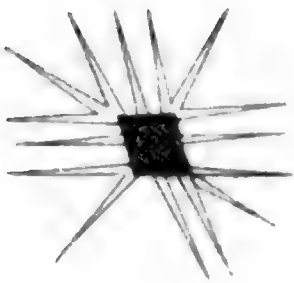
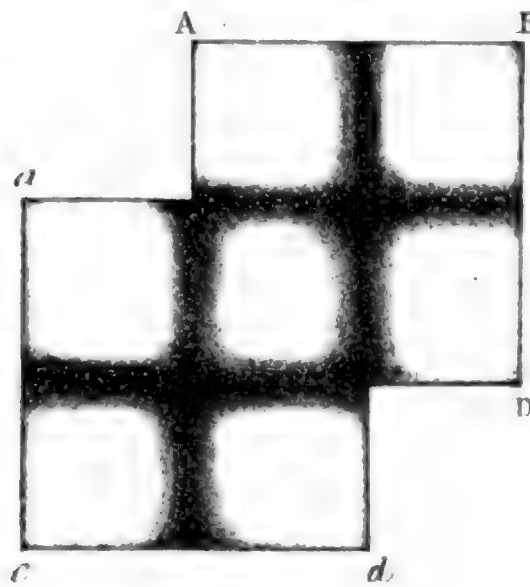


FIG. 4.

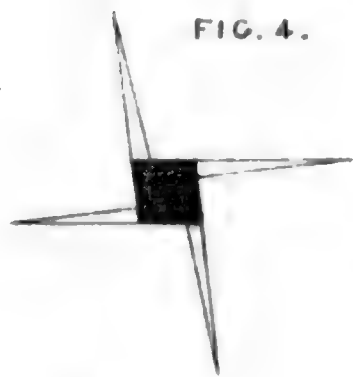


FIG. 5.

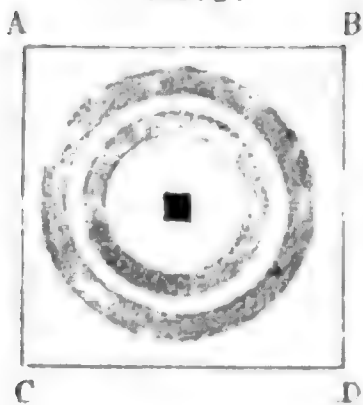
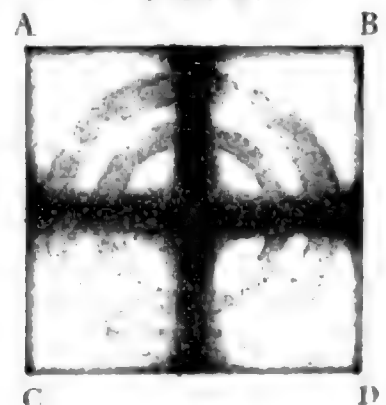


FIG. 6.



contain any notable quantity of the latter, but, until the experiments are carried further, it must remain a question whether the oxygen results from a small quantity of water contained in the liquid, the hydrogen combining with the liquid itself, or from a decomposition similar to that of the peroxides. The experiments certainly add a new and striking analogy to those already known to exist between the peroxides and the halogens, but they do not, as far as I have hitherto carried them, necessarily prove analogy of composition.

In conclusion, I would call attention to a point which I omitted to notice in my original paper, viz. the explanation afforded by the results contained in it of the hitherto mysterious phenomena of the non-polar decomposition of water by electrical discharges, as in the experiments of Pearson and Wollaston. This class of decompositions may now be carried much further. With the exception of fused metals, I know of no liquid, which, when exposed to intense heat such as that given by the electric spark, the voltaic arc, or incandescent platinum, does not give off permanent gas; phosphorus, sulphur, acids, hydrocarbons, water, salts, bromine and chloride of iodine, all yield gaseous matter.

Viewing these effects simply as facts, and without entering on any theoretical explanations or speculations, I cannot but think that there is a remarkable generality pertaining to them worthy of the most careful attention.

The apparatus I have described, particularly that represented by fig. 5, and the numerous applications of voltaic ignition which will occur to those who duly consider the subject, promise, I venture to believe, new methods and powers of investigating the molecular constitution of matter, and will, I trust, lead to many novel and important results.

Nov. 10, 1846.

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XIX. *On the Modification of the Doubly Refracting and Physical Structure of Topaz, by Elastic Forces emanating from Minute Cavities.* By Sir DAVID BREWSTER, K.H., D.C.L., F.R.S., and V.P.R.S. Edin.\*

[With a Plate.]

WHILE examining, in polarized light, the form and structure of the numerous crystals which I had discovered in the fluid cavities of *topaz*, my attention was particularly called to certain optical phenomena exhibited in other parts of the specimen. These phenomena, when first

\* Read before the Royal Society of Edinburgh on the 20th of January 1845, and published in their Transactions, vol. xvi. part 1. p. 7.

presented to me, were very indefinite in their character, and very imperfectly developed; but after a diligent examination of nearly 900 specimens of topaz, I succeeded in obtaining the most satisfactory exhibition of them under various forms, and in various degrees of intensity.

When an elastic force is propagated from a centre, in a soft and compressible medium, an increase of density is communicated to the surrounding mass,—of a temporary nature if the medium is a hard solid, like glass, but of a permanent nature if the medium is soft, and becomes indurated during the continuance of the compressing force. Both these effects may be exhibited experimentally; the first by a pressure upon glass, and the second by the action of an expanded bubble of air upon gum in a state advancing to induration.

The physical change thus produced in the transparent medium, whether it be temporary or permanent, may be exhibited to the eye in two ways; either by the property of the compressed parts in depolarizing light, or in the unequal refraction of common light produced by a varying density, and consequently a varying refractive power. In the *first* of these cases, the depolarizing action is displayed in the production of four quadrants of light, separated by the radii of a black rectangular cross, similar to the central portion, or the tints of the first order, in the uniaxal system of polarized rings; and, in the *second* case, the inequality of refractive density is shown by the mirage of a luminous point, in the form of concentric circles surrounding the centre of force, each circle marking successive actions of the central force.

When the four luminous quadrants of depolarized light, shown at A, B, C, D in Plate I. fig. 1, first presented themselves to me, I had some difficulty in perceiving the seat of the force, by which I believed that they were produced. The centres, or intersections of the black cross, were either too deep beneath the surface of the topaz, or too much covered by fluid cavities, to be seen; but by removing the part of the crystal which contained these cavities, I succeeded in finding that in every case there was a minute cavity in the centre of the luminous quadrants, or at the intersections of the arms of the black cross, from which the compressing force had emanated. One of these cavities is shown at E, fig. 2. It is of a quadrangular form, like the section of a rhomboidal prism, sometimes elongated, and sometimes of a slightly irregular shape. When perfectly regular, these cavities are between the 3000<sup>th</sup> and the 4000<sup>th</sup> of an inch in diameter. They are always dark, as if the elastic substance which they contained had collapsed into an opaque powder; and I have met

with only one case in which there seemed to be a speck of light in the centre. The degree of compression to which the topaz has been subjected is measured by the polarized tint developed in the luminous quadrants. It varies from the faintest pale *blue* to the *white* of the first order. In one case I found the luminous quadrant of one cavity coinciding with a luminous quadrant of another cavity, and thus producing the sum of their separate tints. This effect is shown in fig. 3.

In the phænomenon now described, the elastic force has spent itself in the compression of the topaz. The cavity itself has remained entire, without any fissure by which a gas or a fluid could escape. I have discovered, however, other cavities, and these generally of a larger size, in which the sides have been rent by the elastic force; and fissures, from *one* to *six* in number, propagated to a small distance around them. These fissures have modified the doubly refracting structure produced by compression; but, what is very interesting, no solid matter has been left on the faces of fracture, such as that which is invariably deposited, when an ordinary cavity, containing one or both of the two new fluids, is exploded by heat. The form of some of the cavities which have suffered this disruption is shown in fig. 4.

The influence of the compressing forces in altering the density, and consequently the refractive power of the topaz, is so distinctly seen in common light as to indicate the phænomena that are seen under polarized light. When the cavity is most distinctly perceived, it is surrounded with luminous and shaded circles, as shown in fig. 5; and traces of these are distinctly seen, as shown in fig. 6, when the specimen is examined in polarized light.

The cavities now described have obviously no resemblance whatever to those which I have described in previous papers as containing two new fluids. When any of the latter are either burst by heat, or exposed under high temperatures to the compressing forces of the fluids which they contain, they exhibit none of the phænomena peculiar to the former. The doubly refracting structure suffers no change; and when the cohesive forces of the crystal are overpowered, the faces of most eminent cleavage separate, and are covered with translucent crystalline particles, which the evaporated or discharged fluids leave behind.

The peculiar character of the *pressure cavities*, as we may call them, is still further evinced by the nature of the specimens in which they occur. I have never found them accompanying the ordinary cavities with two fluids. The specimens which contain them have imbedded in them numerous crystals,



differing little in their refractive power from topaz, and exhibiting in polarized light the most beautiful colours, varying with the thickness of the crystal, and diminishing in intensity as their axes approach to the plane of primitive polarization.

It is impossible to review the preceding facts without arriving at the conclusion, that the topaz must have been in a soft and plastic state when it yielded to the compressing force which emanated from the cavities; and that a mineral body thus acted upon could not have been formed, according to the received theory, by the aggregation of molecules having the primitive form of the crystal.

In a letter to Sir Joseph Banks, printed in the Philosophical Transactions for 1805, I deduced, from my experiments on depolarization, the existence of a new "species of crystallization, which is the effect of time alone, and which is produced by the slow action of corpuscular forces;" and I have remarked that "this kind of crystallization will probably be found to have had an extensive influence in those vast arrangements which must have attended the formation of our globe." These views have been confirmed by various new facts, wholly independent of each other;—by the existence of crystals imbedded in topaz, and having their axes in all possible directions, but especially by the nature and form of the strata of fluid cavities in that mineral. These strata cut at all inclinations the primary and secondary planes of the crystal. They are bent in the most capricious manner, forming planes of double curvature; and, what is also true of individual cavities stretching in every possible direction, they could never have been formed but when the topaz was in a soft and plastic state.

An objection to these views may be drawn from the fissures which proceed from the pressure cavities. The topaz must, doubtless, have been indurated when these fissures took place; but it is equally obvious that the depolarization produced by compression must have previously existed, and it is probable that the fissures were produced after the crystal had been removed from its matrix, and when, from cleavage or otherwise, its cohesive forces had been diminished.

St. Leonard's College, St. Andrews,  
January 16, 1845.

**XX. *Researches on the Composition and Characters of certain Soils and Waters belonging to the Flax districts of Belgium, and on the Chemical Constitution of the Ashes of the Flax Plant.* By Sir ROBERT KANE, M.D., M.R.I.A.**

[Continued from p. 45.]

**3. *Results of the Examination of the Ashes of Flax grown upon the Soils previously analysed.***

A. This was coarse flax; and the flax of this district is usually of rather poor quality. It is however in most cases sown late, about the 15th of May.

On incineration, this flax was found to give of pure ash, in average, 4·237 per cent.

The stem, dried at 212°, and analysed, was found to contain 0·982 per cent. of nitrogen.

The ash contained, per cent., after deducting the sand and charcoal, which can be considered but as accidentally present :

Potash . . . . .	7·697
Soda . . . . .	19·186
Lime . . . . .	15·379
Magnesia . . . . .	3·446
Oxide of iron . . . . .	4·501
Alumina . . . . .	0·444
Oxide of manganese . . . . .	a trace
Sulphuric acid . . . . .	6·280
Phosphoric acid . . . . .	11·206
Carbonic acid . . . . .	20·599
Chloride of sodium . . . . .	8·213
Silica . . . . .	3·056
	<hr/>
	100·000

B. This flax was of the very best description, and was grown from first-class seed.

The stem, dried at 212°, and analysed, was found to contain per cent 0·756 of nitrogen.

On incineration, the plant, dried at 212°, yielded in average 5·434 per cent. of pure ash.

After deducting the sand and charcoal accidentally present, the ash was found to contain per cent., —

Potash . . . . .	22·897
Soda . . . . .	none
Lime . . . . .	16·483
Magnesia . . . . .	3·332
Peroxide of iron . . . . .	1·523
Alumina . . . . .	0·438
Oxide of manganese . . . . .	a trace
Sulphuric acid . . . . .	6·174
Phosphoric acid . . . . .	11·802
Carbonic acid . . . . .	25·235
Chloride of sodium . . . . .	8·701
Silica . . . . .	3·409
	<hr/>
	99·994

C. This flax was very fine, and was said to be as good as any grown in that season.

The stem, dried at 212°, and analysed, was found to contain, per cent., 0·876 of nitrogen.

On incineration, the plant, dried at 212°, yielded in average 3·670 per cent. of pure ash.

After deducting, as usual, the sand and charcoal, the ash was found to contain per cent.,—

Potash . . . . .	22·303
Soda . . . . .	14·116
Lime . . . . .	18·525
Magnesia . . . . .	3·933
Peroxide of iron . . . . .	1·100
Alumina . . . . .	0·725
Oxide of manganese . . . . .	a trace
Sulphuric acid . . . . .	6·833
Phosphoric acid . . . . .	8·811
Carbonic acid . . . . .	16·383
Chloride of sodium . . . . .	4·585
Silica . . . . .	2·678
	<hr/>
	99·992

D. This flax, of a rather coarse quality, had been sown May 2nd, and pulled July 29.

The plant, dried at 212° and analysed, yielded 0·901 per cent. of nitrogen.

On incineration after desiccation, it gave 4·543 per cent. of ashes.

The composition of the ash per cent. was—

Potash . . . . .	25.790
Soda . . . . .	.429
Lime . . . . .	19.098
Magnesia . . . . .	3.648
Peroxide of iron . . . . .	2.281
Alumina . . . . .	none
Oxide of manganese . . . . .	none
Sulphuric acid . . . . .	12.091
Phosphoric acid. . . . .	10.983
Carbonic acid . . . . .	9.895
Chloride of sodium . . . . .	12.751
Silica . . . . .	3.030
	<hr/>
	99.996
Loss . . . . .	.004
	<hr/>
	100.000

H. The flax grown upon the Dutch soil yielded, on analysis, 1.000 per cent. of nitrogen, when dried at 212° Fahrenheit.

It also gave, by incineration, 5.151 per cent. of ashes, of which the composition per cent. was found to be as follows:—

Potash . . . . .	18.410
Soda . . . . .	10.912
Lime . . . . .	18.374
Magnesia . . . . .	3.023
Peroxide of iron . . . . .	2.360
Alumina . . . . .	1.439
Oxide of manganese . . . . .	none
Sulphuric acid . . . . .	9.676
Phosphoric acid . . . . .	11.058
Carbonic acid . . . . .	13.750
Chloride of sodium . . . . .	5.655
Silica . . . . .	5.327
	<hr/>
	99.984
Loss . . . . .	.016
	<hr/>
	100.000

If we examine somewhat in detail the results of the ash analyses above given, there will be found several points worthy of attention, in reference to the probable laws of replacement of acids and bases, as mineral constituents of plants; and also with regard to the necessary presence of certain materials.

It will be seen that in all cases a large proportion of the bases of the ash had been combined with organic acids, and were hence found in the ash as carbonates. This quantity is,



however, variable; and it will be seen that a variation takes place in the quantity of sulphuric acid exactly of an opposite character; so that in the plant, the proportions of organic salts and of sulphates would appear to have been such, that an increase in one replaced any deficiency of the other. Thus when the quantity of carbonic acid in the ash was 25·235, the sulphuric acid was 6·174; but when the sulphuric acid was 12·091, the carbonic acid fell to 9·895. I do not however mean absolutely to assert that the sulphuric and the organic acids of the plant are, in all cases, or exactly, mutually replacing.

The small quantity, as well as the narrow limits of fluctuation of the silica, is worthy of notice; particularly when compared with that which I shall have to notice as regards the composition of Irish flax. It does not appear connected with any of the bases in particular, nor to follow any special variation among them.

There is nothing more peculiarly characteristic in the composition of the ashes of the flax plant, than the quantity of phosphoric acid which is found therein. In order to bring this into full evidence, I shall extract from the works of other chemists the determination of the quantity of phosphoric acid in the ash yielded by the stems of other plants.

Tobacco stalk and leaves . . .	2·73
Wheat stems . . . . .	3·10
Oat stems . . . . .	3·00
Clover plants . . . . .	6·30

The stems of flax are, then, more than double as rich in phosphoric acid as the stems of even the cereal grasses or leguminous plants; and if we even look to the constitution of the ash of many substances used as food by man, we shall find that, in 100 parts, there are from the ash of—

Oats . . . . .	14·9	phosphoric acid
Potatoes . . . . .	11·3	...
Turnips . . . . .	6·1	...

whilst the average of the analyses of Belgian and Dutch flax ashes show that there are present no less than 10·77 per cent. It was this enormous quantity of the most valuable ingredient of manure that first impressed me with the importance of its œconomy, and induced me to endeavour to fix the attention of agriculturists upon the fact; for if we calculate, from the produce per acre, the quantity of phosphoric acid taken from a statute acre of ground by an ordinary crop of any of the usual kinds, we shall find that it amounts in the case of flax to very nearly as much as with any of the ordinary grain or root crops;

and that whilst the mineral elements of these are what the value really consists in, the value of the flax is altogether independent of those constituents, which are thus so much real loss to the farmer.

Hence, under the ordinary plan of cultivation, farmers were certainly in the right to consider it one of the most exhausting crops; and that its place in rotation should be equivalent to that of a grain crop, which it ought by no means to follow, or be followed by; whereas, under a system of management which should allow of the proper œconomy of its mineral constituents, that are separated in the processes of watering and dressing, the phosphoric acid and other materials might be restored to the manure heap or to the field, and the crop of flax be thus deprived of those permanently exhausting qualities which it now possesses.

It will be interesting further to notice the constitution of these ashes, under a point of view which has been put forward by some chemists, as possessing the character of a general rule or law; to wit, that although the individual bases present in an ash may vary very much, and even some (as in one of the ashes analysed, B soda) may be totally absent, yet the sum of the oxygen present in the bases will be found to be constant. If we apply that rule to the ashes above analysed, we shall find—

Title of ash.	Quantity of oxygen in bases.
A . . . . .	13·73
B . . . . .	10·95
C . . . . .	14·65
D . . . . .	13·45
H . . . . .	13·60
Average .	13·28

There is certainly a close agreement among these numbers; and if we excluded one analysis (B), which is also exceptional in containing no soda, it should decidedly appear that the quantity of oxygen present in the bases of 100 parts of ash was represented by a constant number (13·86). It will be found that the analyses of Irish flax lend support to this view; but I think that we shall require very many more analyses before we can fix upon it as a positive law.

In order to afford comparison with the results above given, I have extended my analyses of Irish flax; and as there appear one or two remarkable points of difference between them, I shall notice also my prior results.

The flax I originally experimented on was grown at my own residence, a short distance from Dublin. It yielded,

110 Sir Robert Kane *on the Chemical Constitution of the*

when dried at  $212^{\circ}$ , 0·56 of nitrogen per cent., and 5 per cent. of ashes, consisting of, in 100 parts,—

Potash . . . . .	9·78
Soda . . . . .	9·82
Lime . . . . .	12·33
Magnesia . . . . .	7·79
Alumina . . . . .	6·08
Phosphoric acid . . . .	10·84
Sulphuric acid . . . . .	2·65
Carbonic acid . . . . .	16·95
Chlorine . . . . .	2·41
Silica . . . . .	21·35
	<hr/>
	100·00

I selected for another analysis a specimen of flax given to me by William Blacker, Esq., which had received a prize at the Market-hill show by the tenants of the Earl of Gosford. When dried at  $212^{\circ}$ , this flax yielded 0·672 per cent. of nitrogen, and 5·572 per cent. of ashes, which contained per cent.—

Potash . . . . .	6·332
Soda . . . . .	6·350
Lime . . . . .	22·699
Magnesia . . . . .	4·058
Peroxide of iron . . . .	13·520
Oxide of manganese . . .	1·092
Alumina . . . . .	none
Sulphuric acid . . . . .	8·929
Phosphoric acid . . . . .	7·002
Carbonic acid . . . . .	4·107
Chloride of sodium . . .	0·901
Silica . . . . .	24·978
	<hr/>
	99·968

There is first to be remarked the very curious circumstances of both Irish specimens containing a large quantity of silica, from 21 to 25 per cent., whilst the Belgian and Dutch flax contained only from 3 to 5 per cent. In the Dublin flax there is no particular replacement to which this could be attributed; but in the Armagh flax, the small quantity of carbonic acid, only 4 per cent., shows that the organic acids had been but little generated in the plant, and probably a quantity of silica was substituted for them. The question of whether this large quantity of silica, which, however, is mostly removed from the fibre along with the other materials during its dressing, could produce in it any degree of hardness or brittleness, is very well worthy of the attention of the philosophical agriculturist.

It is remarkable also, that in both Irish flaxes the potash and soda are present in equal quantities, though not in the same quantity in each ash. This, however, may be only a coincidence, though still a remarkable one.

A more interesting peculiarity is the presence, in the Armagh flax, of the very large quantity of peroxide of iron, 13·5 per cent. In the Dublin flax I have not formerly counted iron as an ingredient, although I did find in the analyses a small quantity, because I had burned the plants on a sheet of iron wire-gauze, and I feared that a minute quantity of iron might be derived from that; and also that, in that analysis, my only object was to show the presence of large quantities of valuable ingredients, which the farmer ought to economise. I therefore did not separately determine that minute trace of iron, which, however, could in no way affect the numerical results. The occurrence of the large quantity of iron in the Armagh flax is, therefore, the more curious; and it will be interesting to examine, by other analyses of the flax sown in the sandstone districts of the north of Ireland, whether the same proportion of oxide of iron will be found.

Notwithstanding the great difference in the quantity of silica in the Irish flaxes from the Belgian, the proportion of oxygen per cent. in the bases comes out nearly the same. Thus the bases contain of oxygen,—

Flax from Dublin . . . .	13·41
Flax from Armagh . . . .	13·66

closely coinciding with the number already found for the Belgian and Dutch flax.

It is not unimportant to correct a statement recently made, that prepared fibre of flax is not so destitute of mineral constituents as I have assumed in the preceding investigations. In order to arrive fully at the truth, I have instituted some additional experiments, with the following results:—

A. Very imperfectly dressed flax from the county Clare gave, by incineration, with proper precautions, 0·97 per cent. of ashes, containing principally oxide of iron and lime.

B. A specimen of perfectly dressed flax from Belfast gave, on incineration, 0·62 per cent. of ashes.

C. A specimen of fine dressed linen gave, on incineration, 0·24 per cent. of ashes, principally lime, with some oxide of iron. Hence it is evident that my former results on this point were precisely confirmed by these new trials.

#### 4. *Results of the Examination of the Waters selected for steeping Flax in Belgium.*

No. 1. This water is from a large pond near the bank of



the Scheldt, which has been most likely formed by digging out peat for fuel, as the soil near it is peat, and as in neighbouring ponds peat is now scraped up from the bottom, and prepared for fuel by drying in the sun. This water is renewed by the overflowing of the Scheldt, and is apparently not at all peaty.

This water was pretty clear, but contained some suspended matter. When 100,000 grains were evaporated to dryness there was obtained 51·70 grains of residue, consisting of, in 100 parts,—

Protoxide of iron . . . . .	·514
Lime . . . . .	6·940
Magnesia . . . . .	·856
Soda . . . . .	28·620
Potash . . . . .	8·740
Sulphuric acid . . . . .	8·054
Muriatic acid . . . . .	25·765
Phosphoric acid . . . . .	no trace
Carbonic acid, with organic matter and loss . . . }	20·511
	<hr/> 100·000

No. 2. Water from one of the best Bloe retting pits, near Hamme Log, in Belgium. This water is also supplied from the Scheldt annually, before the retting season commences, and left to stand in the pit for six or eight weeks. The top becomes covered with green weeds which are cleared off immediately the flax is put in. This causes the water to be muddy, as there is a considerable thickness of mud at the bottom which is disturbed, the workmen standing in the pit when cleaning the top of the water. The flax is then laid in; and after laying two or three layers, they shovel up some of the mud in the bottom to put on the flax to sink it; and when the pit is full, the flax is covered by about an inch thickness of mud. This sample was taken from a pit which had just been disturbed and mudded by cleansing the top of weeds, preparatory to putting the flax in.

This water was found very muddy, but the suspended matter was principally organic.

100,000 grains left by evaporation 139·69 grains of solid matter, of ochrey appearance, and consisting, per cent., of—

Protoxide of iron . . . . .	6·633
Lime . . . . .	8·435
Magnesia . . . . .	1·369
Soda . . . . .	11·607
Potash . . . . .	4·181
Sulphuric acid . . . . .	8·435
Muriatic acid . . . . .	8·682
Phosphoric acid . . . . .	no trace
Carbonic acid, with organic matter and loss . . . }	50·658
	<hr/> 100·000

No. 3. This water is from a large pond similar to that from which No. 1 is taken, but from a different part of the country, and a much larger body of water.

It was clear, containing but very little suspended matter.

100,000 grains left on evaporation 50·68 grains of solid residue, which consisted of, per cent.,—

Protoxide of iron . . . . .	2·584
Lime . . . . .	17·829
Magnesia . . . . .	1·530
Soda . . . . .	30·232
Potash . . . . .	15·762
Sulphuric acid . . . . .	11·627
Muriatic acid . . . . .	2·580
Phosphoric acid . . . . .	no trace
Carbonic acid, with organic matter and loss . . . }	17·856
	<hr/> 100·000

No. 4. This water is from the river Lys, so celebrated for its steeping qualities. It was taken from the river in France before it had reached the highest retting place. The specimen was clean, but there was some suspended matter, principally organic.

100,000 grains, evaporated to dryness, left a residue of 45·11 grains, consisting of, in 100 parts,—

Protoxide of iron . . . . .	6·200
Lime . . . . .	5·484
Magnesia . . . . .	1·192
Soda . . . . .	28·298
Potash . . . . .	5·405
Sulphuric acid . . . . .	9·300
Muriatic acid . . . . .	7·754
Phosphoric acid . . . . .	·079
Carbonic acid, with organic matter and loss . . . }	36·288
	<hr/> 100·000

No. 5. This water was from a retting pit in Holland.

100,000 grains, evaporated to dryness, gave a residue of 42·4 grains, which consisted, per cent., of—

Protoxide of iron . . . . .	1·183
Lime . . . . .	3·613
Magnesia . . . . .	7·601
Soda . . . . .	19·277
Potash . . . . .	8·205
Sulphuric acid . . . . .	5·607
Muriatic acid . . . . .	9·439
Carbonic acid, with organic matter and loss. . . . }	45·075
	<hr/> 100·000

With regard to the constitution of these several specimens of water, it can only now be remarked, that in all there was present a large quantity of mineral impurities; and that in Nos. 2 and 4, the very samples which are of the most remarkable and celebrated steeping waters in Belgium, a large quantity of iron is present, so that they might be in a degree termed chalybeate waters. How this regards their excellence for preparing flax I do not pretend to say, and indeed it will require much more extended investigation before a satisfactory solution of it can be given.

All these waters are further remarkable for containing a larger quantity of potash than ordinary waters are found usually to have. I shall not, however, enter minutely into the discussion of their constitution, as I shall have to resume the subject at another time; and I wish only to place on record for the present the analytical results which the samples of waters forwarded to me from Belgium by Mr. Marshall, had afforded.

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#### XXI. On the Theoretical Velocity of Sound. By J. P. JOULE\*.

**T**HE celebrated French mathematician De Laplace has, it is well known, pointed out that the heat evolved by the compression of air is the cause of the velocity of sound, according to the theory of Newton, being so much less than that actually observed. He has also given a formula by which the velocity may be determined when the ratio of the specific heat of air at constant pressure to that at constant volume is known. The determination of the elevation of temperature in air by compression has however been hitherto attended with difficulty, and hence the theorem of De Laplace has never yet been fairly compared with experiment. I was therefore anxious

\* Communicated by the Author.

to ascertain how far the mechanical equivalent of heat, as determined by my recent experiments on the friction of fluids, might be able to contribute to clear up this question.

The capacity of air at constant pressure, according to the experiments of De la Roche and Berard, is 0.2669. Consequently a quantity of heat capable of increasing the temperature of a lb. of water by  $1^{\circ}$ , will give  $1^{\circ}$  also to 3.747 lbs. of air, while the air will be expanded  $\frac{1}{491}$ ; an expansion in which a force equal to 200.7 lbs. through a foot is expended in raising the atmosphere of the earth. The equivalent of a degree of heat per lb. of water, determined by the careful experiments brought before the British Association at Oxford, is 775 lbs. through a foot. Hence 200.7 lbs. through a foot is equal to  $0^{\circ}.259$ .

We see, therefore, that for every degree of heat employed by De la Roche and Berard in expanding and heating air,  $0^{\circ}.259$  was occupied in producing the mechanical effect, leaving  $0^{\circ}.741$  as that actually employed in raising the temperature of the air. Hence the actual specific heat (commonly called capacity at constant volume) is  $0.2669 \times 0.741 = 0.1977$ . Taking this as the specific heat of air and the equivalent 775, it follows that if a volume of air of 171.6 cubic inches be compressed to 170.6 cubic inches, it will be heated  $1^{\circ}$ , a quantity of heat which will occasion an increased pressure of  $\frac{1}{491}$ . So that the celerity of sound will be increased by this means in the subduplicate ratio of 491 to 661.6, or in the simple ratio of 2216 to 2572, which will bring it up from Newton's estimate of 943 to 1095 feet per ", which is as near 1130, the actual velocity at  $32^{\circ}$ , as could be expected from the nature of the experiments on the specific heat of air, and fully confirms the theory of Laplace.

Oak Field, near Manchester,  
July 17, 1847.

XXII. *On the Composition of Caffein, and of some of its Compounds.* By EDWARD CHAMBERS NICHOLSON, Esq.\*

CAFFEIN was first analysed by Professors Liebig and Pfaff† in 1832. The result of this investigation was confirmed by a subsequent analysis of Prof. Wöhler‡.

In 1838 Professor Liebig induced M. Jobst§ to analyse thein, who proved this body to be identical with caffein. His analyses gave the same results as his predecessors. The same remark applies to the experiments of Mulder|| on thein,

\* Communicated by the Chemical Society; having been read Feb. 15, 1847.

† Liebig's *Annalen*, i. 17.

‡ Ibid.

§ Ibid. xxv. 63.

|| *Bulletin des Sciences Phys. et Nat. de Neerlande*. 1838, p. 32.



and also to an analysis which M. Martius \* made of guaranin, a substance, the identity of which with caffein and thein had previously been pointed out by Berthemot and Dechastelust. Lately Dr. Stenhouse †, when examining Paraguay tea, has also made some analyses of thein.

The following table, in which I have recalculated these analyses according to the atomic weights, carbon 6 and hydrogen 1, allows a comparison to be made of the results obtained by these chemists.

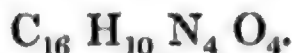
*Mean of the Analyses.*

	Caffein.		Thein.			Guaranin.
	Liebig & Pfaff.	Wöhler.	Mulder.	Jobst.	Stenhouse.	Martius.
Carbon .	49.30	49.25	49.18	49.47	48.95	49.23
Hydrogen	5.22	5.43	5.49	5.20	5.15	5.08
Nitrogen .	28.86		28.53	28.90		28.83

The most simple expression which can be deduced from these numbers is



Stenhouse's analysis however of the platinum compound proves that this formula must be doubled, and that the atom of caffein or thein is



The theoretical numbers of this formula are the following:—

16 eqs. Carbon . . .	96	49.48
10 ... Hydrogen . . .	10	5.15
4 ... Nitrogen . . .	56	28.86
4 ... Oxygen . . .	32	16.51
	<hr/> 194	<hr/> 100.00

From these numerous experiments the composition of caffein might have been considered as perfectly established. In a recent investigation of coffee, however, M. Payen§ states that he has obtained results which differ very sensibly from those obtained by his predecessors, and which he has translated into the formula



which contains 1 equiv. of oxygen less than the formula up to the present time admitted.

The theoretical numbers of Payen's formula are—

16 eqs. Carbon . . .	96	51.43
10 ... Hydrogen . . .	10	5.35
4 ... Nitrogen . . .	56	30.34
3 ... Oxygen . . .	24	12.88
	<hr/> 186	<hr/> 100.00

\* Liebig's *Annalen*, xxxvi. 93.

† Ibid. xxxvi. 90.

‡ Mem. Chem. Soc., vol. i. pp. 215, 237. [Phil. Mag., xxiii. p. 426.]

§ *Comptes Rendus de l'Académie*, tome xxiii. 8.

We observe here a difference of 2 per cent. of carbon, which M. Payen has obtained over the results of the above-mentioned chemists.

In order to elucidate this discrepancy, Dr. Hofmann induced me to make some experiments under his direction, partly with a quantity of beautiful caffein which he gave me\* and partly with a specimen which I have prepared myself.

### *Caffein.*

To ensure perfect purity of the substance it was crystallized three times from dilute alcohol, washed and dried. Thus purified, it formed very beautiful long white prisms, perfectly transparent when dried in the air, but which became opaque if exposed to a higher temperature. The crystals dried in the water-bath lost no weight when kept in an air-bath for four hours at a temperature of  $130^{\circ}$  C.

The specimen which I had prepared myself was obtained from Costa Rico coffee, by boiling the bruised fruit in water, precipitating the decoctions by basic acetate of lead and treating the filtrate with hydrosulphuric acid; after the whole of the lead had been removed, I evaporated the liquid to dryness in a water-bath, in order to get rid of acetic acid, and dissolved the residue in a small quantity of boiling water: upon cooling, the caffein crystallized out of a dark colour, and very impure. To purify it, it was washed and recrystallized three times from water, and finally from alcohol. It was then perfectly white, and had exactly the same appearance as the specimen which I obtained from Dr. Hofmann.

### *Analysis.*

I. 0.3827 grm. of substance, dried at  $100^{\circ}$  C. and burnt with chromate of lead, gave 0.6948 grm. of carbonic acid, and 0.1800 grm. of water.

II. 0.417 grm. of substance, burnt with chromate of lead and chlorate of potash, gave 0.7552 grm. of carbonic acid, and 0.1965 grm. of water.

III. 0.3934 grm. of substance of my own preparation gave 0.7123 grm. of carbonic acid and 0.1878 grm. of water, which calculated in 100 parts gives—

	I.	II.	III.
Carbon . . .	49.51	49.39	49.37
Hydrogen . .	5.22	5.23	5.30

\* I owe this specimen, of great beauty, to the well-known kindness of Mr. E. Merck of Darmstadt.—A. W. H.

which agrees with Professor Liebig's formula, as is seen by the following:—

		Theory.	Mean of experiments.
16 eqs. Carbon . . .	96	49·48	49·42
10 ... Hydrogen . .	10	5·15	5·28
4 ... Nitrogen . .	56	28·86	
4 ... Oxygen . .	32	16·51	
	<u>194</u>	<u>100·00</u>	

*Caffein and Bichloride of Platinum.*

On precipitating a solution of caffein in hydrochloric acid with bichloride of platinum, as Dr. Stenhouse has shown, a precipitate of an orange-yellow colour is obtained. If the two solutions are mixed hot, the fluid on cooling deposits the compound in beautiful granular crystalline tufts, which, when thrown on a filter and washed with alcohol, are perfectly pure. This double salt is only sparingly soluble in alcohol, æther, and water. It does not alter when exposed to light, nor does it lose in weight when kept at 100° C. for a considerable time.

The analyses of salts, all prepared at different periods and dried at 100° C., gave the following results:—

I. 0·5382 grm. of substance, burnt with chromate of lead, gave 0·4765 grm. of carbonic acid, and 0·1387 grm. of water.

II. 0·4881 grm. of substance gave 0·1196 grm. of platinum.

III. 0·4779 grm. of substance gave 0·1172 grm. of platinum.

IV. 0·6022 grm. of substance gave 0·1482 grm. of platinum.

V. 0·5781 grm. of substance gave 0·1425 grm. of platinum.

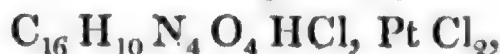
VI. 0·5246 grm. of substance gave 0·1293 grm. of platinum.

VII. 0·3847 grm. of substance made of caffein of my own preparation, gave 0·0945 grm. of platinum.

Which give the following per-centages:—

	I.	II.	III.	IV.	V.	VI.	VII.
Carbon .	23·80						
Hydrogen	2·86						
Platinum ...		24·51	24·52	24·60	24·64	24·64	24·56

leading exactly to the formula given by Dr. Stenhouse, viz.



as is seen when placed in comparison with the calculated numbers.

		Theory.	Mean of my experiments.	Dr. Stenhouse's mean.
16 eqs. Carbon .	96.0	23.97	23.80	24.22
11 ... Hydrogen .	11.0	2.74	2.86	2.89
4 ... Nitrogen .	56.0	13.98		
4 ... Oxygen .	32.0	8.02		
3 ... Chlorine .	106.5	26.59		
1 ... Platinum .	98.9	24.70	24.58	24.49
1 ... Caffein and bichloride of platinum }	400.4	100.00		

The analysis of caffein, as well as that of the platinum compounds, agree so perfectly with the numbers of Professor Liebig's formula, that there can be no doubt about its accuracy.

Assuming 1 equiv. of oxygen less in the equivalent of caffein, as is proposed by M. Payen, the platinum compound should contain not less than 24.46 per cent. of carbon and 25.12 of platinum. Now three determinations by Dr. Stenhouse, and six which I have made, never gave more than 24.64 per cent., that is, 0.6 per cent. less of platinum.

Not satisfied, however, with these proofs, I have tried to find some other compounds by which the atomic weight of caffein could be determined with equal accuracy.

In what follows a description of several new double salts of caffein will be given, the analyses of which correspond equally well with the original formula of this substance.

#### *Caffein and Nitrate of Silver.*

This compound is obtained when a solution of nitrate of silver is added in excess to an aqueous or alcoholic solution of caffein. If the solutions are concentrated it falls down in white hemispherical nodules, which adhere firmly to the side of the vessel.

When washed with water and crystallized from alcohol it is absolutely pure. This compound is indistinctly crystalline, of a perfectly white colour, and if dry undergoes no change when exposed to light, but if moist acquires a purplish hue. It is very soluble in hot water and alcohol, sparingly soluble in cold, and may be boiled in either solvent without undergoing decomposition. It loses no weight in the water-bath, but at a higher temperature it is decomposed, caffein sublimes, and metallic silver is left.

*Analysis.*—When burnt with chromate of lead—

I. 0.4514 grm. of substance gave 0.4345 grm. of carbonic acid, and 0.1162 grm. of water.



II. 0·2500 grm. of substance gave ·0744 grm. of silver.

III. 0·2716 grm. of substance gave ·0810 grm. of silver, which give the following per-centages:—

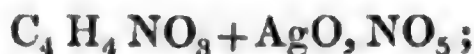
	I.	II.	II.
Carbon . . .	26·45		
Hydrogen . .	2·86		
Silver . . .	...	29·76	29·82,

and the formula— $C_{16}H_{10}N_4O_4 + AgO, NO_5$ , as may be seen by the following calculation:—

		Theory.	Found.
16 eqs. Carbon . . .	96	26·37	26·45
10 ... Hydrogen . .	10	2·74	2·86
5 ... Nitrogen . .	70	19·23	
10 ... Oxygen . .	80	22·00	
1 ... Silver . . .	108	29·66	29·79
	364	100·00	

The only analogues to this singular compound which I know are those of urea and nitrate of silver, analysed by Wether\*: the formulæ of which are— $C_2H_4N_2O_2 + AgO, NO_5$ , and  $C_2H_4N_2O_2 + 2(AgO, NO_5)$ .

These compounds, however, in consequence of the peculiar nature of urea, are not very stable, being decomposed when boiled with water into nitrate of ammonia and cyanate of silver. There likewise exists a compound of nitrate of silver and glycoll, lately described by Horsford†, having the formula



and, according to H. Rose, a compound of nitrate of silver with ammonia, 3 equivs. of this gas being absorbed by 1 equiv. of the former salt.

#### *Chloride of Mercury and Caffein.*

This beautiful compound is obtained when an aqueous or alcoholic solution of caffein is added to a solution of chloride of mercury; the latter being kept in excess, the fluid remains perfectly clear, but after the lapse of a few seconds solidifies into a mass of very small crystals, which when recrystallized from water or alcohol and washed on a filter, are quite pure.

When pure and crystallized from water it is very similar in appearance to caffein, the crystals not being however quite so large. It is very soluble in alcohol and water, hydrochloric, nitric, and oxalic acids, and seems to form with the latter a crystalline compound. It is nearly insoluble in æther. In reference to its constitution, it is distinguished from the dou-

\* Liebig's *Annalen*, lvi. 262.

† Ibid. lx. 36.

ble salt of platinum, for in this instance the caffein is in direct combination with the chloride of mercury, and is exactly analogous to the corresponding compounds of leucoline and aniline investigated by Dr. Hofmann\*. The mercurial compounds of this kind are generally easily decomposed, but the compound of chloride of mercury and caffein is so stable, that it may be boiled in water for a considerable time without undergoing the slightest change in its properties. It may be dried at 100° C. and loses no weight at that temperature.

I endeavoured to combine the determination of the carbon, hydrogen and mercury of this substance in *one* combustion, and have perfectly succeeded. The operation was conducted as follows:—The substance was mixed with chromate of lead and introduced into a combustion-tube of at least 26 inches in length. About 6 inches of copper turnings are placed above the mixture, leaving a space of 8 inches from the copper to the anterior end of the tube. A receptacle for the mercury is formed out of the tube itself by contracting it about an inch from the copper turnings, and again so as to leave an elongated bulb of an inch in length. At the close of the operation the tube is cut with a file at the posterior contraction. In order to separate the water from the mercury, the chloride of calcium tube (which has not been detached) is connected with an aspirator and air admitted through chloride of calcium, the bulb being kept at a temperature of 100° C.

I obtained in my analysis the following numbers:—0.7833 gm. of substance gave 0.5832 gm. of carbonic acid, 0.1639 gm. of water, and 0.3365 gm. of mercury, corresponding to the following per-centage, which I place in comparison with the theoretical numbers:—

		Theory.	Expt.
16 eqs. Carbon	. . . . . 96	20.68	20.30
10 ... Hydrogen	. . . . . 10	2.15	2.32
4 ... Nitrogen	. . . . . 56	12.11	
4 ... Oxygen	. . . . . 32	6.89	
2 ... Chlorine	. . . . . 70	15.08	
2 ... Mercury	. . . . . 200	43.09	42.91
	464	100.00	

#### *Caffein and Terchloride of Gold.*

This compound is formed when a solution of terchloride of gold is added in excess to caffein dissolved in dilute hydrochloric acid. If concentrated solutions are employed, the whole immediately solidifies into a mass of a most splendid lemon-yellow colour; this is to be washed with cold water

\* Liebig's *Annalen*, xlvii. 37.

and crystallized from alcohol, and finally dried in the water-bath.

The crystals from an alcoholic solution are in the form of long needles, of an orange-yellow colour and a very bitter metallic taste; they are soluble in alcohol and water. When boiled in water for a short time, the salt is decomposed, a yellow flocculent matter precipitating, which is insoluble in alcohol, ether and water, but soluble in hydrochloric acid. If an aqueous solution is kept on the sand-bath for some hours at a temperature of about  $68^{\circ}\text{C}$ . it is also decomposed, and metallic gold separates in shining scales.

It is not altered when exposed to light, and when dry may be heated to  $100^{\circ}\text{C}$ . without undergoing decomposition.

*Analysis.*—When burnt with chromate of lead—

I. 0.8530 grm. of substance gave 0.5525 grm. of carbonic acid and 0.1622 grm. of water.

II. 0.3224 grm. of substance gave 0.1197 grm. of metallic gold.

III. 0.3019 grm. of substance gave 0.1115 grm. of metallic gold, which give the following per-centages:—

	I.	II.	III.
Carbon . .	17.72		
Hydrogen . .	2.11		
Gold . . .		37.12	36.93

corresponding to the formula  $\text{C}_{16}\text{H}_{10}\text{N}_4\text{O}_4\text{HClAuCl}_3$ , as may be seen by the following table, where the calculated and quantities found are placed in comparison:—

		Theory.	Found.
16 eqs. Carbon .	96.00	17.98	17.72
11 ... Hydrogen .	11.00	2.06	2.11
4 ... Nitrogen .	56.00	10.50	
4 ... Oxygen .	32.00	6.01	
4 ... Chlorine .	142.00	26.60	
1 ... Gold . .	196.66	36.85	37.02
	<u>533.66</u>	<u>100.00</u>	

The caffein compounds which I have analysed are therefore—

Caffein . . . . .	$\text{C}_{16}\text{H}_{10}\text{N}_4\text{O}_4$ .
Platinum compound	$\text{C}_{16}\text{H}_{10}\text{N}_4\text{O}_4\text{HClPtCl}_3$ .
Silver compound .	$\text{C}_{16}\text{H}_{10}\text{N}_4\text{O}_4\text{AgO}, \text{NO}_5$ .
Mercury compound	$\text{C}_{16}\text{H}_{10}\text{N}_4\text{O}_4, 2(\text{HgCl})$ .
Gold compound . .	$\text{C}_{16}\text{H}_{10}\text{N}_4\text{O}_4\text{HCl}, \text{AuCl}_3$ .

There exist several other double compounds of caffein, which I have however not subjected to analysis.

On mixing a hot alcoholic solution of caffein with an alcoholic solution of cyanide of mercury, beautiful needles of a double salt are deposited upon cooling, which correspond most likely to the mercury salt I have just described. A solution of caffein in hydrochloric acid gives a beautiful brown precipitate with chloride of palladium; and the filtered solution deposits another compound in the form of yellow scales, very similar in appearance to iodide of lead.

Caffein gives no precipitate with solutions of sulphate of copper, chloride of tin, acetate of lead, and nitrate of suboxide of mercury. When boiled with sesquichloride of iron, a reddish-brown precipitate subsides upon cooling, which is perfectly soluble in water, and is most likely a double compound of caffein and sesquichloride of iron.

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XXIII. *Note in reference to the extension of Euler's Theorem.*  
*By J. R. YOUNG, Professor of Mathematics in Belfast College.*

*To Richard Taylor, Esq.*

DEAR SIR,

IN the Philosophical Magazine for June last a communication of mine was published respecting an extension of a certain theorem of Euler concerning the products of the sums of squares. At the time that notice was written, I was under the impression that the theorem admitted of an extent of generalization which a further investigation of the matter proves to me has not place. I am now prepared to show that the proposition does not hold beyond the case for eight squares, the formulæ for which I have already printed in the Proceedings of the Royal Irish Academy; in the Transactions of which body it is probable that the entire investigation of the theorem for eight squares, and the proof that it does not apply beyond that number, will hereafter appear.

It may perhaps be interesting to algebraists to find the real limits to this theorem demonstrably established; and thus to know—in any attempts that may hereafter be made to extend Sir W. R. Hamilton's remarkable and very fertile theory of quaternions—beyond what boundaries such attempts must prove fruitless.

I remain, dear Sir,

Very faithfully yours,

Belfast, July 16, 1847.

J. R. YOUNG.



XXIV. *On the Precipitate produced in Spring and River Waters by Acetate of Lead.* By A. CONNELL, Esq., Professor of Chemistry in the University of St. Andrews\*.

THE white precipitate which it is well known is usually produced in spring and river waters by acetate of lead, has been commonly attributed to the presence of sulphates, chlorides and carbonates. The comparatively trifling action of silver salts, however, shows that it is very rarely, unless in the case of what are called mineral waters, due to chlorides; and the ready solubility of the precipitate in acetic acid in whole or in great part, proves that it is not due to sulphates or phosphates, except in so far as it may be insoluble in acetic acid. Carbonates therefore remain as the probable cause; and this is established by the circumstance, that although effervescence cannot be noticed on the immediate addition of acetic acid, effervescence will be observed if the precipitate is allowed to subside, and the greater part of the solution decanted, and an acid then added. I have found on investigation that carbonate of lime is the usual source of the reaction. The remarkable fact however on this view is, that the reaction is scarcely diminished by boiling and filtering the water; and indeed in some instances does not take place unless these steps are had recourse to, and acetic acid still dissolves the whole or great part. If the waters referred to are boiled and filtered and then largely concentrated by evaporation, they usually deposit carbonate of lime, and do not indicate any such alkaline reaction as shows an alkaline carbonate. The carbonate of lime causing the reaction is therefore evidently held dissolved in the water independently of the presence of free carbonic acid; and I do not think that chemists, generally speaking, are aware that common water may still retain enough of carbonate of lime to give, with acetate of lead, a considerable precipitate of carbonate of lead, although they may have been boiled and filtered. If in any such case the precipitate should be found to dissolve in acetic acid truly *without* effervescence, the probable cause would be the presence of a sufficient quantity of some organic matter, such as crenic or apocrenic acid, which precipitates lead salts; for it is not the least likely that fluorine, which has been found in some spring waters, should ever be present in sufficient quantity to affect lead salts, and fluoride of lead would very likely not be soluble in acetic acid.

The question then arises, whence proceeds this carbonate

\* Communicated by the Author.

of lime. To know whether it arises from the water redissolving carbonate of lime, which had been held dissolved by carbonic acid and then precipitated by boiling, I transmitted a current of carbonic acid through lime water till it completely redissolved the precipitate which had at first formed. I then boiled the solution for a short time, as in experimenting with the spring waters, and filtered the liquid; but although it was slightly precipitated by acetate of lead, the effect was very much less than that on common water; showing that we cannot account for the effect on common water by supposing that all the carbonic acid had not been driven off by the ebullition. Again, when distilled water was left in contact with marble in impalpable powder for several days, both acetate of lead and oxalate of ammonia showed less lime than in the common waters, although rather more than in the lime-water experiment. I incline therefore to think that the carbonate of lime owes its origin to double decomposition between an alkaline carbonate and a lime salt, such as a chloride. If to a few ounces of distilled water a drop or two of muriate of lime and a drop or two of carbonate of soda be added, the liquid remains quite transparent; and the reaction of common water with acetate of lead and acetic acid may be exactly imitated with this liquid. And in all the common waters yielding the reaction, I could detect alkalies in union with acids.

The common water of the town of St. Andrews, I found, after being boiled and filtered, to yield by evaporation  $\frac{1}{31323}$  of carbonate of lime; and other well and river waters may contain still more. Fresenius has stated that water is capable of holding in solution  $\frac{1}{100001}$  of carbonate of lime, after being saturated with that salt by long-continued boiling, and left in contact for four weeks with the deposit formed on cooling. Nature of course does not take such pains to charge spring waters with lime; and I think the method I have suggested affords a much more simple and probable means of effecting this end.

The St. Andrews' water also contains a trace of carbonate of magnesia after being boiled and filtered; and it is probable that this substance may sometimes be in part the cause of the reaction referred to, but to a much less extent\*.

\* I have given fuller details on this subject in a paper inserted in the Transactions of the Royal Society of Edinburgh for the present year.

XXV. *On the Action of a mixture of Red Prussiate of Potash and Caustic Alkali upon Colouring Matters.* By JOHN MERCER, Esq.\*

ABOUT ten years since I discovered and used extensively in calico-printing the oxidizing properties of a mixture of red prussiate of potash and caustic alkali. For many years I have been in the habit of communicating to my friends several applications of this interesting reaction, among whom I may mention Mr. Crum of Glasgow and Dr. Lyon Playfair. Since then Boudault † has directed attention to the oxidizing power of the same mixture, as far as relates to metallic oxides, but has not shown any important practical application of the knowledge thus acquired.

There are but few processes known in the arts for bleaching indigo, the principal of these being that in which chromic acid liberated from the bichromate of potash by means of an acid is used. In certain cases this process is attended with various disadvantages, and the cloth requires to be subjected to a clearing process to remove the oxide of chromium. The topical application of a mixture of red prussiate of potash and an alkali at once effects the same purpose, and in a most complete manner, leaving a brilliant white on the spot where the colour is discharged without rendering any injury to the fabric. The manner of applying this discharge may be arranged to suit the conditions of the calico-printer. As a class experiment for a lecture-table it is convenient to impregnate the indigo-blue calico with a solution of prussiate of potash, and then dip it into a weak solution of alkali.

This action is a beautiful illustration of those double affinities which we frequently find at play in combinations or decompositions. Thus, though neither chlorine nor charcoal can decompose alumina *per se*, the same gas passed over a mixture of alumina and charcoal combines with the metallic radical; the charcoal in this case having aided the combination by withdrawing the oxygen. It is the same kind of action in the case under consideration. Red prussiate of potash,  $\text{Fe}_2\text{Cy}_6\text{3K}$ , differs from the yellow prussiate,  $\text{Fe}_2\text{Cy}_6\text{4K}$ , by containing one atom less potassium. When potash is presented to the former, this deficient atom of potassium is supplied, but the affinity is not strong enough to liberate the oxygen. When however a second body having an attraction for oxygen, such as litharge or indigo, is presented to the potash and red prussiate, this second affinity

\* Communicated by the Chemical Society; having been read Feb. 1, 1847.

† *Journal de Pharmacie*. [Phil. Mag., vol. xxvii. p. 307.]

acting in a different direction withdraws the oxygen and allows the potassium to unite with the compound radical ferrocyanogen; thus  $\text{Fe}_2 \text{Cy}_6 3\text{K} + \text{KO} + \text{PbO} = \text{Fe}_2 \text{Cy}_6 4\text{K} + \text{PbO}_2$ , the decomposition being of the same kind when an organic matter is substituted for the oxide capable of further oxidation. Soda and ammonia may be substituted for potash in the above decomposition, producing the oxidation or discharging the indigo. This is curious in the case of ammonia, for it cannot be explained by any other than by the ammonium theory, and shows the complete analogy between the oxide of ammonium and the oxide of the simple metallic radicals, potassium and sodium. It is interesting also to observe that the last member in the formula  $\text{Fe}_2 \text{Cy}_6 4\text{R}$ , may be substituted by any alkaline base. Thus, that it may either be  $\text{Fe}_2 \text{Cy}_6 3\text{K K}$ , or  $\text{Fe}_2 \text{Cy}_6 3\text{K Na}$ , or  $\text{Fe}_2 \text{Cy}_6 3\text{K NH}_4$ . This circumstance points to important theoretical considerations in the atomic constitution of the prussiates, which would be foreign to the present paper, the principal object of which is to furnish a means of discharging indigo, and thus supply a process much wanted in the art of calico-printing, and which I have followed for many years with success.

XXVI. On the Preparation of Hippuric Acid.

By WILLIAM GREGORY, M.D.\*

SINCE the discovery of hippuric acid by Liebig, that body has at all times attracted much attention. Its composition and the products of its decomposition, among which were benzoic acid and benzamide, rendered it interesting, and various ingenious views were entertained of its constitution. Its detection in human urine by Liebig gave it additional importance.

The beautiful discovery of Dessaignes, that hippuric acid, when heated with strong acids, is resolved into benzoic acid and glycocoll, has greatly increased the interest already attached to hippuric acid, which now affords the best means of obtaining glycocoll, and has enabled Horsford, in his elaborate researches on that substance, to fix its formula in a very satisfactory manner.

If to hydrated hippuric acid . . . . .	$\text{C}_{18} \text{N H}_9 \text{O}_6$ ,
we add 1 equiv. water . . . . .	$\text{H O}$ ,
and from the sum . . . . .	$\text{C}_{18} \text{N H}_{10} \text{O}_7$ ,
subtract 1 equiv. glycocoll . . . . .	$\text{C}_4 \text{N H}_4 \text{O}_3$ ,
there remain . . . . .	$\text{C}_{14} \text{H}_6 \text{O}_4$ ,

which is hydrated benzoic acid.

\* Communicated by the Chemical Society; having been read March 15, 1847.



There cannot, I think, be any longer a doubt that  $C_4 N H_4 O_3$  is the true formula of glycoll, and Horsford has, in establishing this point, at the same time confirmed and explained in the most satisfactory manner the observation of Dessaignes.

The researches of Horsford, however, have also demonstrated that glycoll is in itself one of the most interesting compounds known to chemists, and it is evident that the further study of this most singular body will lead to very valuable results.

I have already stated that glycoll is best obtained from hippuric acid, but as soon as I began to prepare for this purpose a considerable quantity of hippuric acid, I found, as all who have done so must have found, that the operation as prescribed in books is not only tedious and troublesome, but uncertain.

The usual process consists in evaporating the urine of the horse or cow at a moderate temperature to about one-eighth of its bulk, and adding hydrochloric acid, when on standing a few hours, crystals of impure hippuric acid are deposited. But it is well-known that if the temperature should rise too high, although still to a point short of boiling, the hippuric acid will partially or totally disappear, and benzoic acid will be found in its place. Now when we bear in mind that the urine contains but little hippuric acid, it is evident that to obtain this acid in quantity we must operate with a very large bulk of urine, and those who have done so well know how tedious the evaporation is, since if we attempt to hasten it by raising the temperature, we run the risk of losing the whole; and this indeed frequently happens.

The impure, highly-coloured acid first obtained has been purified by different chemists in a great variety of ways. Some have used chloride of lime; but this method is not easily managed, and often converts the whole into benzoic acid.

The last and by far the best method of purification is that of Schwarz, who boils the impure acid with an excess of milk of lime, and strains the alkaline liquid from the undissolved lime. It passes rapidly and clear through calico, and the lime retains the colouring matter, so that the addition of acid to the filtered liquid causes the deposition of crystals of hippuric acid nearly white. Schwarz recommends the addition of chloride of calcium to the filtered or unfiltered liquid, and the precipitation of the lime as carbonate by carbonate of potash or soda, when the precipitated carbonate of lime carries with it the last traces of colouring matter. I have not found this necessary, as a repetition of the process with the milk of lime never fails to yield colourless crystals.

As it was clear that the hippuric acid was not in the slightest degree decomposed by boiling with excess of lime, although so easily metamorphosed by acids, I thought that by applying the same principle to the urine directly, I might be enabled to *boil it down*, and thus shorten the process, and at the same time prevent the decomposition of the hippuric acid, since it would appear that hippurate of lime is not affected by boiling, nor by excess of lime.

Accordingly, I took some urine of the horse, mixed it with excess of milk of lime and boiled for a few minutes. I then strained the solution, which was very materially decolorized, and boiled the clear liquid as rapidly as possible down to the requisite bulk. On adding hydrochloric acid I obtained a copious deposit of crystals, which when pressed had a slight red colour. I then treated them by Schwarz's method and obtained an abundant crop of almost colourless crystals, which consisted entirely of the needles of hippuric acid, without a visible trace of benzoic acid, the crystallization of which is easily recognized. A second treatment with milk of lime, which was hardly needed, and probably would have been quite unnecessary had a greater excess of lime been used in the previous one, yielded snow-white crystals of the utmost beauty and purity.

The improvement which I have thus introduced in the preparation of hippuric acid may seem trifling, and is indeed only the application of Schwarz's method to the urine, instead of to the crude acid; but any one who tries to prepare some ounces, not to say pounds, of hippuric acid, will soon find that the difference is practically important. By my method it is possible to extract in one day the hippuric acid from as much urine as would require a week to operate upon on the usual plan, so that the quantity of hippuric acid which we can thus obtain is only limited, as it were, by the quantity of urine to be procured. The tedious evaporation at low temperatures is got rid of, and we are sure of obtaining the whole hippuric acid originally present; whereas, on the former plan, however carefully the evaporation is conducted, and it requires constant superintendence, it almost always happens that some of the hippuric acid is decomposed; while a very slight accidental rise of temperature may destroy the whole of it, as I have often seen.

On the whole, I am satisfied that all who wish to study hippuric acid and glycocoll will find on trial that what was formerly a disagreeable and troublesome operation is now a very easy and short one; and that they may now easily obtain these remarkable compounds in any desired quantity.

*Phil. Mag. S. 3. Vol. 31. No. 206. Aug. 1847.* K

XXVII. *Proceedings of Learned Societies.*

## CAMBRIDGE PHILOSOPHICAL SOCIETY.

[Continued from vol. xxx. p. 367.]

Nov. 9, 1846. **O**N the Structure of the Syllogism, and on the application of the Theory of Probabilities to Questions of Argument and Authority\*. By Professor De Morgan.

The object of this paper is twofold : first, to establish two distinct theories of the syllogism, both differing materially from that of Aristotle, and each furnishing a general canon for the detection of all its legitimate forms of inference ; secondly, to investigate the mode in which the distinctive character of the two great sources of conviction, *argument* and *authority*, affects the application of the notion of probability to questions not admitting of absolute demonstration.

The two theories of the syllogism arise out of simple notions connected with the *forms* of propositions and their *quantities*. The difference between a positive and negative assertion is not essential, but depends on the manner in which objects of thought are described by language. If *Y* and *y* be names so connected that each contains everything which is not in the other, and the two have nothing in common (a relation which is described by calling them *contrary*

\* Upon this paper a controversy has arisen, which, up to the present time, may be summed up as follows :—*April 30.* Mr. De Morgan published a statement in answer to an assertion of Sir W. Hamilton of Edinburgh, to the effect that the second, or quantitative, system of syllogism, was a wilful plagiarism from certain letters which Sir W. Hamilton had written to Mr. De Morgan. *May 22.* Sir W. Hamilton replied at length in another pamphlet, retracting the assertion of *wilful* plagiarism, but maintaining that the system was taken, unconsciously, from those letters. This was followed by a letter from Mr. De Morgan in the *Athenæum* of *May 29*, and another from Sir W. Hamilton in the same publication for *June 5*. The point at issue now seems to be as follows :—Mr. De Morgan challenges Sir W. Hamilton to show anything in his second system which was not substantially contained in a digressive section of the description of his first system, admitted to have been sent to Cambridge before any communication had taken place. Sir W. Hamilton, in reply, contends that the digression above-mentioned contains nothing to the purpose. Mr. De Morgan defers further reply until he publishes a work which he states himself to be preparing on logic.

In the *Athenæum* of *June 19*, appeared a letter from Mr. James Broun, asserting certain mistakes on the part both of Sir W. Hamilton and Mr. De Morgan, and giving certain extensions to the quantitative forms of the latter. Again, *June 26*, appeared in the same publication a letter from Mr. De Morgan, dated *June 19*, stating that he also had arrived at Mr. Broun's forms, giving reasons for their rejection in favour of certain simpler forms, giving the heads of an extended system of quantitative syllogism, and asserting that he had materially extended both his systems. So the matter stands. The subject of the structure of the syllogism seems to be likely to excite some attention ; and, without pronouncing any opinion on the personal claims or conflicts of the several parties, we recommend the attention of our readers to this rather neglected branch of pure science.—  
ED. PHIL. MAG.

names), the propositions 'Every X is Y' and 'no X is y' are simply identical. In the same manner, the particular and universal proposition are only accidentally distinct. If in 'some Xs are Ys' the Xs there specified had had a name belonging to them only, say Z, then the preceding proposition would have been identical in meaning with 'every Z is Y.'

From the above it is made to follow, that every legitimate syllogism can be reduced to one of universal affirmative premises, either by introduction of contrary terms, or invention of subgeneric names.

In considering the nature of the simple proposition, Mr. De Morgan uses a notation proposed by himself. Thus—

Every X is Y	is denoted by	X)Y	A
No X is Y	.. ..	X.Y	E
Some Xs are Ys	.. .	XY	I
Some Xs are not Ys	..	X:Y	O

and names which are contraries are denoted by large and small letters. Aristotle having excluded the contrary of a name from formal logic, and having thereby reduced the forms of proposition to four, these forms (*universal affirmative, universal negative, particular affirmative, particular negative*) the writers on logic in the middle ages represented by the letters A, E, I, O. Thus X)Y and Y)X are equally represented by A. When contraries are expressly introduced, all the forms of assertion or denial which can obtain between two terms and their contraries, are *eight* in number; and the most convenient mode of representing them is as follows:—Let the letters A, E, I, O have the above meaning, but only when the order of subject and predicate is XY. Then let *a, e, i, o* stand for the same propositions, after *x* and *y*, the contraries, are written for X and Y. The complete system then is—

A=X)Y	a=x)y=Y)X
O=X:Y	o=x:y=Y:X
E=X.Y	e=x.y
I=XY	i=xy

and every form in which subject and predicate are in any manner chosen out of the four X, Y, *x*, *y*, so that one shall be either X or *x*, and the other either Y or *y*, is reducible to one or other of the preceding.

The propositions *e* and *i*, which are thus newly introduced, are only expressible as follows, with reference to X and Y.

(i.) *There are things which are neither X nor Y.*

(e.) *There is nothing but is either X or Y or both.*

The connexion of these eight forms is fully considered, and the various syllogisms to which they lead. Rejecting every form of syllogism in which as strong a conclusion can be deduced from a weaker premise; rejecting, for instance,

$$Y)X + Y)Z = XZ$$

because XZ equally follows from Y)X + YZ, in which YZ is *weaker* than Y)Z—all the forms of inference are reduced to three sets.

1. A set of two, called *single* because the interchange of the terms



of the conclusion does not alter the syllogism. Neither of these forms are in the Aristotelian list. One of them is

$$X)Y + Z)Y = xz;$$

or if every *X* be a *Y*, and also every *Z*, then there are things which are neither *X* nor *Z*; namely, all which are not *Y*s.

2. A set of six, in which the interchange produces really different syllogisms of the same form, and in which both premises and conclusion can be expressed in terms of *three names*, without the contrary of either. This set includes the whole Aristotelian list, except those in which a weaker premise will give as strong a conclusion, or the one in which the same premises will give a stronger conclusion.

3. A set of six resembling the last in everything but this, that no one of them is expressible without the new forms *e* and *i*; that is, requiring three names and the contraries of one or more of them.

Those of the third set are not reducible to Aristotelian syllogisms, as long as the eight standard forms of assertion are adhered to.

The second theory of the syllogism has its principles laid down in the memoir before us; but those principles are only applied to the evolution of the cases which are not admitted into the Aristotelian system. The formal statement of the manner in which the ordinary cases of syllogism are connected with those peculiar to this second system is contained in an *Addition*.

In providing that premises shall certainly furnish a conclusion, the common system requires that one at least of the premises shall speak *universally* of the middle term; that is, shall make its assertion or denial of *every* object of thought which is named by the middle term. Mr. De Morgan points out that this is not necessary: *m* being the fraction of all the cases of the middle term mentioned in one premise, and *n* in the other, all that is necessary is that *m* + *n* should be greater than unity. In such case, the real middle term, being the collection of all the cases by comparison of which with other things inference arises, is the fraction *m* + *n* - 1 of all the possible cases of the middle term. Thus, from the premises 'most *Y*s are *X*s' and 'most *Y*s are *Z*s,' it can be inferred that some *X*s are *Z*s, since *m* and *n* are both greater than one-half. The assignment of definite quantity to the middle term in both premises, gives a canon of inference, of which the Aristotelian rule is only a particular case.

In the addition above alluded to, this same canon, namely 'that more *Y*s in number than there exist separate *Y*s shall be spoken of in both premises together,' is made to take the following form:—If in an affirmation or negation, in '*As* are *Bs*' and '*As* are not *Bs*,' definite numerical quantity be given to both subject and predicate, if it be stated how many *As* are spoken of and how many *Bs*—the number of *effective* cases of the middle term is seen to be the number of *subjects* in an affirmative proposition, whether the middle term be subject or predicate. Hence, defining the effective number of a premise to be the number of subjects if the proposition be affirmative, and the number of cases of the middle term if it be negative, all that is necessary for inference (over and above the usual condition

that both premises must not be negative) is that the sum of the effective numbers of the two premises shall exceed the number of existing cases of the middle term ; and the excess (being the fraction denoted by  $m+n-1$  in the Memoir) gives the number of cases in which inference can be made.

To attempt to combine these two systems of *form* and of *quantity* is rendered useless by language not possessing the forms of mixed assertion and denial, which the syllogisms deduced from the combination would require. As far as the combination can, in Mr. De Morgan's opinion, be made, nothing is required but a distinct conception of, and nomenclature for, the usual modes of expressing a logical form, and implying one or the other of the alternations which the mere expression leaves unsettled. Mr. De Morgan proposes the following language.

Two names are *identical* when each contains all that the other contains : but when all the first (and more) is contained in the second, then the first is called a *subidentical* of the second, and the second a *superidentical* of the first. Two names are *contrary* when everything (or everything intended to be spoken of) is in one or the other and nothing in both. But when the two names have nothing in common, and do not between them contain everything, they are called *subcontraries* of one another. And again, if everything be in one or the other, and some things in both, they are called *supercontraries* of one another. Lastly, if the two names have each something in common and something not in common, and moreover do not between them contain everything, each is called a *complete particular* of the other. A table is then given, which contains every form of complex syllogism.

If  $X$  and  $Z$  be the terms of the conclusion, and both be described in terms of  $Y$ , the middle term : it can be seen from this table what can be affirmed and what denied, of  $X$  with respect to  $Z$ . For instance, if  $X$  be supercontrary of  $Y$ , and  $Z$  subcontrary, then  $X$  must be a superidentical of  $Z$  : but if  $X$  and  $Z$  be both subidenticals of  $Y$ , nothing can be affirmed ; only it may be denied that  $X$  is either contrary or supercontrary of  $Z$ .

The remaining part of this paper relates to the application of the theory of probabilities above-mentioned. Mr. De Morgan asserts that no conclusion of a definite amount of probability can be formed from argument alone ; but that all the results of argument must be modified by the testimony to the conclusion which exists in the mind, whether derived from the authority of others, or from the previous state of the mind itself. The foundation of this assertion is the circumstance that the insufficiency of the argument is no index of the falsehood of the conclusion. Various cases are examined ; but it must here be sufficient to cite one or two results.

If  $\mu$  be the probability which the mind attaches to a certain conclusion,  $a$  the probability that a certain argument is valid, and  $b$  the probability that a certain argument for the contradiction is valid : then the probability of the truth of the conclusion is

$$\frac{(1-b)\mu}{(1-b)\mu + (1-a)(1-\mu)}.$$

If  $b=0$ , or if there be no argument against, and if the mind be unbiassed, or if  $\mu = \frac{1}{2}$ , this becomes

$$\frac{1}{2-a} \text{ or } a + \frac{(1-a)^2}{2-a}.$$

For this writers on logic generally substitute  $a$ , confounding the absolute truth of the conclusion with the validity of the argument, and neglecting the possible case of the argument being invalid, and yet the conclusion true.

Nov. 23.—On a New Notation for expressing various Conditions and Equations in Geometry, Mechanics and Astronomy. By the Rev. M. O'Brien.

If  $A, P, P'$  be any three points in space, whether in the same straight line or not, and if the lines  $AP$  and  $AP'$  be represented in magnitude and direction by the symbols  $u$  and  $u'$ , then, according to principles now well-known and universally admitted, the line  $PP'$  is represented in magnitude and direction by the symbol  $u' - u$ . Now if  $AP$  and  $AP'$  be equal in magnitude, and make an indefinitely small angle with each other,  $PP'$  is an indefinitely small line at right angles to  $AP$ , and  $u' - u$  becomes  $du$ . Hence it follows, that, if  $u$  be the symbol of a line of invariable magnitude,  $du$  is the symbol of an indefinitely small line at right angles to it; and therefore, if  $\lambda$  be any arbitrary coefficient,  $\lambda du$  is the general expression for a right line perpendicular to  $u$ .

The sign  $\lambda d$  therefore indicates perpendicularity, when put before the symbol of a line of invariable length. The object of the author is to developpe this idea, and to show that it not only leads to a simple method of expressing perpendicularity, but also furnishes a notation of considerable use in expressing various conditions and equations in geometry, mechanics, astronomy, and other sciences involving the consideration of *direction* and *magnitude*.

The author first reduces the sign  $\lambda d$  to a more convenient form, which not only secures the condition that  $u$  is invariable in length, but also defines the magnitude and direction of the perpendicular which  $\lambda du$  denotes. This he does in the following manner. He assumes

$$u = x\alpha + y\beta + z\gamma,$$

(where  $\alpha \beta \gamma$  represent three lines, each a unit in length, drawn at right angles to each other, and  $x y z$  are any arbitrary numerical coefficients,) and supposes that the differentiation denoted by  $d$  affects  $\alpha \beta \gamma$ , but not  $x y z$ . This secures the condition that  $u$  is invariable in length, and leads to the following expression for  $\lambda du$ , viz.

$$\lambda du = (zy' - z'y)\alpha + (xz' - x'z)\beta + (yx' - y'x)\gamma,$$

$x' y' z'$  being arbitrary coefficients.

Assuming  $u' = x'\alpha + y'\beta + z'\gamma$ , it appears from this expression for  $\lambda du$ , that  $du=0$  when  $u=u'$ , and therefore that  $d$  denotes a differential taken on the supposition that  $u'$  is constant.

On this account the author substitutes the symbol  $D_u$  in place of  $\lambda d$ ; he then shows that the operation  $D_u$  is *distributive* with respect to  $u'$  (i. e. that  $D_{u'+u''} = D_{u'} + D_{u''}$ ), and to indicate this he elevates

the subscript index  $u'$ , and writes  $Du'.u$  instead of  $D_u u$ . Thus he obtains the expression

$$Du'.u = (zy' - z'y)\alpha + (xz' - x'z)\beta + (yx' - y'x)\gamma.$$

From this it follows that  $Du'.u$  is a line perpendicular both to  $u'$  and  $u$ , and that the numerical magnitude of  $Du'.u$  is  $rr' \sin \theta$ , where  $r$  and  $r'$  are the numerical magnitudes of  $u$  and  $u'$ , and  $\theta$  the angle made by  $u$  and  $u'$ .

Having investigated the principal properties of the operation  $Du'$ , the author, by a similar method, obtains another notation,  $\Delta u'.u$ , which represents the expression  $xx' + yy' + zz'$ , or  $rr' \cos \theta$ . He then gives some instances of the application of these two notations to mechanics, which may be briefly stated as follows:—

1st. If  $U, U', U'', \&c.$  be the symbols\* of any forces acting upon a rigid body, and  $u, u', u'', \&c.$  the symbols† of their respective points of application, then the six equations of equilibrium are included in the two equations

$$\Sigma U = 0 \text{ and } \Sigma Du.U = 0.$$

2nd. That these two equations are the necessary and sufficient conditions of equilibrium, may be proved very simply from first principles by the use of the notation  $Du$ .

3rd. The theory of couples is included in the equation  $\Sigma Du.U = 0$ . In fact the symbol  $Du.U$  expresses, in magnitude and direction, the axis of the couple by which the force  $U$  is transferred from its point of application  $U$  to the origin.

4th. Supposing that the forces  $U, U', U'', \&c.$  do not balance each other, and putting  $\Sigma U = V$ ,  $\Sigma Du.U = W$ , we may show immediately, by the use of the notation  $\Delta u$ , that the condition of there being a single resultant is

$$\Delta V.W = 0;$$

and when there is not a single resultant, the axis of the couple of minimum moment is

$$\frac{\Delta V.W}{\Delta V.V}.V.$$

5th. The three equations of motion of a rigid body about its centre of gravity are included in the equation

$$\frac{d}{dt} \left( \Sigma Du. \frac{du}{dt} \delta m \right) = \Sigma Du.U \delta m; \dots \dots (1.)$$

$u$  being the symbol of the position of any particle  $\delta m$  of the body, and  $U$  the symbol of the accelerating force acting on  $\delta m$ .

6th. If  $\omega$  be assumed to represent the expression  $\omega_1 \alpha + \omega_2 \beta + \omega_3 \gamma$ , where  $\omega_1, \omega_2, \omega_3$  are the angular velocities of the planes of  $yz, zx, xy$  about the axes of  $x, y, z$  respectively, then the symbol of the

\* By the symbol of a force is meant the expression  $X\alpha + Y\beta + Z\gamma$ , where  $X Y Z$  are the three components of the force.

† By the symbol of a point is meant the expression  $x\alpha + y\beta + z\gamma$ , where  $x y z$  are the coordinates of the point.



velocity of  $\delta m$  is  $Dw.u$ ; from which follow immediately the three well-known equations,

$$\frac{dx}{dt} = \omega_2 z - \omega_3 y, \quad \frac{dy}{dt} = \omega_3 x - \omega_1 z, \quad \frac{dz}{dt} = \omega_1 y - \omega_2 x.$$

The symbol  $\omega$  represents in direction the axis of instantaneous rotation, and in magnitude the angular velocity about that axis.

7th. The equation (1.) may be reduced to the form

$$\frac{d}{dt} \{ A\omega_1\alpha + B\omega_2\beta + C\omega_3\gamma \} = \Sigma Du.U\delta m,$$

which includes Euler's three equations of motion about a fixed point.

8th. If the forces  $U, U', U'', \&c.$  arise from the attraction of a distant body, the symbol of whose position is  $u'$ , this equation may be further reduced to the form

$$\frac{d}{dt} (A\omega_1\alpha + B\omega_2\beta + C\omega_3\gamma) = \frac{3m'}{r'^3} Du'.(Ax'\alpha + By'\beta + Cz'\gamma).$$

9th. In the case of the earth attracted by the sun or moon, this equation becomes

$$\frac{d\omega}{dt} = \frac{3m'}{r'^3} \lambda(\Delta u' : \gamma)(Du' : \gamma);$$

$\gamma$  being the polar axis, and  $\lambda = \frac{C-A}{A}$ .

10th. The mean daily motion of  $\gamma$  is given by the equation

$$\frac{d\gamma}{dt} = \frac{3m'}{nr'^3} \lambda(\Delta u' : \gamma)(Du' : \gamma);$$

which equation gives immediately all the well-known expressions for solar and lunar precession and nutation, for  $\frac{d\gamma}{dt}$  is the symbol of the velocity of the north pole, representing that velocity both in magnitude and direction.

Supplement to a Memoir on some cases of Fluid Motion. By G. G. Stokes, M.A., Fellow of Pembroke College, Cambridge.

In a former paper the author had given the mathematical calculation of an instance of fluid motion, which seemed to offer an accurate means of comparing theory and observation in a class of motions, in which, so far as the author is aware, they had not been hitherto compared. The instance referred to is that in which a vessel or box of the form of a rectangular parallelepiped is filled with fluid, closed, and made to perform small oscillations. It appears from theory that the effect of the inertia of the fluid is the same as that of a solid having the same mass, centre of gravity and principal axes, as the solidified fluid, but different principal moments of inertia. In this supplement the author gave a series for the calculation of the principal moments, which is more rapidly convergent than one which he had previously given. It is remarkable that these series, though numerically equal, appear under very different forms, the  $n$ th term of

the latter containing exponentials of the forms  $e^{n\pi x}$  and  $e^{\frac{n\pi}{x}}$ , while the  $n$ th term of the former contains exponentials of the second form only. In conclusion, the author referred to some experiments which he had performed with a box, such as that described, filled with water, employing the method of bifilar oscillations. The moment of inertia of the fluid about an axis passing through its centre of gravity (*i. e.* the moment of inertia of the imaginary solid which may be substituted for the fluid), was a little greater as determined by experiment than as determined by theory, as might have been expected, since the friction of the fluid was not considered in the calculation. The difference between theory and experiment varied in different cases from the  $\frac{1}{13}$ th to the  $\frac{1}{31}$ st part of the whole quantity.

Dec. 7.—On the Principle of Continuity in reference to certain results of Analysis. By Professor Young of Belfast College.

The object of this paper is to inquire into the influence of the law of continuity, as it affects the extreme or ultimate values of variable functions, more especially those involving infinite series and definite integrals.

The author considers that this influence has hitherto been improperly overlooked; and that to this circumstance is to be attributed the errors and perplexities with which the different theories of those functions are found to be embarrassed. He shows that every particular case of a general analytical form—even the ultimate or limiting case—must come under the control of the law implied in that form; this law being equally efficient throughout the entire range of individual values. Except in the limiting cases, the law in question is palpably impressed on the several particular forms; but at the limits it has been suffered to escape recognition, because indications of its presence have not been actually preserved in the notation.

It is in this way that the series  $1-1+1-1+\&c.$  has been confounded with the limits of the series  $1-x+x^2-x^3+\&c.$ ; these limits being arrived at by the continuous variation of  $x$  from some inferior value up to  $x=1$ , and from some superior value down to  $x=1$ . It is shown however that the series  $1-1+\&c.$  has no equivalent among the individual cases of  $1-x+x^2-\&c.$ , with which latter, indeed, it has no connexion whatever.

By properly distinguishing between the real limits, and what is generally confounded with them, the author arrives at several conclusions respecting the limiting values of infinite series directly opposed to those of Cauchy, Poisson, and others. And to prevent a recurrence of errors arising from a neglect of the distinction here noticed, he proposes to call such an isolated series as  $1-1+1-\&c.$  *independent* or *neutral*; and the extreme cases of  $1-x+x^2-\&c.$ , *dependent* series: the difference between a dependent and a neutral series becomes sufficiently marked, as respects notation, by introducing into the former what the author calls the *symbol of continuity*, which indeed is no other than the factor, whose ascending powers Poisson introduces—and, as here shown, unwarrantably—into the successive terms of strictly *neutral* series; thus bringing such series

under the control of a law to which in reality they owe no obedience.

An error somewhat analogous to this is shown to be committed in the treatment of certain definite integrals, which are here submitted to examination and correction, and some disputed and hitherto unsettled points in their theory fully considered. The author is thus led to what he considers an interesting fact in analysis; viz. that the *differentials* of certain forms require *indeterminate corrections*, in a manner similar to that by which *determinate corrections* are introduced into *integrals*; and he attributes to the neglect of these the many erroneous summations assigned to certain trigonometrical series. This is illustrated by a reference to the processes of Poisson.

The paper concludes with some observations on what has been called *discontinuity*; a term which the author thinks is sometimes injudiciously employed in analysis, and prefers to treat discontinuous functions as implying distinct continuities; and by considering these in accordance with the principles established in the former part of the paper, he arrives at results for definite integrals of the form

$\int_{-m}^{+n} x^{-p} dx$  totally different from those obtained by Poisson. Two

notes are appended to the paper; one explaining what the author denominates *insensible convergency* and *insensible divergency*, and the other discussing some conclusions of Abel in reference to certain trigonometrical developments.

March 1, 1847.—On the Theory of Oscillatory Waves. By G. G. Stokes, M.A., Fellow of Pembroke College.

The waves which form the subject of this paper are characterized by the property of being propagated with a constant velocity, and without degradation, or change of form of any kind. The principal object of the paper is to investigate the form of these waves, and their velocity of propagation, to a second approximation; the height of the waves being supposed small, but finite. It is shown that the elevated and depressed portions of the fluid are not similar, as is the case to a first approximation; but the hollows are broad and shallow, the elevations comparatively narrow and high. The velocity of propagation is the same as to a first approximation, and is therefore independent of the height of the waves. It is remarkable that the forward motion of the particles near the surface is not exactly compensated by their backward motion, as is the case to a first approximation; so that the fluid near the surface, in addition to its motion of oscillation, is flowing with a small velocity in the direction in which the waves are propagated; and this velocity admits of expression in terms of the length and height of the waves. The knowledge of this circumstance may be of some use in leading to a more correct estimate of the allowance to be made for leeway in the case of a ship at sea. The author has proceeded to a third approximation in the case in which the depth of the fluid is very great, and finds that the velocity of propagation is increased by a small quantity, which bears to the whole a ratio depending on the square of the ratio of the height of the waves to their length.

In the concluding part of the paper is given the velocity of propagation of a series of waves propagated along the common surface of two fluids, of which the upper is bounded by a horizontal rigid plane. There is also given the velocity of propagation of the above series, as well as that of the series propagated along the upper surface of the upper fluid, in the case in which the upper surface is free. In these investigations the squares of small quantities are omitted.

March 15.—Contributions towards a System of Symbolical Geometry and Mechanics. By the Rev. M. O'Brien.

The distinction which has been made by an eminent authority in mathematics between *arithmetical* and *symbolical* algebra, may be extended to most of the sciences which call in the aid of algebra. Thus we may distinguish between *symbolical geometry* and *arithmetical geometry*, *symbolical mechanics* and *arithmetical mechanics*. This distinction does not imply that in one division numbers only are used, and in the other symbols, for symbols are equally used in both; but it relates to the degree of generality of the symbolization. In the arithmetical science, the symbols have a purely numerical signification; but in the symbolical they represent, not only abstract quantity, but also all the circumstances which, as it is expressed, *affect* quantity. The arithmetical science is in fact the first step of generalization, the symbolical is the complete generalization.

In this view of the case, the author has entitled his paper Contributions towards a System of Symbolical Geometry and Mechanics. The proposed geometrical system consists, first, in representing curves and surfaces, not by equations, as in the Cartesian method, but by *single symbols*; and secondly, in using the *differential notation* proposed in a former paper\* to denote *perpendicularity*, and to express various equations and conditions. The proposed mechanical system is analogous in many respects. Examples of it have already been given in the paper just quoted.

The author uses the term *direction unit* to denote a line of a unity of length drawn in any particular direction; and he employs the symbols  $\alpha$   $\beta$   $\gamma$  to denote any three direction units at right angles to each other.

He defines the position of any point P in space by the symbol representing the line OP (O being the origin) in magnitude and direction. If  $x$   $y$   $z$  be the numerical values of the coordinates of P, and  $\alpha$   $\beta$   $\gamma$  the direction units of the coordinate axes, the expression

$$x\alpha + y\beta + z\gamma$$

represents the line OP in magnitude and direction, and therefore defines the position of P. This expression he calls the *symbol* of the point P.

If  $r$  be the numerical magnitude, and  $\epsilon$  the direction unit of OP, we have

$$r\epsilon = x\alpha + y\beta + z\gamma :$$

$r\epsilon$  is therefore another form for the symbol of the point P.

\* Read Nov. 23, 1846.



The following is the method by which the author represents curves and surfaces.

If the symbol of a point involves an arbitrary quantity, or, as it is called, a variable parameter, the position of the point becomes indeterminate, but so far restricted that it will be always found on some line or curve. Hence the symbol of a point becomes the symbol of a line or curve when it involves a variable parameter.

In like manner, when the symbol of a point involves *two* variable parameters, it becomes the symbol of a surface.

The parameters here spoken of are supposed to be numerical quantities. An arbitrary direction unit is clearly equivalent to two such parameters; and therefore, when the symbol of a point involves an arbitrary direction unit, it becomes the symbol of a surface.

The following are examples of this method :—

1. If  $u$  be the symbol of any particular point of a right line whose direction unit is  $\epsilon$ , then the symbol of that right line is

$$u + r\epsilon,$$

$r$  being arbitrary.

2. If  $u$  be the symbol of the centre of a sphere, and  $r$  its radius, the symbol of the surface of a sphere is

$$u + r\epsilon,$$

$\epsilon$  being an arbitrary direction unit.

3. If  $u$  be the symbol of any particular point of a plane,  $\epsilon$  and  $\epsilon'$  the direction units of any two lines in the plane, the symbol of the plane is

$$u + r\epsilon + r'\epsilon',$$

$r$  and  $r'$  being arbitrary.

4. If  $\epsilon$  be the direction unit and  $r$  the numerical magnitude of the perpendicular from the origin on a plane, the symbol of the plane is

$$r\epsilon + Dv.\epsilon,$$

$v$  being an arbitrary line symbol, *i. e.* denoting in magnitude and direction any arbitrary line.

5. If  $u$  and  $u'$  be the symbols of two points, the symbol of the right line drawn through them is

$$u + m(u' - u),$$

$m$  being arbitrary.

6. If  $u$  be the symbol of any curve in space, the symbol of the tangent at the point  $u$  is

$$u + mdu,$$

$m$  being arbitrary.

7. The symbol of the osculating plane at the point  $u$  is

$$u + mdu + m'd^2u,$$

$m$  and  $m'$  being arbitrary.

8. If  $s$  denotes the length of the arc of the curve, and  $\epsilon$  the direction unit of the tangent, then

$$\epsilon = \frac{du}{ds}.$$

9.  $\frac{ds}{ds}$  or  $\frac{1}{ds} d\left(\frac{du}{ds}\right)$  represents a line equal to the reciprocal of the radius of curvature drawn from the point  $u$  towards the centre of curvature, i. e. it represents what may be called the *index of curvature* in magnitude and direction.

Hence, since  $u = x\alpha + y\beta + z\gamma$ , the numerical magnitude of  $\frac{1}{ds} d\left(\frac{du}{ds}\right)$  is

$$\frac{1}{ds} \sqrt{\left\{ \left(d\frac{dx}{ds}\right)^2 + \left(d\frac{dy}{ds}\right)^2 + \left(d\frac{dz}{ds}\right)^2 \right\}},$$

which is the general expression for the reciprocal of the radius of curvature.

10. The symbol of the normal which lies in the osculating plane is

$$u + md\left(\frac{du}{ds}\right),$$

$m$  being arbitrary.

11. The symbol of *any* normal at the point  $u$ , i. e. the symbol of the normal plane, is

$$u + Dv \cdot du,$$

$v$  being an arbitrary line symbol.

12. The symbol of the normal perpendicular to the osculating plane is

$$u + mDd^2u \cdot du,$$

$m$  being arbitrary.

13. If  $u$  be the symbol of a surface, involving therefore two variable parameters,  $\lambda$  and  $\mu$  suppose, then the symbol of the normal at the point  $u$  is

$$u + mD \frac{du}{d\lambda} \cdot \frac{du}{d\mu},$$

$m$  being arbitrary.

14. The symbol of the tangent plane at the point  $u$  is

$$u + mdu, \text{ or } u + m \frac{du}{d\lambda} + n \frac{du}{d\mu},$$

$m$  and  $n$  being arbitrary.

15. The symbol of the plane which contains the three points  $u u' u''$  is

$$u + m(u' - u) + n(u'' - u).$$

16. If  $u$  be the symbol of a right line, the symbol of the plane containing it and the point  $u'$  is

$$u + m(u' - u).$$

The following are examples of the proposed mechanical system in addition to those given in the paper already quoted.

1. If  $r$  be the radius vector of a planet, and  $\alpha \beta \gamma$  be chosen so that  $\alpha$  is the direction unit of the radius vector, and  $\gamma$  perpendicular to the plane of the orbit, it may be shown immediately by the sym-

bolical method, that the symbol of the force acting on the planet is

$$\left(\frac{d^2r}{dt^2} - r\omega^2\right)\alpha + \frac{1}{r} \frac{d(r^2\omega)}{dt} \beta + r\omega\omega'\gamma,$$

where  $\omega$  is the angular velocity of the planet, and  $\omega'$  that of the plane of the orbit about the radius vector. The expressions for the three component forces along  $r$ , perpendicular to  $r$ , and perpendicular to the plane of the orbit, are the coefficients of  $\alpha \beta \gamma$  in this expression.

2. The equation of motion of the planet, when the force is the attraction of a fixed centre varying as the inverse square of the distance, is

$$\frac{d^2u}{dt^2} = -\frac{\mu\alpha}{r^3}.$$

It is curious that this equation is *immediately* integrable, the integral being the two equations

$$r^2\omega = h;$$

$$\frac{h}{r} = \frac{\mu}{h} + e\Delta\beta.\epsilon.$$

The latter equation is the symbolical equation of a conic section, the origin being focus,  $h$   $c$  and  $\epsilon$  being the arbitrary constants introduced by integration.

3. The application of this method to the case of a planet acted on by a disturbing force is worthy of particular notice, as it expresses the variations of the elements of the orbit with great facility, in the following manner:—

If  $U$  be the symbol of the disturbing force, we have

$$\frac{d(h\gamma)}{dt} = Du.U \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

$$\frac{d(e\epsilon)}{dt} = \frac{\mu r}{h^2} \beta \Delta\beta.U + U. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

These two equations determine with great facility all the elements of the orbit. For  $\gamma$  is a direction unit perpendicular to the plane of the orbit (*i. e.* it is the symbol of the pole of the orbit), and therefore it defines completely the position of the plane of the orbit. Also  $\epsilon$  is a direction unit in the plane of the orbit at right angles to the axis major, and therefore it determines the position of the axis major; in fact the direction unit of the axis major is  $D\gamma.\epsilon$ . The letters  $h$  and  $e$  have their usual signification.

To find  $h$  and  $\gamma$  separately from (1.), suppose that we obtain by integration of (1.)

$$h\gamma = W;$$

then  $h^2 = \Delta W.W$ ; and  $h$  being thus found, we have  $\gamma = \frac{W}{h}$ . The same observation applies to (2.).

4. The expression for the parallax of the planet is

$$\frac{\mu}{h^2} + \Delta\beta \cdot \int dt \left( \frac{\mu r}{h^2} \beta \Delta\beta \cdot U + U \right).$$

These instances suffice to show the nature of the proposed symbolical method.

ROYAL ASTRONOMICAL SOCIETY.

[Continued from vol. xxx. p. 211.]

April 9, 1847.—On an important error in Bouvard's Tables of Saturn. By Mr. Adams.

Having lately entered upon a comparison of the theory of Saturn with the Greenwich observations, I was immediately struck with the magnitude of the tabular errors in heliocentric latitude, and the more so, since the whole perturbation in latitude is so small, that it could not be imagined that these errors arose from any imperfection in the theory. In order to examine the nature of the errors, I treated them by the method of curves, taking the times of observation as abscissæ, and the corresponding tabular errors as ordinates. After eliminating, by a graphical process, the effects of a change in the node and inclination, a well-defined inequality became apparent, the period of which was nearly twice that of Saturn. One of the principal terms of the perturbation in latitude (viz. that depending on the mean longitude of Jupiter minus twice that of Saturn) having nearly the same period, I was next led to examine whether this term had been correctly tabulated by Bouvard. The formula in the introduction appeared to be accurate; but on inspecting the Table XLII., which professes to be constructed by means of this formula, I was surprised to find that there was not the smallest correspondence between the numbers given by the formula and those contained in the table, the latter following the simple progression of sines, while the formula contained two terms. The origin of this mistake is rather curious. Bouvard's formula for the terms in question is

$$9''\cdot67 \sin\{\phi - 2\phi' - 60^\circ\cdot29\} + 28''\cdot19 \sin\{2\phi - 4\phi' + 66^\circ\cdot12\};$$

but in tabulating the last term he appears to have taken the simple argument  $\phi - 2\phi'$  instead of  $2\phi - 4\phi'$ , so that the two parts may be united into a single term,

$$25''\cdot85 \sin\{\phi - 2\phi' + 43^\circ\cdot88\},$$

which I find very closely to represent Bouvard's Table XLII.

After correcting the above error, and making a proper alteration in the inclinations and place of the node, the remaining errors of latitude are in general very small. I subjoin a correct table to be used instead of Bouvard's. The constant added being  $36''\cdot0$  instead of  $26''\cdot0$ , it will be necessary to subtract  $10''\cdot0$  from the final result.



Table XLII.—Argument III. de la Longitude.

Argument.	Equation.	Argument.	Equation.	Argument.	Equation.	Argument.	Equation.
0	52.4	2500	17.4	5000	68.1	7500	6.1
100	54.4	2600	16.2	5100	69.4	7600	4.0
200	56.0	2700	15.5	5200	70.2	7700	2.3
300	57.2	2800	15.2	5300	70.5	7800	1.1
400	58.0	2900	15.2	5400	70.4	7900	0.4
500	58.3	3000	15.7	5500	69.8	8000	0.1
600	58.3	3100	16.6	5600	68.7	8100	0.4
700	57.8	3200	17.9	5700	67.2	8200	1.0
800	56.9	3300	19.6	5800	65.3	8300	2.2
900	55.7	3400	21.7	5900	62.9	8400	3.7
1000	54.1	3500	24.1	6000	60.1	8500	5.7
1100	52.2	3600	26.7	6100	57.1	8600	8.0
1200	50.0	3700	29.7	6200	53.7	8700	10.7
1300	47.5	3800	32.8	6300	50.0	8800	13.7
1400	44.9	3900	36.2	6400	46.2	8900	16.8
1500	42.1	4000	39.6	6500	42.1	9000	20.2
1600	39.2	4100	43.1	6600	38.0	9100	23.7
1700	36.2	4200	46.5	6700	33.9	9200	27.3
1800	33.3	4300	50.0	6800	29.8	9300	31.0
1900	30.4	4400	53.3	6900	25.7	9400	34.5
2000	27.7	4500	56.5	7000	21.8	9500	38.0
2100	25.1	4600	59.4	7100	18.1	9600	41.4
2200	22.8	4700	62.1	7200	14.6	9700	44.6
2300	20.6	4800	64.5	7300	11.4	9800	47.5
2400	18.8	4900	66.5	7400	8.5	9900	50.1
2500	17.4	5000	68.1	7500	6.1	1000	52.4

Constante ajoutée 36''·0.

On the Development of the Disturbing Function R. By Sir John Lubbock.

The greatest practical difficulty which is encountered in the planetary theory consists in the development of the expression for the reciprocal of the linear distance between the disturbed and disturbing planets. The algebraical expression of this development may be obtained either by means of the binomial theorem or by Taylor's theorem applied to several variables; by the latter method M. Binet has carried the development as far as terms of the 7th order. But when high powers of the eccentricities and inclinations are retained, the expressions become excessively complicated, so that further progress in this direction appears utterly hopeless.

The numerical coefficients of the series may also be obtained by quadratures; but to determine all the coefficients in this way would involve very great labour.

In considering the problem of the perturbations of bodies whose eccentricities and inclinations are considerable, the author has been led to another mode of development, which he conceives to possess great advantages over those just mentioned, and the use of which may be greatly facilitated in all cases by special tables, which may be prepared beforehand.

The principle of this method consists in expressing the square of the ratio of the distance between the two planets to the radius vector of the more distant planet, under the form of  $P - Q$ , in which  $P$  is the product of any convenient number of factors of the form  $1 + A \cos \alpha$ , and all the terms in  $Q$  have small coefficients. Then  $(P - Q)^{-\frac{1}{2}}$  may be developed by the binomial theorem in a series of ascending powers of  $Q$ , which consequently converges rapidly, and the values of the quantities  $P^{-\frac{1}{2}}$ ,  $P^{-\frac{3}{2}}$ , &c., which enter into the successive terms of this series, may be found by multiplying together the developments of the several factors  $(1 + A \cos \alpha)^{-\frac{1}{2}}$ ,  $(1 + A \cos \alpha)^{-\frac{3}{2}}$ , &c. If then tables were prepared, giving for different values of  $A$  the coefficients of the development of these quantities in cosines of multiples of  $\alpha$ , all the operations requisite for the development of the disturbing function might be performed with great facility.

The author remarks, in conclusion, that instead of developing, as is usually done, in powers of the ratio of the mean distances  $\left(\frac{a}{a'}\right)$ , it would be preferable to develop according to powers of  $\sqrt{\frac{a}{a^2 + a'^2}}$ , which is much less than the former when  $a$  and  $a'$  do not differ widely from each other.

Observations of Hind's Second Comet in full Sunshine\*. By Mr. Hind.

I take the liberty to send you two positions of the comet discovered here Feb. 6, obtained yesterday in full daylight, and about five hours before the perihelion passage. The visibility of a comet in the day-time, and within  $2^\circ$  distance from the sun, is a phænomenon of so rare occurrence, that it may in some measure interest you if I give very briefly the particulars of our observations.

I had determined, by theory, that the intensity of light on March 30 ought to be 100 times stronger than that of a star of fourth magnitude, and was induced to make preparations for a daylight observation. I first saw the comet about 11 A.M. When the sky was perfectly cloudless about the sun, it had a whitish appearance, which rendered it a matter of no little difficulty to see the comet; but during the passage of some cumuli clouds over the sun, and between the breaks, I obtained some excellent views of the comet, and several observations, which will no doubt be of great assistance in the accurate determination of the elements. The nucleus was nearly, if not perfectly, round, beautifully defined and planetary, the diameter  $8''$  or  $10''$ . Two faint branches of light formed a divided tail, extending about  $40''$  from the head, like two longish erect ears or horns rising from each side of the disc. At times I felt certain that the nucleus *twinkled*. The tail resembled a thin smoke.

With respect to the observations for position, I can only add that they were as good as could possibly be made, under the circum-

\* The comet was seen at noon near the sun by two other observers, at Truro and in the Isle of Anglesey.

stances, by instrumental comparisons. The index errors are very constant, and were accurately determined last evening.

	Greenwich M.T.	Comet's R.A.	Comet's Dec.	Weight.
	<sup>h</sup> <sup>m</sup> <sup>s</sup>	<sup>h</sup> <sup>m</sup> <sup>s</sup>	<sup>°</sup> <sup>'</sup> <sup>"</sup>	
March 30	1 23 40	7 32 27	+1 48 42	1
	1 55 8	7 33 56	+1 45 21	2

In the observations for the first position the centre of the field was estimated, and nine single results are tolerably accordant. The second place depends on one good observation with cross wires, clouds preventing any further comparisons.

Had the sky been free from the whiteness which is so fatal to vision by daylight, I should have obtained much better places.

I communicated an ephemeris to Mr. Dawes, who has observed the comet with extreme care, but I do not know at present whether he saw it yesterday in daylight.

## XXVIII. *Intelligence and Miscellaneous Articles.*

### ON A NEW TEST FOR PRUSSIC ACID, AND ON A SIMPLE METHOD OF PREPARING THE SULPHOCYANIDE OF AMMONIUM. BY PROF. LIEBIG.

**W**HEN some sulphuret of ammonium and caustic ammonia are added to a concentrated aqueous solution of prussic acid, and the mixture heated with the addition of pure flowers of sulphur, the prussic acid is converted in a few minutes into sulphocyanide of ammonium. This metamorphosis depends on the circumstance, that the higher sulphurets of ammonium are instantly deprived by the cyanide of ammonium of the excess of sulphur they contain above the monosulphuret; for instance, if a mixture of prussic acid and ammonia be added to the pentasulphuret of ammonium, the solution of which is of a deep yellow colour, and the whole gently heated, the sulphuret of ammonium is soon decolorized; and when the clear colourless liquid is evaporated, and the admixture of sulphuret of ammonium expelled, a white saline mass is obtained, which dissolves entirely in alcohol. The solution yields, on cooling or evaporation, colourless crystals of pure sulphocyanide of ammonium. Only a small quantity of sulphuret of ammonium is requisite to convert, in the presence of an excess of sulphur, unlimited quantities of cyanide of ammonium into sulphocyanide; because the sulphuret of ammonium, when reduced to the state of monosulphuret, constantly reacquires its power of dissolving sulphur and transferring it to the cyanide of ammonium. The following proportions will be found to be advantageous:—2 oz. of solution of caustic ammonia of 0·95 spec. grav. are saturated with sulphuretted hydrogen gas; the hydrosulphate of ammonia thus obtained is mixed with 6 oz. of the same solution of ammonia, and to this mixture 2 oz. of flowers of sulphur are added; and then the product resulting from the distillation of 6 oz. prussiate of potash, 3 oz. of the hydrate of sulphuric acid, and 18 oz. water. This mixture is digested in the

water-bath until the sulphur is seen to be no longer altered and the liquid has assumed a yellow colour; it is then heated to boiling, and kept at this temperature until the sulphuret of ammonium has been expelled and the liquid has again become colourless. The deposited, or excess of, sulphur is now removed by filtration, and the liquid evaporated to crystallization. In this way from  $3\frac{1}{3}$  to  $3\frac{1}{2}$  oz. of dazzling white dry sulphocyanide of ammonium are obtained, which may be employed as a reagent, and for the same purposes as the sulphocyanide of potassium. Of the 2 oz. of sulphur added,  $\frac{1}{2}$  an oz. is left undissolved.

The behaviour of the higher sulphurets of ammonium towards prussic acid furnishes an admirable test for this acid. A couple of drops of a prussic acid, which has been diluted with so much water that it no longer gives any certain reaction with salts of iron by the formation of prussian blue, when mixed with a drop of sulphuret of ammonium and heated upon a watch-glass until the mixture is become colourless, yields a liquid containing sulphocyanide of ammonium, which produces with persalts of iron a very deep blood-red colour, and with persalts of copper, in the presence of sulphurous acid, a perceptible white precipitate of the sulphocyanide of copper. —Liebig's *Annalen*, Jan. 1847.

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ON THE FUSION OF IRIDIUM AND RHODIUM. BY R. HARE.

This communication respects mainly my success in fusing both iridium and rhodium, neither of which, in a *state of purity*, had been previously fused. It may be supposed that the globule of iridium, obtained by Children's colossal battery, forms an exception; but the low specific gravity and porosity of that globule may justify a belief that it was not pure, and at any rate the means employed were of a nature not to be at command for the repetition of the process, so that iridium might as well be infusible, as to be fusible only by such a battery.

The first specimen of the last-mentioned metal on which I operated was one given me by Mr. Booth, a former pupil of Wöhler, whom he had assisted in obtaining it by the excellent process devised by that distinguished chemist. This specimen was fused in the presence of Mr. Booth. Subsequently I procured specimens, warranted pure, severally from the house of Pelletier at Paris, and from Messrs. Johnson and Cock, London. Another specimen was given to me by a friend, who had received it as pure, from a source on which reliance may be placed; and lastly, I obtained myself, by Wöhler's process, a specimen of about sixty grains, from the insoluble residue of platinum ore. All the specimens thus procured were found to be fusible under my hydro-oxygen blowpipe. The specimen obtained from Messrs. Johnson and Cock, after repeated fusions, by which it was much consolidated, weighed sixty-seven grains. During fusion there appeared to be an escape of volatile matter, supposed to be osmic acid, arising from the presence of a minute portion of osmium, between which and iridium an affinity of



a peculiar degree of energy exists. At a certain point of the process a reaction took place sufficiently explosive to throw a portion of the metal, in globules, off from the support. One of these, about twice as large as the head of a common brass pin, proved to be hollow. By prolonged and repeated fusion the metal became more compact and more fusible.

Fused iridium has nearly the grain of soft cast steel, with the pale whiteness of antimony, and appears to be susceptible of a fine polish. Although as hard as untempered steel, it is somewhat sectile, since, when split by means of a cold chisel, the edge penetrated about the eighth of an inch before a division was effected. By light hammering a corner was flattened without fracture, although under heavier blows the mass cracked. I infer that although nearly unmalleable and very hard, iridium may be wrought in the lathe.

I have already mentioned that I fused into a globule a specimen of iridium obtained by me from the insoluble residuum of platinum ore by Wöhler's process. From this globule, while congealing, a portion ran out from the inside, leaving a cavity and covering one of its sides externally with an incrustation, among which crystalline spangles, or facets, were discernible. The specific gravity of the globule of iridium, from the specimen furnished by Messrs. Johnson and Cock, was taken by Mr. T. R. Eckfelt of the United States mint at Philadelphia, and by Dr. Boyé, both having balances of the greatest accuracy, and being very skilful in the employment of them. In the first instance there was a perfect coincidence in the results obtained, 21.83 being the numbers found by both of these gentlemen. Agreeably to another trial made by Dr. Boyé, using river-water instead of distilled, the number was 21.78, water being in either case about sixty-eight, with allowance for the difference of the water, and the temperature being above the standard of 60°. The specific gravity of the specimen may then be estimated at 21.80.

The specific gravity of fused platinum, purified according to the instructions of Berzelius, before subjection to the hammer, proved in one specimen to be not more than 19.70, although by hammering it became equal to 21.23. It is with fused platinum that fused iridium should be compared. Of course the specific gravity of the last-mentioned metal, when both are obtained by fusion, may be assumed to be one-tenth greater than that of the former. Moreover, as this metal is the only impurity existing in the standard platinum of London, of Paris, or of St. Petersburg, it follows that a high specific gravity is not to be viewed as a proof of purity. Accordingly a specimen of platinum, purified from iridium by the Berzelian process, and which had proved eminently susceptible of being beaten into leaf, was found only to be of the gravity of 21.16, while that of a specimen of standard Russian platinum, very brilliantly white but inferior in malleability, presented to me by his Excellency Count Cancrine, as a specimen of the purest platinum of the Russian mint, was 21.31.

Of rhodium I have fused two specimens, one of five pennyweights, purchased of Messrs. Johnson and Cock, the other received through

the same channel as the specimen of iridium above-mentioned\*. Rhodium is at least as fusible as iridium, both of the specimens alluded to having been converted into fluid globules. That procured from Messrs. Johnson and Cock gave a globule weighing ninety grains. On a second fusion it formed a perfect globule as fluid as mercury; and yet in congealing lost its brilliancy by becoming studded with crystalline facets all over its surface, excepting the portion in contact with the support. The facets had the appearance of incipient spangles. The rapidity with which they were formed seemed anomalous. The mass being split by a cold chisel and viewed by a microscope, it appeared porous immediately beneath the facets. When the mass was first fused, I found by the gravimeter the specific gravity to be 11.0, which coincides with the observations of Wollaston. Yet by a careful trial made at the United States mint by Mr. Eckfelt, after the second fusion and the formation of the facet, the specific gravity proved to be only 10.8. This is sufficiently explained by the porosity above mentioned. In fact the porosity to which rhodium and iridium are liable may render it difficult to find specimens of precisely the same specific gravity.

In sectility, malleability and hardness, rhodium did not appear to differ much from iridium, but it is not of so pale a white as iridium. The one has the pale white of antimony, the other the ruddy hue of bismuth.

Osmiuret of iridium, as existing in the native spangles associated with platina ore, or as otherwise obtained, is far more difficult of fusion than pure iridium. The propensity to assume the crystalline form, and to adhere to it, is even greater in this alloy than in the last-mentioned metal. On first exposure to the most intense heat of the hydro-oxygen blowpipe some slight appearances of fusion may be seen, and the spangles or grains may be made to cohere. Nevertheless it yields very slowly, and requires an expenditure of gas too great to be incurred unless it were for the purpose of once well determining the question of its ultimate fusibility. This object was obtained completely as respects a globule of 45 grains in weight. The specific gravity of this globule appeared to be 20.4, but this result was evidently less than that which would have been obtained had there not been some minute cavities, which, after splitting the globule, were detected by a magnifier.

The specific gravity of some large spangles of osmiuret of iridium from South American ore was, by Dr. Boyé, found to be 19.835. That of some grains heavier but not so flat, presented to me by Count Cancrine, was found to be 20.938.

That the alloy of iridium with osmium should be more difficult to fuse than pure iridium, leads to the inference that osmium must be the most infusible of the metals, although, like carbon, very susceptible of combustion, and capable, like that infusible non-metallic radical, of forming a volatile peroxide. Of course its liability to oxidizement would render it impossible to fuse it by the hydro-

\* One other larger specimen from the same source has been fused since the above was written.

oxygen blowpipe, of which the efficacy requires the simultaneous presence of oxygen and the most intense heat. It might be fused by exposure *in vacuo* to the discharge of a powerful voltaic series, by means of the apparatus of which a description with engravings has been given in a recent volume of the Transactions of the American Philosophical Society, and republished in '*Silliman's Journal*' for 1841, vol. xl. p. 303.

I have obtained osmium by heating the osmiate of ammonia in a glass tube with sal-ammoniac, agreeably to the instructions given by Berzelius. In this way a result was obtained which the information given by that distinguished chemist had not led me to anticipate. The tube became coated with a ring of osmium, which it would be impossible by inspection merely to distinguish from the arsenical ring on the peculiar features of which reliance has been placed for the detection of arsenic.

It follows from my experiments and observations, that of all metallic bodies, osmiuret of iridium is the most difficult to fuse; that rhodium and iridium are both fusible by the hydro-oxygen blowpipe, properly employed; that the former has the rosy whiteness of bismuth, the latter the pale white of antimony; and that both of them are slightly sectile, though extremely hard and nearly unmalleable; that iridium *merely fused* is heavier than platinum *condensed by the hammer*. Thus it follows from my experiments, and from the recent observations of Breithaupt, on some specimens of native iridium, that the metal, whether in this state or pure as obtained by chemical skill and consolidated by fusion, must be allowed that pre-eminence in density, which, until of late, was given to platinum.

It may be proper to add, that subsequently to the writing of the preceding narrative, receiving some large quantities of iridium and rhodium from Messrs. Johnson and Cock, my experiments were successfully repeated on a larger scale, but without any result besides that of confirming the facts above stated.—*Silliman's Journal* for Nov. 1846, p. 365.

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NOTE ON THE MEANS OF TESTING THE COMPARATIVE VALUE  
OF ASTRINGENT SUBSTANCES FOR THE PURPOSES OF TANNING.  
BY ROBERT WARINGTON, ESQ.

Having been frequently called upon to examine the value of astringent substances imported into this country for the purposes of tanning, such as valonia, divi-divi, sumac, catch, &c., I am induced to believe that the detail of the manipulation adopted may not be without interest to some of the members of the Society. As the manufacture of leather was the object of the purchaser of these materials, gelatin was selected as the basis for the estimation of their comparative value; and after several trials with various kinds of natural and manufactured gelatin, such as varieties of isinglass, glue, patent gelatin, &c., the finest long staple isinglass was found to be the most constant in its quality and least liable to undergo change.

With this therefore the test solution was prepared, of such a strength, that each division, by measure in the ordinary alkalimeter tube, should be equivalent to the one-tenth or one-fourth of a grain of pure tannin, and thus the number of divisions used would indicate the proportion of available tannin or substance precipitable by gelatin contained in any specimen. A given weight of the sample under trial was then infused in water, or if necessary the astringent matter extracted by boiling, and the clear liquid precipitated by the test solution until no further deposit occurred.

It was necessary in the course of this operation to test at intervals a portion of the solution under examination, to ascertain the progress of the trial; and this, from the nature of the precipitate, was attended at first with some little difficulty; paper filters were inadmissible from the quantity of the solution they would absorb, and thus introduce a source of extensive error; subsidence rendered the operation very tedious. The plan I have adopted is as follows:—a piece of glass tubing, about twelve inches in length and about half an inch internal diameter, is selected, and this has a small piece of wet sponge loosely introduced into its lower extremity, and when it is wished to abstract a part of the fluid under investigation for a separate testing, this is immersed a few seconds in the partially precipitated solution; the clear liquid then filters by ascent through the sponge into the tube, and is to be decanted from its other extremity into a test glass; if on adding a drop of the gelatin solution to this a fresh precipitate is caused, the whole is returned to the original bulk, and the process proceeded in, and so on until the operation is perfected; this method of operating is facilitated by conducting the examination in a deep glass. After a few trials the manipulation will be found extremely easy, and in this way considerable accuracy may be arrived at.—*From the Proceedings of the Chemical Society.*

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#### ON THE TWO VARIETIES OF ARSENIOS ACID. BY M. BUSSY.

The author first gives a new process for determining the quantity of arsenious acid. This process is based on the employment of standard reagents. The reagent which he uses is permanganate of potash, which M. Marguerite has already successfully employed for the quantitative determination of iron.

When a solution of permanganate of potash is poured into a solution of arsenious acid, it becomes arsenic acid, and the red colour of the reagent disappears. The liquor begins to become coloured only when the transformation of arsenious acid is complete. When, then, a standard solution of permanganate of potash is prepared, the quantity of arsenious acid contained in any solution may be determined by that of the permanganate required to convert it into arsenic acid.

M. Bussy states that the two varieties of arsenious acid, the vitreous and opaque, absorb the same quantity of permanganate, and consequently that the differences observed in their solubility is not derived from any difference of oxidizement.

With respect to the solubility of the two varieties of arsenious



acid, M. Bussy has arrived at the following conclusions :—1st. The vitreous, so far from being less soluble in water than the opake acid, as stated by chemists, is, on the contrary, much more soluble. This difference is nearly in the proportion of 3 to 1, at about  $53^{\circ}$  to  $55^{\circ}$  of F. ; the same quantity of water which dissolves 36 to 38 parts of the vitreous acid, will take up only 12 to 14 of the opake. 2nd. The vitreous acid dissolves much more rapidly than the opake acid. 3rd. Neither of the varieties possesses a degree of solubility which is to be regarded as strictly peculiar to it. 4th. The opake acid is converted into vitreous acid by long boiling in water ; that is to say, it then acquires the same degree of solubility as the vitreous arsenious acid, which is such that 11 parts are dissolved by 100 of water. 5th. Under the influence of water and a low temperature, the vitreous acid is converted into opake acid ; that is to say, a solution of vitreous acid becomes reduced after a certain time to the point of saturation which belongs to the opake acid. 6th. The mixture of the two varieties of acid in the same solution explains the anomalies observed in the solubility of arsenious acid, which in fact offers nothing opposed to the principles admitted by chemists. 7th. Division, which facilitates the solution of the opake acid, without however increasing its solubility, considerably diminishes that of the vitreous acid ; and to such an extent, that this acid, reduced to fine powder and levigated, is not sensibly more soluble in water than the opake acid ; this resulting unquestionably from a transformation which it undergoes, either at the moment of pulverization, or of its contact with water. 8th. Acid which has been rendered opake by the action of ammonia, and acid crystallized in water, act similarly with water, and appear to belong to the same variety. 9th. The opake acid dissolves more slowly than the vitreous in dilute hydrochloric acid. This circumstance, which thus modifies the nature of the products formed during solution, explains why the luminous phenomena observed by M. Rose in the crystallization of the vitreous acid, are not in general observable with so great intensity in the solution of the opake variety. 10th. The difference which has been observed in the action of the two arsenious acids on tincture of litmus is merely apparent. If the opake acid does not redden the tincture, it is on account of its slight solubility, and especially because it dissolves slowly ; whilst the vitreous acid, which dissolves quickly, immediately reddens the tincture. But if comparative experiments be made, and the tincture be exposed to the action of the powder, it becomes gradually red, and no difference is perceptible at the expiration of three or four days.—*Comptes Rendus*, Mai 1847.

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#### ON THE PREPARATION OF GUN-COTTON.

Mr. Coathupe recently forwarded to the Chemical Society two specimens of gun-cotton, with a view to illustrate the greatly increased explosive effects that are to be derived from a subsequent immersion of the gun-cotton, when properly prepared in the ordinary way, in a saturated solution of chlorate of potash.

" Having experimented with solutions of nitrate of ammonia, nitrate of potash, nitrate of soda, bichromate of potash, &c. &c., for the purpose of increasing the explosive properties of this interesting substance, I can affirm that none of the results will bear the slightest comparison with those obtained from the solution of chlorate of potash, either in rapidity of ignition or in intensity of flame. The process adopted for preparing the inclosed specimens was as follows : viz. into a mixture of equal measures of strong *nitrous* acid and of oil of vitriol, spec. grav. 1.845, the cotton was immersed and stirred with a glass rod during about three minutes : it was then well-washed in many waters and dried ; a portion of it was then soaked for a few minutes in a saturated solution of chlorate of potash, well-squeezed and dried."

#### ON BALSAM OF TOLU, AND SOME PRODUCTS DERIVED FROM IT.

M. E. Kopp states that the experiments which he has made on this substance confirm the greater number of the results previously obtained. He remarks that the balsam is composed of a very small quantity of tolene  $C^{10}H^{16}$ ,  $C=75$   $H=6.25$  ; of free cinnamic acid,  $C^{18}H^{16}O^4$  ; of a resin very soluble in alcohol,  $C^{36}H^{28}O^8$  ; of a resin slightly soluble in alcohol,  $C^{18}H^{20}O^5$ , or  $C^{36}H^{40}O^{10}$ .

*Tolen.*—This carburetted hydrogen was prepared by exactly following the plan proposed by M. Deville. It is colourless, very fluid, of a penetrating taste somewhat like pepper, and its smell resembles that of elemi. Its density at  $60^{\circ}$  F. is 0.858 ; its boiling-point is between  $310^{\circ}$  and  $320^{\circ}$  F. Exposed in an imperfectly closed tube, it gradually becomes resinous and very slightly coloured. M. Deville gives as its formula  $C^{12}H^{18}$ . M. Kopp states that his analysis, which differs but little from that of M. Deville, indicates  $C^{10}H^{16}$ .

*Cinnamic Acid.*—The free acid of balsam of Tolu, as observed by M. Fremy, is merely cinnamic acid. This fact was proved by analysis, and by its conversion into nitrocinnamic acid, very slightly soluble in cold alcohol ; whereas benzoic and nitrobenzoic acids are very soluble in it. The results obtained by M. Deville are probably derived from his having examined the acids procured by the distillation of the balsam, or extracted by concentrated alkaline solutions.

M. Kopp has shown that, under these two circumstances, the resins of balsam of Tolu are so changed as to give rise to a large proportion of benzoic acid. The resins, cautiously distilled with caustic soda, yield pure benzoen, and a coaly residue which contains much benzoate of soda. Cinnamic acid, mixed with cold concentrated caustic soda, and submitted to a current of chlorine, is converted into chlorocinnamic acid  $C^{18}(H^{14}Cl^2)O^4$ . If however the temperature be raised and the action is very strong, the chlorinated oil described by Mr. Stenhouse is disengaged, and chlorobenzoic acid,  $C^{14}(H^{10}Cl^2)O^4$ , is formed.

These two acids strongly resemble each other ; but the latter is more soluble in water and in alcohol, and its salts crystallize more readily. Cinnamic acid, treated with concentrated nitric acid, is at

first converted into nitrocinnamic acid, then into benzoic acid, and finally into nitrobenzoic acid.

Cinnamic and benzoic æthers are both, though with great difficulty, converted into nitrocinnamic acid and nitrobenzoic æther. There is almost always a great part of the æther decomposed, and the acids are set free.

Nitrobenzoic æther is solid, colourless, and of an aromatic odour and taste. It crystallizes in fine rhombic laminæ. Its melting-point is  $116^{\circ}$ , and its boiling-point  $564^{\circ}$ . It is easily obtained by exposing an alcoholic solution of nitrobenzoic acid to a current of hydrochloric acid gas. Its formula is  $C^{14} (H^8 N^2 O^4) O^3 + C^4 H^{10} O = C^{18} H^{18} N^2 O^8$ .

Nitrocinnamic acid dissolved in an alcoholic solution of sulphuret of ammonia is reduced with the assistance of a gentle heat. Sulphur is deposited, and two distinct substances are formed, one of which is of a yellowish colour and belongs to the class of resins, and the other to that of alkaloids. The latter is solid, colourless, crystallizes in small indistinct masses, insoluble in water, soluble in alcohol and in æther, and forms difficultly crystallizable salts.

*Resin  $\alpha$* ,  $C^{36} H^{38} O^8$ . This substance is brown, translucent, brittle when cold; its powder agglomerates at  $59^{\circ}$  F. and fuses perfectly at  $140^{\circ}$  F. Concentrated sulphuric acid imparts a purple colour to it. When dissolved in potash and exposed to the air, it is readily oxidized, and is converted into resin  $\beta$ . By dry distillation it yields benzoen and benzoic acid. It dissolves readily in alcohol and in æther.

*Resin  $\beta$* ,  $C^{18} H^{20} O^5$ . Colour dull brownish-yellow, without taste or smell, slightly fusible (above  $212^{\circ}$  F.), but little soluble in alcohol or æther. It is less alterable than the preceding resin. Sulphuric acid renders it of a violet colour; potash dissolves it with a brown colour.

The mixture of the two resins treated with nitric acid yields, as gaseous products, carbonic acid, nitrous vapours and nitric oxide; as volatile products, hydruret of benzule, hydrocyanic acid, and a little benzoic acid; as residue, a flocculent yellowish substance, which is benzoic acid intimately combined with a yellow colouring matter of a resinous nature, which destroys its crystallizing power, and accompanies it in all its combinations, even in that of æther. By the action of heat, especially by distillation, the resinous matter is destroyed, and perfectly pure benzoic acid is obtained. The resin yields nearly one-third of its weight of benzoic acid.

As to the constitution of balsam of Tolu, it seems very simple. Primarily it is formed of the soft resinous matter  $C^{36} H^{38} O^8$ , or of that which gives rise to it. This resin, under the influence of the air, is converted into cinnamic acid and resin  $\beta$ :  $C^{36} H^{38} + O^2 = C^{18} H^{16} O^4 + C^{18} H^{20} O^5 + H^2 O$ . In fact it is observed that in time balsam of Tolu becomes hard, and contains a larger quantity of cinnamic acid. The resin  $C^{18} H^{20} O^5$  may itself easily furnish benzoic acid for  $C^{18} H^{20} O^5 = C^{14} H^{12} O^4 + H^2 O + C^4 H^4$ . The carburetted hydrogen perhaps gives rise to toluene; but it is more probable that it is converted by the action of oxidizing bodies into resinous colour-

ing matter, or perhaps into water and carbonic acid.—*Ann. de Ch. et de Phys.*, Juillet 1847.

#### ON THE EQUIVALENT OF TITANIUM. BY M. ISIDORE PIERRE.

The author remarks that chemists generally agree that it would be difficult to add to the precision of the numbers which represent the equivalents of hydrogen, carbon, chlorine, bromine, iodine, phosphorus, arsenic and silicon, as determined by the researches of Dumas, Marignac and Pelouze.

M. Pierre thinks however that this is not the case with titanium; and that if the labours of different periods respecting this substance be examined, it will be evident that its equivalent requires renewed examination.

M. H. Rose originally obtained, by various methods, numbers which varied between 380 and 450; but he afterwards found that the sulphuret of titanium which he employed in his experiments, was procured free from titanous acid with great difficulty.

In his last experiments, M. Rose made use of chloride of titanium, which he decomposed by water. He precipitated with ammonia the titanous acid derived from this decomposition, and afterwards treated the filtered liquor with nitrate of silver, in order to separate the chlorine in the state of chloride of silver; this method gave him 303·686 as the equivalent of titanium.

The chloride of titanium used by M. Pierre was not prepared from rutil, but from calcined artificial oxide of titanium: it was free from oxide of iron and from chloride of silicium, and its boiling-point was perfectly stationary. The chloride employed had been kept in a small tube from the time of its preparation hermetically sealed: it was broken by agitation in a stopped bottle, one quarter filled with distilled water. By frequent agitation, without unstopping the bottle, the whitish cloud at first produced above the liquid disappears. Without this precaution there would be a probable loss of hydrochloric acid in opening the bottle too soon, or by introducing the solution of silver, which would expel a small quantity of this vapour.

The following results were obtained:—

	gr.
I. Chloride of titanium employed..	0·8215
Silver .....	1·84523
indicating Chlorine .....	0·60623
Titanium by difference.....	0·21727

These results gave 314·76 as the equivalent of titanium.

II. Chloride of titanium employed..	0·774
Silver.....	1·73909
indicating Chlorine.....	0·57136
Titanium by difference.....	0·20264

These numbers give for the equivalent of titanium 314·37.

III. Chloride of titanium employed..	0·7775
Silver.....	1·74613
indicating Chlorine .....	0·57367
Titanium by difference.....	0·20383

The equivalent of titanium deduced from this experiment is 314·94.



IV. Chloride of titanium employed..	0·716
Silver.....	1·61219
indicating Chlorine.....	0·52966
Titanium by difference.....	0·18634
Equivalent of titanium	311·84.
V. Chloride of titanium employed..	0·8085
Silver.....	1·82344
indicating Chlorine.....	0·59907
Titanium by difference.....	0·20943
Equivalent of titanium	309·38.

The three first numbers agree perfectly, but the two latter are notably less, especially the last, since it differs from the three first by five whole numbers, or more than  $1\frac{1}{2}$  per cent. It was difficult to attribute this difference entirely to deficient precision in the method used. It occurred to the author that it might be owing to the partial decomposition of the chloride of titanium, by the moisture of the air during manipulation, and this was soon found to be the case by direct experiment.

M. Pierre proposes to adopt, as the nearest approximation to truth, 314·69, the mean of the three first experiments, as the equivalent number for titanium.

This number is very different from 355 deduced from 6·536, the density of the vapour of the chloride of titanium observed by M. Dumas. Its density, calculated from 314·69, would be 6·614.—*Ann. de Ch. et de Phys.*, Juillet 1847.

**ON A MODIFICATION OF THE APPARATUS OF VARRENTRAPP AND WILL FOR THE ESTIMATION OF NITROGEN. BY WARREN DE LA RUE.**

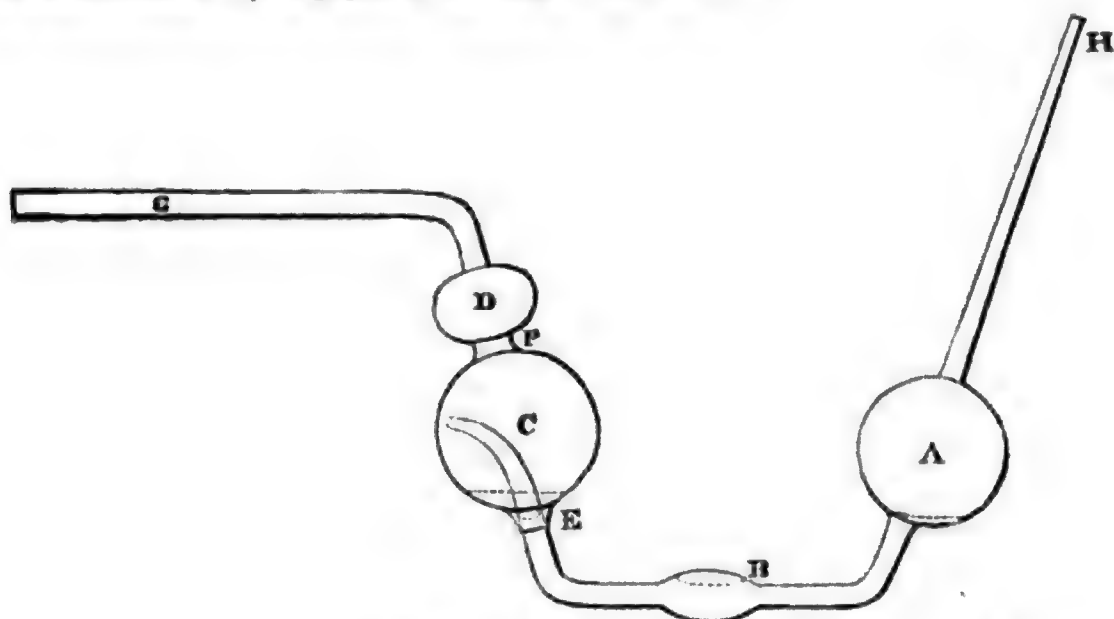
My attention having been called to a communication by Mr. Alex. Kemp in the number of the 'Chemical Gazette' for the 1st of April 1847, in which he describes a modification of Messrs. Varrentrapp and Will's tube for nitrogen determinations, of a very similar construction to one I employed as far back as November 1845 in the laboratory of the Royal College of Chemistry, and which I have repeatedly shown to my friends, I am induced to lay before the Society a description of my form of apparatus, which differs somewhat from that described by Mr. Kemp.

By the drawing, it will be seen that the tube B E, instead of opening immediately into the bottom of the flattened bulb C, is prolonged and rises for some distance into the bulb curving over towards its side; in this respect Mr. Kemp's apparatus does not differ materially from mine. I found it necessary however to have a third bulb (D) blown (which is best of a spheroidal form), in order to effectually prevent the acid from being drawn into the tube G whenever a sudden absorption took place; this third bulb communicates with C by a narrow neck. If the apparatus be constructed without

the third bulb D, a portion of fluid generally passes into the tube G from the rotary motion induced in the fluid in C.

The dotted lines indicate the height of fluid in the bulbs, and this quantity is quite sufficient for the condensation of all the ammonia likely to be formed. I would remark, that if during the progress of the combustion a cessation of the production of gas should occur, the construction of the apparatus is such as to prevent the whole of the acid ever being carried over into the bulb C, so that on the evolution again commencing no fear need be entertained for the complete condensation of the ammonia.

It only remains for me to add, that though this new form of apparatus is not so readily rinsed out as the original one of Messrs. Varrentrapp and Will, no great inconvenience is experienced from that cause, as the acid can, at the close of the operation, be easily caused to flow into the bulb C and out at the tube G, by properly inclining the bulbs, &c., and when this is done water or alcohol may be introduced by a pipette through the limb H.



*From the Proceedings of the Chemical Society.*

#### ON THE DETECTION OF COTTON IN LINEN. BY G. C. KINDT.

This subject has frequently engaged the attention of commercial and scientific men; many experiments have been made in order to detect cotton thread in linen; many processes have been recommended, but none have hitherto proved satisfactory. I was therefore much surprised when a stranger, a few weeks ago, showed me a sample of linen from the one-half of which all the cotton filaments had been eaten away. He had obtained it in Hamburg, and asked me whether I could give him a process for effecting this purpose. Now since, as far as I am aware, nothing has been published on this subject, and it is of very general interest, I consider it a duty to communicate the results of my experiments. I had already observed, in experimenting with explosive cotton, flax, &c., that these two substances behave somewhat differently towards concentrated acids; and although it has long been known that strong sulphuric acid con-

verts all vegetable fibre into gum, and when the action is continued for a longer period, into sugar, I found that cotton was metamorphosed much more rapidly by the sulphuric acid than flax. It is therefore by means of *concentrated sulphuric acid* that cotton may be removed from linen when mixed with it; and this object may be obtained by the following process:—

The sample to be examined must be freed as perfectly as possible from all dressing by repeated washing with hot rain- or river-water, boiling for some length of time, and subsequent rinsing in the same water; and I may expressly observe, that its entire removal is requisite for the experiment to succeed. When it has been well-dried, the sample is dipped for about half its length into common oil of vitriol, and kept there for about half a minute to two minutes, according to the strength of the tissue. The immersed portion is seen to become transparent. It is now placed in water, which dissolves out the gummy mass produced from the cotton; this solution may be expedited by a gentle rubbing with the fingers; but since it is not easy to remove the whole of the acid by repeated washing in fresh water, it is advisable to immerse the sample for a few instants in spirits of hartshorn (purified potash or soda have just the same effect), and then to wash it again with water. After it has been freed from the greater portion of the moisture by gentle pressure between blotting-paper, it is dried. If it contained cotton, the cotton threads are found to be wanting in that portion which had been immersed in the acid; and by counting the threads of the two portions of the sample, its quantity may be very readily estimated.

If the sample has been allowed to remain too long in sulphuric acid, the linen threads likewise become brittle, or even eaten away; if it were not left a sufficient time in it, only a portion of the cotton threads have been removed; to make this sample useful, it must be washed, dried, and the immersion in the acid repeated. When the tissue under examination consists of pure linen, the portion immersed in the acid likewise becomes transparent, but more slowly and in a uniform manner, whereas in the mixed textures the cotton threads are already perfectly transparent, while the linen threads still continue white and opaque. The sulphuric acid acts upon the flax threads of pure linen, and the sample is even somewhat transparent after drying as far as the acid acted upon it, but all the threads in the sample can be seen in their whole course.

Cotton stuffs containing no linen dissolve quickly and entirely in the acid; or if left but one instant in it, become so brittle and gummy that no one will fail to recognise it as cotton when treated in the above manner.—Liebig's *Annalen*, Feb. 1847.

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#### THE PLANET HEBE\*.

On July 1, M. Henke of Driessen in Prussia, discovered another planet, which appears to belong to the singular group lying between the orbits of Mars and Jupiter. It was first observed accurately at

\* Communicated by J. R. Hind, Esq., F.R.A.S.

Berlin by Prof. Encke on July 5, and since that date observations have been made very generally at the different European observatories. The following are the elements according to different calculators :—

	Galle and d'Arrest.	Neumann.	H <sup>i</sup> nd.
Epoch.	July 10 <sup>o</sup> 0 Berlin.	July 5 <sup>o</sup> 41864 Berlin.	July 0 <sup>o</sup> 0 Greenwich.
Mean anomaly .....	268° 55' 50.5	283° 9' 44.6	283° 56' 54.0
Long perihelion.....	19 4 14.9	9 3 9.6	8 17 24.1
Ascending node .....	139 5 3.1	138 12 16.2	137 25 35.1
Inclination .....	14 38 58.5	14 49 53.6	15 2 56.1
Sin <sup>-1</sup> e .....	10 41 16.7	13 5 48.2	13 49 20.0
Log. semi-axis major	0.3772450	0.3955266	0.4016899

The longitudes in first and second set are counted from M. Equinox of 1847.0; in the third set from M. Equinox of July 0.

### METEOROLOGICAL OBSERVATIONS FOR JUNE 1847.

*Chiswick.*—June 1—3. Clear and very fine. 4. Light clouds and fine. 5. Cloudy. 6. Light clouds: clear. 7. Clear: cloudy. 8. Rain: thunder-showers. 9. Clear and fine. 10. Rain: cloudy: clear. 11, 12. Clear and very fine. 13. Rain: cloudy. 14. Densely clouded: showery. 15. Rain: thunder and heavy showers. 16. Cloudy: rain. 17, 18. Rain. 19. Cloudy and fine. 20. Cloudy: slight showers. 21. Cloudy: fine. 22. Very fine. 23. Very fine: heavy showers, with thunder. 24. Cloudy and fine. 25. Rain: cloudy and fine. 26. Very fine. 27. Drizzly: cloudy and fine. 28. Fine. 29. Very fine. 30. Light clouds: very fine: overcast.

Mean temperature of the month ..... 58° 46

Mean temperature of June 1846 ..... 66 °63

Mean temperature of June for the last twenty years ..... 66 °90

Average amount of rain in June ..... 1.88 inch.

*Boston.*—June 1—4. Fine. 5, 6. Cloudy. 7. Fine. 8. Fine: rain early A.M. 9. Fine. 10. Cloudy: rain early A.M.: showery all day. 11, 12. Fine. 13. Cloudy: rain early A.M. 14. Cloudy: rain early A.M.: rain P.M. 15. Fine: rain P.M. 16. Fine: rain A.M. and P.M. 17. Fine. 18. Cloudy: rain early A.M.: heavy rain P.M. 19. Cloudy: rain early A.M. 20. Cloudy: rain A.M. and P.M. 21. Cloudy: rain P.M. 22, 23. Fine: rain P.M. 24. Rain: rain P.M. 25. Fine: rain P.M. 26. Fine. 27. Cloudy. 28. Fine. 29, 30. Cloudy.—This month has been the coldest since 1843, and the wettest since June 1841.

*Sandwich Manse, Orkney.*—June 1, 2. Clear: fine. 3. Cloudy: fog. 4. Bright: cloudy. 5. Showers: cloudy. 6. Bright: cloudy. 7. Showers. 8. Bright: drops. 9. Cloudy: rain. 10. Showers: sleet-showers. 11. Bright: cloudy. 12. Cloudy. 13. Cloudy: rain. 14. Rain: damp. 15. Cloudy: rain: cloudy. 16. Cloudy: fine. 17, 18. Bright: fine. 19. Clear: fine. 20. Bright: rain. 21. Showers: clear. 22. Bright: showers: fine. 23. Bright: showers. 24. Bright: thunder: drops. 25. Bright: thunder. 26. Clear: fine. 27. Damp. 28. Cloudy. 29. Fog: cloudy. 30. Damp: fog.

*Applegarth Manse, Dumfries-shire.*—June 1—3. Very fine. 4. Warm, but overcast. 5. Fair A.M.: showers P.M. 6. Fair A.M. 7. Threatening: rain P.M. 8. Slight shower. 9. Fair: thunder: rain. 10. Fair: clear. 11. Fair, but cool. 12. Cloudy: rain P.M. 13. Rain. 14. Fine: thunder: rain. 15. Drizzly: thunder. 16. Bright A.M.: rain. 17. Drizzly. 18. Fair and fine. 19. Fine: a few drops. 20. Rain P.M. 21. Wet A.M.: cleared. 22. Showery. 23. Fine, very: slight shower. 24. Showery: thunder. 25. Showers A.M.: thunder. 26. Slight shower P.M. 27. Shower A.M.: fair. 28—30. Very fine.

Mean temperature of the month ..... 55° 2

Mean temperature of June 1846 ..... 63 °2

Mean temperature of June for 25 years ..... 56 °10

Mean rain in June for 20 years... ..... 2.32 inches.



*Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at Chiswick, near London; by Mr. Veall, at Boston; by the Rev. W. Dunbar, at Applegarth Manse, Dumfries-shire; and by the Rev. C. Clouston, at Sandwick Manse, Orkney.*

Days of Month.	Barometer.						Thermometer.						Wind.				Rain.																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																									
	Chiswick.			Dumfries-shire.			Orkney, Sandwick.			Chiswick.			Boston.			Dumfries-shire.			Orkney, Sandwick.			Chiswick.			Dumfries-shire.			Orkney, Sandwick.																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																														
	Max.	Min.	Mean.	9 a.m.	9 p.m.	94 a.m.	94 p.m.	Max.	Min.	Mean.	84 a.m.	Max.	Min.	Mean.	Chiswick.	Max.	Min.	Mean.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.	94 a.m.	94 p.m.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																														

THE  
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AND  
JOURNAL OF SCIENCE.

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[THIRD SERIES.]

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SEPTEMBER 1847.

XXIX. *On certain Products of Decomposition of the Fixed Oils in contact with Sulphur.* By THOMAS ANDERSON, Esq., M.D., F.R.S.E., Lecturer on Chemistry, Edinburgh\*.

NUMEROUS researches have established as a general rule that the products of the decomposition of organic substances vary with the circumstances of the experiment, and the nature of the agents under the influence of which it is performed. If, for instance, we examine the action of heat alone, we find it causing a set of decompositions specially characterized by the evolution of carbonic acid, formed by the union of part of the carbon of the substance with the whole or part of its oxygen; and this action is rendered more definite, and the number of the products circumscribed by all circumstances facilitating the formation of carbonic acid, such as the presence of a base, which will even cause its evolution when heat alone is incapable of producing decomposition. Acids, on the other hand, have a precisely opposite effect; they, in some instances, altogether prevent the formation of carbonic acid, and cause the oxygen to exert its action on the hydrogen of the compound, and to eliminate one or more atoms of water which do not generally exist ready formed in it.

In these particular instances, decomposition takes place at the expense of the constituent atoms of the compounds themselves, the extraneous substances serving merely as disponents to the oxidation; in the one case of part of their carbon, in the other of their hydrogen. But there is another class of agents, which, besides eliminating one or more substances, are capable at the same time of entering into union with the residual atoms, and forming a new derivative of the original compound. The best investigated of this class of agents are chlo-

\* Read before the Royal Society of Edinburgh on the 19th of April, 1847, and published in their Transactions, vol. xvi. part 3, p. 363.

*Phil. Mag.* S. 8. Vol. 31. No. 207. Sept. 1847. M

rine, bromine, nitric acid and ammonia; the three former of which exert their action on the hydrogen, the latter on the oxygen of the substance, and form compounds, the complete investigation of which is important, not merely in a purely chemical point of view, but also from the light which they seem likely to throw on the general question of the atomistic constitution of matter. In fact, the great object of the researches of organic chemistry at the present moment is that of developing the relations which the individual atoms bear to the molecules of their compound, by a knowledge of which we hope eventually to arrive at some definite conclusions with regard to the mode in which the elementary atoms are grouped together in a complex molecule. Almost all the scanty information which we possess on this subject has been derived from investigating the products of the action of different agents upon organic substances; and it is sufficiently obvious, that the more varied the circumstances, and numerous the points of view under which these reactions can be examined, so much the more likely are we to arrive at definite results.

It was the consideration of these points which led me to undertake an investigation into the nature of the action of sulphur in the free state upon organic compounds, a subject hitherto totally uninvestigated, unless we except the curious researches of Zeise\* on the simultaneous action of ammonia and sulphur upon acetone, which yields a variety of remarkable products, the properties of which he has described, without however determining their constitution. The results at which I have already arrived in these researches are contained in the following pages. They are, however, to be considered only as the commencement of the investigation; and I am desirous of submitting them to the Society even in their present very imperfect state, as it is impossible to fix a period within which a series of researches, surrounded by so many difficulties, can be completed. No one who has not been specially occupied with such experiments can have any conception of the numerous sources of annoyance which they present, and of the expenditure of time and labour which is necessary for their performance. Indeed, I have more than once felt inclined altogether to abandon a subject occupying so much time in proportion to the results obtained, and the completion of which is further protracted by the nauseous odour of the compounds, which is so disgusting that it is impossible to pursue the investigation for any length of time continuously.

At the commencement of these researches I endeavoured to examine the action of sulphur upon some of the simpler

\* *Forhandlingar vid de Skandinaviska Naturforskarnes tredje möte*, p. 303.

organic compounds, in the hope of arriving at results of corresponding simplicity. My expectations, however, were disappointed, and I was obliged to have recourse to the fixed oils, on which sulphur has been long known to exert an action; the product obtained by heating together olive oil and sulphur until a uniform balsam-like substance was formed, having been employed in medicine by the older physicians under the name of the balsam of sulphur.

The phenomena which manifest themselves during the mutual action of sulphur and a fixed oil are these:—At the first application of heat, the sulphur melts and forms a stratum at the bottom of the oil; but as the temperature rises it slowly dissolves, with the formation of a thick viscid fluid of a dark red colour. As the heat approaches that at which the oil undergoes decomposition when heated alone, a violent action takes place attended by the evolution of sulphuretted hydrogen in such abundance, that the viscid mass swells up and occupies a space many times its original bulk. If at this point the mixture be allowed to cool, it concretes into a tough sticky tenacious mass, adhering strongly to the fingers, and having a disagreeable sulphureous odour; if however the heat be sustained, the frothing and evolution of sulphuretted hydrogen continue, and at the same time an oil of a peculiar disgusting odour, resembling that of garlic, but more disagreeable, passes into the receiver.

In the investigation of the products of this action, the first and most essential step was to determine the particular constituents of the oil from which they are derived. In order to do this, it was necessary to examine separately the action of sulphur upon each of its components. I commenced therefore by making use of stearic acid, which can be readily obtained in a pure state: experiment however showed that none of the peculiar products were derived from it; for when mixed with half its weight of sulphur and distilled, mere traces of sulphuretted hydrogen were evolved, and the products were identical with those obtained from the unmixed acid. The nauseous smelling oils being then obviously derived either from the oleic acid, or the glycerine of the oil, I prepared a quantity of pure oleic acid, by the decomposition of the æthereal solution of the oleate of lead. This, when mixed with half its weight of sulphur, and distilled in a capacious retort, underwent decomposition precisely as the crude fixed oil did; sulphuretted hydrogen was developed in great abundance, and the product of the distillation could not be distinguished from that which I had previously obtained. I was unable to obtain glycerine in sufficient quantity to make a separate investiga-

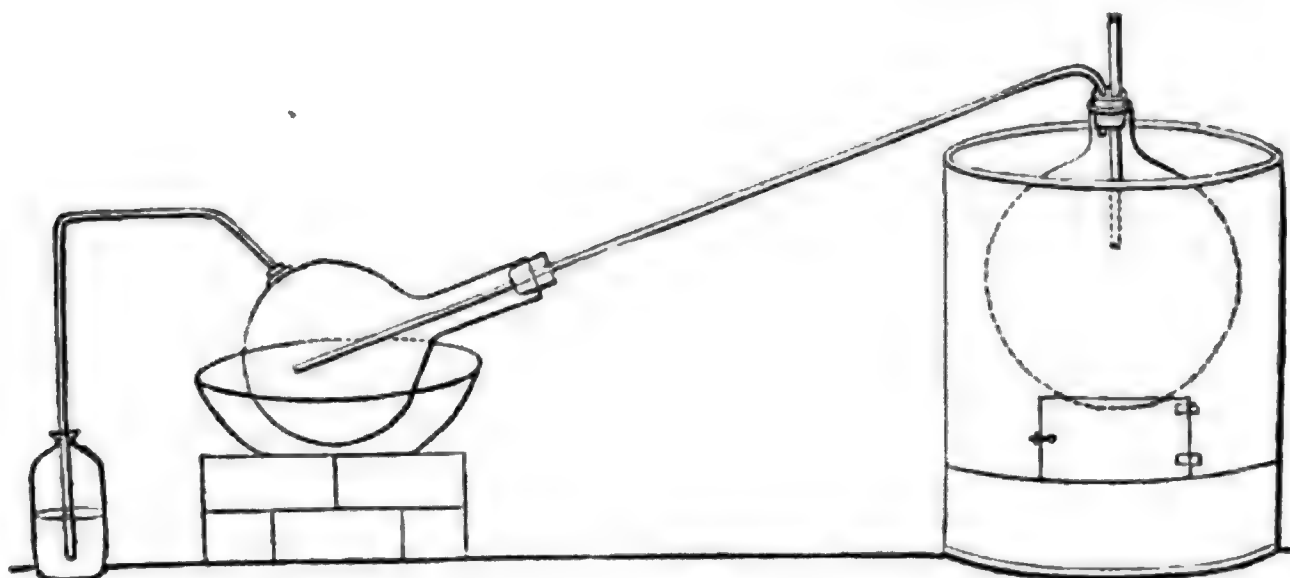


tion of the products of its decomposition ; but these must also be peculiar, as I could not distinguish the presence of acroleine during any period of the distillation of an oil with sulphur.

The product of the distillation of oleic acid was in the form of a reddish-brown oil, having an extremely nauseous odour, in which that of sulphuretted hydrogen was apparent. When rectified, this sulphuretted hydrogen was driven off, and the first portions which distilled were perfectly transparent and colourless. As the process continued, however, the products became gradually darker in colour, and the last portions which distilled became semisolid on standing, from the deposition of a quantity of white crystalline plates. These were separated by filtration through cloth, expressed strongly, and purified by successive crystallizations from alcohol, until they were entirely free from smell and colour. The product was then in the form of white pearly scales, which possessed acid properties, and were totally insoluble in water ; they were not therefore sebacic acid, no trace of which could be discovered among the products, but, on the contrary, possessed all the properties of margaric acid. These crystals were obtained from quantities of oleic acid, prepared at different times, and with the greatest possible care, and must have been formed during the decomposition. In order however to set this point at rest, some of the same oleic acid was distilled alone, when abundance of sebacic acid was obtained, and the latter portions of the rectified product did not deposit any crystals on cooling, but remained perfectly fluid. As this solid acid is produced only in comparatively small quantity, and I was unable to obtain enough of oleic acid, I made use, in preparing it on the large scale, of pure almond oil, which, according to Schübler and Gasserow, is entirely free of margarine. The oil which I employed was expressed specially for these experiments, at a temperature slightly above  $32^{\circ}$  ; and in order to satisfy myself of the absence of margaric acid in the products of its ordinary decomposition, a quantity was distilled alone, and the product rectified. The latter portions being collected apart did not deposit margaric acid ; and this I have also found to be the case with the ordinary almond oil of commerce, in the expression of which a moderate degree of heat is employed.

In distilling the oil and sulphur on the large scale, it became impossible to perform the process by the simple admixture of the substances, the frothing being so great as inevitably to expel the materials from the retort. After a trial of various methods, I found it most convenient to employ the apparatus, of which this is a sketch. The oil was introduced into a large

glass balloon, to the mouth of which two tubes were adapted, one descending to near the middle, and furnished with a cork



at the upper end, the other which constituted the neck of the distilling apparatus passed into a tubulated receiver, kept cold by immersion in water or ice. To the tubulature, a doubly bent tube was affixed, which descended into a vessel of alcohol, for the purpose of retaining any of the more volatile portions which might be carried over by the rapid current of sulphuretted hydrogen. The heat must be applied by means of an open charcoal fire; and the furnace should be so constructed that the fire may be rapidly withdrawn in the event of the action becoming too violent. It is very desirable too that the balloon should go down into the furnace, so that it may be entirely surrounded by hot air. The oil is introduced into the balloon, of which it must not occupy more than a fifth, or a fourth at most, along with a few small pieces of sulphur, and heat is gradually applied. So soon as effervescence commences, the cork of the small tube is withdrawn, and a small piece of sulphur is introduced; and this is continued gradually, so as to keep up a uniform action. A dark reddish-brown oil passes into the receiver, and at the same time sulphuretted hydrogen passes in torrents through the alcohol; it there deposits a certain quantity of oil, and on escaping, may be kept burning during the whole operation, with a flame eight or nine inches high. The principal difficulty of this process consists in regulating the heat, so as to keep up a steady action. If the heat be allowed to fall, the contents of the balloon become so viscid as inevitably to boil over; and at the same time too high a temperature causes the whole action to go on with

excessive violence. I have generally operated on quantities of three pounds, each of which requires a complete day for its distillation, during which time the operator must never leave it, but constantly attend to the regulation of the heat, and the gradual addition of sulphur in small quantities. When a quantity equal to about half the oil employed has distilled over, the remaining mass becomes excessively viscid; and just at this point the balloon frequently cracks, the contents escape, and the whole catches fire, and blazes off with a bright yellow flame and smell of sulphurous acid.

The product of this distillation, which exactly resembled that of the pure oleic acid, was rectified, and the crystals which deposited from the latter portions were expressed and purified by successive crystallizations in alcohol. They then presented all the characters of margaric acid, and gave the following results of analysis:—

I.	5.275 grains of the acid gave		
	14.558	...	carbonic acid, and
	5.919	...	water.
II.	6.358 grains of the acid gave		
	17.578	...	carbonic acid, and
	7.212	...	water.

Which gives the following results per cent.:—

	Experiment.		Calculation.		
	I.	II.			
Carbon .	75.27	75.40	75.55	C <sub>34</sub>	2500.0
Hydrogen	12.51	12.66	12.59	H <sub>34</sub>	425.0
Oxygen .	12.22	11.94	11.86	O <sub>4</sub>	400.0
	100.00	100.00	100.00		3325.0

These results agree completely with the formula for margaric acid, and were further confirmed by the analysis of its silver salt and æther.

4.643 grains of the silver salt gave 1.325 of silver = 28.53 per cent.

7.926 grains of the silver salt gave 2.284 of silver = 28.70 per cent.

The calculated result for margarate of silver gives 28.65 per cent.

The æther was prepared in the usual manner, by dissolving the acid in absolute alcohol, and passing dry hydrochloric acid gas through the solution. The product, which possessed all the properties of margaric æther, gave the following results of analysis:—

{ 5.596 grains of the æther gave  
15.662 ... carbonic acid,  
6.399 ... water.

		Experiment.	Calculation.		
Carbon	. .	76.33	76.51	C <sub>38</sub>	2850.0
Hydrogen	. .	12.70	12.74	H <sub>38</sub>	475.0
Oxygen	. .	10.97	10.79	O <sub>4</sub>	400.0
		100.00	100.00		3725.0

These analyses establish, in a satisfactory manner, that the acid produced was margaric acid. It is scarcely possible however, in the present state of the investigation, to give anything like a rational explanation of the mode in which it is here formed. Its production from oleic acid has been already observed by Laurent as the first product of oxidation by nitric acid; but the action of sulphur is certainly of a very different character, and cannot be considered as bearing any analogy to that of an oxidizing agent. The quantity of margaric acid produced does not appear to be constant, but varies with the rapidity of the distillation, and is always most abundant when it is slowly performed.

The oil which distils previous to and along with the margaric acid, and constitutes by far the most abundant product of the action of sulphur upon oleic acid and oil of almonds, is a very complex substance, and contains some of its constituents in very small proportion. On this account I found it necessary to prepare it in very large quantity; and in doing so I abandoned the use of almond oil and employed linseed oil instead, which is a much cheaper substance, and yields the same fluid products. When the product of the action of sulphur is carefully rectified, the first portions which pass over are perfectly transparent and colourless, highly limpid and mobile, and boil at the temperature of 160° Fahr. Only a small quantity however passes at this temperature, and the immersed thermometer gradually rises without indicating any fixed boiling-point for the fluid. My first attempts to purify this oil and separate it into its various constituents, did not afford any satisfactory conclusions. Numerous analyses of the more volatile portions were made without obtaining comparable results, although all indicated the presence of carbon and hydrogen nearly in the proportion of equal atoms. The following are the details of three of these analyses:—

I. { 4.657 grains of the most volatile oil gave  
12.688 ... carbonic acid, and  
5.127 ... water.



- II. { 5.501 grs. of an oil less volatile than the preceding gave  
 15.762 ... carbonic acid, and  
 6.292 ... water.
- III. { 4.191 grains of another portion of oil gave  
 12.185 ... carbonic acid, and  
 4.720 ... water.

Which correspond to the following results per cent. :—

	I.	II.	III.
Carbon . .	75.03	78.79	79.95
Hydrogen .	12.20	12.72	12.75

All these oils, when treated with fuming nitric acid, yielded an abundant precipitate of the sulphate of barytes; but as the results of the combustion were not constant, no quantitative determination was made.

The action of precipitants however upon this oil afforded a more satisfactory method of obtaining some of its constituents. It gives with corrosive sublimate a bulky white precipitate, and with bichloride of platinum a yellow compound, the characters of which vary slightly, according as it is prepared from the more or less volatile portion of the oil. Nitrate of silver and acetate of lead, mixed with the alcoholic solution of the oil, produce only a slight cloudiness, but on boiling the solutions, the sulphurets of silver and lead are deposited.

*The Mercury Compound.*—In order to obtain this substance in the pure state, the oil was dissolved in alcohol, and an alcoholic solution of corrosive sublimate added. The precipitate which fell was collected on a filter, and washed with æther until the oil was thoroughly extracted, for which purpose a considerable quantity of æther is required. It is then boiled with a large quantity of alcohol, which dissolves a part of it; and the solution being filtered hot, allows the compound to deposit, on cooling, in the pure state. It is then in the form of a white crystalline powder, having a very fine pearly lustre, and exhibiting under the microscope crystals of a very peculiar form. They are six-sided tables, two opposite angles of which are rounded off, so as to give them a very close resemblance to the section of a barrel. It possesses, even after long-continued washing with æther, a peculiar slight sickening smell, which becomes more powerful on heating, and its powder irritates the nose. It is insoluble in water, which moistens it with difficulty. It requires several hundred times its weight of boiling alcohol for solution, and is almost entirely deposited, on cooling, in microscopic crystals. In æther it is almost insoluble. When heated, it is decomposed with the evolution

of a peculiar nauseous smelling oil. The sparing solubility of this compound in alcohol renders its preparation in sufficient quantity for analysis an extremely tedious process, and I have sought in vain for a more abundant solvent. The only substance which I have found capable of taking it up in larger quantity is coal-tar naphtha; but its employment is inadmissible, as the best which can be procured is an extremely impure substance, and the crystals of the compound deposited from it always acquire a rose or violet tint from some of its impurities. Oil of turpentine likewise dissolves it, but not more abundantly than alcohol.

By many successive solutions in alcohol, I obtained enough of this substance for an analysis, of which the following are the results :—

$$\left\{ \begin{array}{l} 12.302 \text{ grains, dried in } \textit{vacuo}, \text{ gave} \\ 6.592 \quad \dots \quad \text{of carbonic acid, and} \\ 3.018 \quad \dots \quad \text{of water.} \end{array} \right.$$

8.061 grains deflagrated with a mixture of nitre and carbonate of soda, gave 7.297 grains of sulphate of baryta =  $1.0067 = 12.48$  per cent. of sulphur.

The mercury and chlorine were determined together by mixing the substance with quicklime, and introducing the mixture into a combustion-tube. The end was then drawn out into an elongated bulb, into which the mercury sublimed, and which was afterwards cut off, dried in the water-bath, and weighed, both with and without the mercury; the chlorine was determined in the usual way from the residue in the tube.

9.958 grains gave 5.976 mercury = 60.01 per cent., and 4.310 grains chloride of silver = 10.67 per cent. of chlorine.

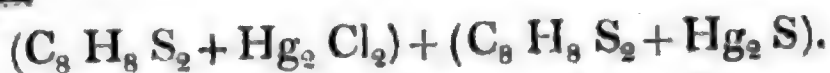
5.797 grains gave 2.409 of chloride of silver = 10.25 per cent. of chlorine.

These results correspond closely with the formula  $C_{16} H_{16} S_5 Hg_4 Cl_2$ , as is shown by the following comparisons :—

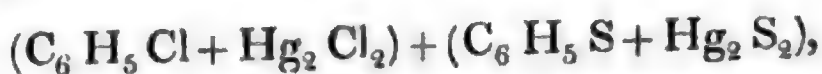
	Experiment.		Calculation.		
	I.	II.			
Carbon .	14.61	...	14.46	$C_{16}$	1200.0
Hydrogen	2.72	...	2.42	$H_{16}$	200.0
Mercury .	60.01	...	60.32	$Hg_4$	5008.6
Chlorine .	10.67	10.25	10.67	$Cl_2$	885.3
Sulphur .	12.48	...	12.13	$S_5$	1005.8
	100.49		100.00		8294.7

It is sufficiently obvious that the formula  $C_{16} H_{16} S_5 Hg_4 Cl_2$

cannot be supposed to represent the rational formula of this substance. On the contrary, the remarkable analogy between its properties and those of the mercury compound of sulphuret of allyle appear clearly to indicate a similarity in their chemical constitution,—a similarity which, as we shall afterwards see, is borne out by the properties of the platinum compound. I consider this substance to contain an organic sulphuret analogous to sulphuret of allyle, the constitution of which must be represented by the formula  $C_8 H_8 S_2$ , to which I give the provisional name of sulphuret of odmyl (from *ὀδμή*, *odour*), and that the rational formula of the mercury compound is—



On contrasting this with the formula of the allyle compound, which is—



two important points of difference are apparent, namely, that in the new compound we have the sulphuret, and not the chloride, of the base in union with corrosive sublimate, and the presence of subsulphuret in place of sulphuret of mercury in the second member of the compound. It is even possible to approximate more closely the formulæ of the allyle and odmyle compounds, by assuming the sulphuret of odmyle to be represented by  $C_4 H_4 S$ ; in which case the mercury compound becomes—



This formula is however incompatible with its reactions, as it involves the presence of calomel in the compound. Treatment with caustic potash however shows that this is not the case, as it immediately becomes yellow, from the separation of oxide of mercury, while the black suboxide would have been formed had calomel been present.

When a current of sulphuretted hydrogen is passed through the mercury compound suspended in water, it becomes rapidly black, a peculiar smell is observed, along with that of sulphuretted hydrogen; and by distillation an oil passes over, which is obtained floating on the surface of the water. It is perfectly transparent and colourless. Its smell is peculiar, and resembles the nauseous odour developed by crushing some umbelliferous plants. When dissolved in alcohol, it gives with corrosive sublimate a white precipitate, soluble in hot alcohol, from which it is deposited in crystals precisely similar to those from which it had been originally separated, and with bichloride of platinum a yellow precipitate, slightly soluble in

hot alcohol and æther. This oil is in all probability the sulphuret of odmyle  $C_8 H_8 S_2$ ; but the small quantity in which I have been able to obtain it, has prevented my performing any analysis of it.

*The Platinum Compound.*—When a solution of bichloride of platinum is added to the alcoholic solution of the crude oil, a yellow precipitate makes its appearance, which does not fall immediately, but goes on gradually increasing for some time, precisely as is the case with the allyle compound. The properties of this precipitate are not however perfectly constant, but vary according to the portion of the oil employed to yield it. That obtained from the more volatile portion has a fine sulphur-yellow colour, but the less volatile oil gives an orange precipitate. It is insoluble in water, sparingly soluble in alcohol and æther. When heated it becomes black, an oil is evolved smelling exactly like that obtained from the mercury compound, and sulphuret of platinum is left behind, which requires a high temperature to drive off all its sulphur, and leaves metallic platinum as a silver-white mass. When treated with hydrosulphuret of ammonia, it is converted into a brown powder, exactly like that obtained under similar circumstances from allyle.

The analysis of the yellow compound has not hitherto given results of a satisfactory character. I have found the amount of platinum to oscillate between 43·06 and 49·66 per cent. The former of these was obtained from the most volatile oil, the latter from that which boiled between  $300^{\circ}$  and  $400^{\circ}$  F., and intermediate results were obtained at intermediate temperatures. The results obtained from the oil which boiled at a high temperature were remarkably constant; thus I have found, in different experiments, 49·00, 49·51, and 49·66 per cent. of platinum, which appear to indicate the presence of some compound of rather sparing volatility. The precipitate obtained from the most volatile oil appears to be that corresponding to the mercury compound which has just been described. Of it I have been able only to perform a very incomplete analysis, which is insufficient to establish its constitution, especially as it is impossible to ascertain whether it is a homogeneous substance. As the results, however, approximate to a formula analogous to that of the mercury compound, I give the details, such as they are.

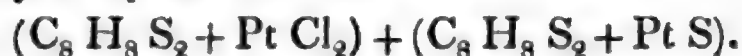
{	9·155 grains of the platinum compound gave
	7·474     ...     carbonic acid, and
	3·294     ...     water.

5·701 grains gave 2·455 grains of platinum = 43·06 per cent.



172 *On certain Products of Decomposition of the Fixed Oils.*

These results approximate to a formula similar to that of the mercury compound; viz.—



Experiment.			Calculation.		
Carbon	.	22·26	20·83	$C_{16}$	1200·0
Hydrogen	.	3·99	3·47	$H_{16}$	200·0
Platinum	.	43·06	42·84	$Pt_2$	2466·6
Chlorine	.	...	15·38	$Cl_2$	885·3
Sulphur	.	...	17·48	$S_5$	1005·8
			100·00		5757·7

The analogy which those substances bear to allyle is exceedingly interesting, as showing the possibility of forming, by artificial processes, substances similar in constitution to so remarkable a compound, which is not a product of decomposition, but exists ready-formed in a variety of different vegetables, where it must obviously be produced under circumstances very different from the artificial substance; for allyle cannot exist at all at a high temperature, and is entirely decomposed at, or even below, its point of ebullition. Unfortunately, however, the examination of this substance is much complicated by the necessity of examining its compounds in place of itself. Had it been possible to separate it directly from the crude oil, the determination of its constitution and that of its compounds would have presented comparatively little difficulty, and been arrived at with much less labour than that expended upon the imperfect details I have been able to accumulate. Another point worthy of observation, is the total alteration of the products of decomposition of oleic acid produced by the presence of sulphur; no sebacic acid, and, in fact, none of its ordinary products being evolved, although all the substances produced contain carbon and hydrogen in the proportion of equal atoms, just as they exist among the ordinary products,—a circumstance which, taking into consideration the abundant evolution of sulphuretted hydrogen, we certainly should not have anticipated.

The oil which remains after the separation of the mercury compound, likewise contains sulphur as one of its constituents; but I have not yet had time to commence the investigation of this part of the subject. The discussion of it, as well as various other points connected with the compounds already described, I hope to make the subject of a future communication.

XXX. *On the Mechanical Equivalent of Heat, as determined by the Heat evolved by the Friction of Fluids.* By J. P. JOULE, *Secretary to the Literary and Philosophical Society of Manchester*\*.

IN the Philosophical Magazine for September 1845 I gave a concise account of some experiments brought before the Cambridge Meeting of the British Association, by which I had proved that heat was generated by the friction of water produced by the motion of a horizontal paddle-wheel. These experiments, though abundantly sufficient to establish the equivalency of heat to mechanical power, were not adapted to determine the equivalent with very great numerical accuracy, owing to the apparatus having been situated in the open air, and having been in consequence liable to great cooling or heating effects from the atmosphere. I have now repeated the experiments under more favourable circumstances, and with a more exact apparatus, and have moreover employed sperm oil as well as water with equal success.

The brass paddle-wheel employed had, as described in my former paper, a brass framework attached, which presented sufficient resistance to the liquid to prevent the latter being whirled round. In this way the resistance presented by the liquid to the paddle was rendered very considerable, although no splashing was occasioned. The can employed was of copper, surrounded by a very thin casing of tin. It was covered with a tin lid, having a capacious hole in its centre for the axle of the paddle, and another for the insertion of a delicate thermometer. Motion was communicated to the paddle by means of a drum fitting to the axle, upon which a quantity of twine had been wound, so as by the intervention of delicate pulleys to raise two weights, each of 29 lbs., to the height of about  $5\frac{1}{4}$  feet. When the weights in moving the paddle had descended through that space, the drum was removed, the weights wound up again, and the operation repeated. After this had been done twenty times, the increase of the temperature of liquid was ascertained. In the second column of the following table the whole distance through which the weights descended during the several experiments is given in inches. I may observe also that both the experiments on the friction of water, and the interpolations made in order to ascertain the effect of the surrounding atmosphere, were conducted under similar circumstances, each occupying forty minutes.

\* Read before the Mathematical and Physical Section of the British Association at Oxford, and communicated by the Author.

TABLE I.—Friction of Distilled Water.

Nature of experiment.	Total descent of each weight of 29 lbs. in inches.	Mean temperature of the room.	Difference.	Temperature of the water.		Gain or loss of heat.
				Before experiment.	After experiment.	
Friction .....	1268.5	60.839	0.040—	60.452	61.145	0.693 gain.
Interpolation	0	61.282	0.120—	61.145	61.180	0.035 gain.
Friction .....	1266.1	61.007	0.408+	61.083	61.748	0.665 gain.
Interpolation	0	61.170	0.570+	61.752	61.729	0.023 loss.
Friction .....	1265.8	57.921	0.809—	56.752	57.472	0.720 gain.
Interpolation	0	58.119	0.628—	57.472	57.511	0.039 gain.
Friction .....	1265.4	58.152	0.293—	57.511	58.207	0.696 gain.
Interpolation	0	58.210	0.003+	58.207	58.219	0.012 gain.
Friction .....	1265.1	57.860	0.215+	57.735	58.416	0.681 gain.
Interpolation	0	58.162	0.256+	58.416	58.420	0.004 gain.
Friction .....	1265.3	57.163	0.220+	57.050	57.716	0.666 gain.
Interpolation	0	57.602	0.121+	57.716	57.731	0.015 gain.
Friction .....	1265.2	57.703	0.359+	57.731	58.393	0.662 gain.
Interpolation	0	58.091	0.304+	58.393	58.397	0.004 gain.
Friction .....	1262.4	56.256	0.015—	55.901	56.582	0.681 gain.
Interpolation	0	56.888	0.285—	56.590	56.617	0.027 gain.
Friction .....	1262.3	57.041	0.078—	56.617	57.310	0.693 gain.
Interpolation	0	57.612	0.285—	57.310	57.344	0.034 gain.
Mean friction experiments }	1265.13	.....	0.0037—	.....	.....	0.6841 gain.
Mean of the interpolations }	0	.....	0.0071—	.....	.....	0.0163 gain.
Corrected result .....	1265.13	.....	.....	.....	.....	0.6680 gain.

We see then that the weights of 29 lbs., in descending through the altitude of 1265.13 inches, generated 0.668 in the apparatus. But in order to reduce these quantities, it became necessary in the first place to ascertain the friction of the pulleys and that of the twine in unwinding from the drum. This was effected by causing the twine to go once round a roller of the same diameter as the drum, working upon very fine pivots, the two extremities of the twine being thrown over the pulleys. Then it was found that, by adding a weight of 3150 grains to either of the two weights, the friction was just overcome. The actual force employed in the experiments would therefore be 406000 grs. — 3150 grs. = 402850 grs. through 1265.13 inches, or 6067.3 lbs. through a foot.

The weight of water being 77617 grs., that of the brass paddle-wheel 24800 grs., the copper of the can 11237 grs.,

and the tin casing and cover 19896 grs., the whole capacity of the vessel and its contents was estimated at  $77617 + 2319 + 1056 + 363 = 81355$  grs. of water. Therefore the quantity of heat evolved in the experiments, referred to a pound of water, was  $7^{\circ}7636$ .

The equivalent of a degree of heat in a pound of water was therefore found to be 781.5 lbs. raised to the height of one foot.

I now made a series of experiments in which sperm oil was substituted for the water in the can. This liquid, being that employed by engineers as the best for diminishing the friction of their machinery, appeared to me well-calculated to afford another and even more decisive proof of the principles contended for.

TABLE II.—Friction of Sperm Oil.

Nature of experiment.	Total descent of each weight of 29 lbs. in inches.	Mean temperature of the room.	Difference.	Temperature of the oil.		Gain or loss of heat.
				Before experiment.	After experiment.	
Friction .....	1263.8	56.677	0.453+	56.354	57.906	1.552 gain.
Interpolation	0	57.316	0.595+	57.906	57.917	0.011 gain.
Friction .....	1269.0	56.198	1.024+	56.516	57.929	1.413 gain.
Interpolation	0	56.661	1.221+	57.920	57.836	0.093 loss.
Friction .....	1268.7	57.958	0.588+	57.813	59.280	1.467 gain.
Interpolation	0	57.051	0.773+	57.836	57.813	0.023 loss.
Friction .....	1268.5	58.543	1.685—	55.951	57.766	1.815 gain.
Interpolation	0	57.153	1.504—	55.568	55.731	0.163 gain.
Friction .....	1268.1	59.097	0.534—	57.766	59.361	1.595 gain.
Interpolation	0	57.768	1.927—	55.731	55.951	0.220 gain.
Friction .....	1268.3	56.987	0.186—	56.029	57.573	1.544 gain.
Interpolation	0	57.156	0.413+	57.573	57.563	0.008 loss.
Friction .....	1268.7	57.574	0.734+	57.581	59.036	1.455 gain.
Interpolation	0	57.336	0.237+	57.565	57.581	0.016 gain.
Friction .....	1267.6	58.537	0.820—	56.884	58.532	1.648 gain.
Interpolation	0	59.641	0.384+	60.026	59.984	0.042 loss.
Friction .....	1268.0	59.131	0.148+	58.532	60.026	1.494 gain.
Interpolation	0	60.164	0.138—	59.984	60.069	0.085 gain.
Mean friction experiments	} 1267.85	.....	0.034+	.....	.....	1.5537 gain.
Mean of the interpolations	} 0	.....	0.004+	.....	.....	0.0366 gain.
Corrected result .....	} 1267.85	.....	.....	.....	.....	1.5138 gain.



In this instance, the force employed, corrected as before for the friction of the pulleys, was equal to raise 6080·4 lbs. to the height of one foot.

In estimating the capacity for heat of the apparatus, it was necessary in this instance to obtain the specific heat of the sperm oil employed. For this purpose I employed the *method of mixtures*. 43750 grs. of water were heated in a copper vessel weighing 10403 grs. to  $82^{\circ}\cdot697$ . I added to this 28597 grs. of oil at  $55^{\circ}\cdot593$ , and after stirring the two liquids together, found the temperature of the mixture to be  $76^{\circ}\cdot583$ . Having applied to these data the requisite corrections for the cooling of the liquids during the experiment, and for the capacity of the copper vessel, the specific heat of the sperm oil came out 0·45561. Another experiment of the same kind, but in which the water was poured into the heated oil, gave the specific heat 0·46116. The mean specific heat was therefore 0·45838.

The weight of oil employed was 70273 grains, and the paddle, can, &c. were the same as employed in the first series of experiments; consequently the entire capacity in this instance will be equivalent to that of 35951 grs. of water. The heat evolved was therefore  $7^{\circ}\cdot7747$  when reduced to the capacity of a pound of water.

Hence the equivalent deduced from the friction of sperm oil was 782·1, a result almost identical with that obtained from the friction of water. The mean of the two results is 781·8\*, which is the equivalent I shall adopt until further and still more accurate experiments shall have been made.

XXXI. *Letter from Prof. SCHœNBEIN to Prof. FARADAY, F.R.S., on a new Test for Ozone†.*

MY DEAR FARADAY,

HAVING a good opportunity for sending you a few lines, I will make use of it to tell you something about my little doings. You are no doubt struck with the peculiarity of the ink in which this letter is written, and I am afraid you will think it a very bad production; but in spite of its queer colour, you will like it when I tell you what it is, and when I

\* This number is slightly different from 775, the equivalent stated at Oxford, and used by me as one of the data for calculations on the velocity of sound. The reason of the difference was that by an oversight I had taken the friction of *both* pulleys as the correction of each weight instead of both weights. The whole of the experiments are exactly the same as those presented to the Oxford meeting. The slight alteration in the equivalent will make only a very trifling alteration in the theoretical velocity of sound given in the last Number of this Magazine.

† Communicated by Professor Faraday.

assure you that as long as the art of writing has been practised no letter has ever been written with such an ink. Dealing now again in my ozone business, I found out the other day that all manganese salts, be they dissolved or solid, are decomposed by ozone, hydrate of peroxide of manganese being produced and the acid set at liberty. Now to come round again to my ink, I must tell you that these lines are written with a solution of sulphate of manganese. The writing being dry, the paper is suspended within a large bottle, the air of which is strongly ozonized by means of phosphorus. After a few minutes the writing becomes visible, and the longer you leave it exposed to the action of ozone the darker it will become. Sulphurous acid gas uniting readily with the peroxide of manganese to form a colourless sulphate, the writing will instantly disappear when placed within air containing some of that acid; and it is a matter of course that the writing will come out again when again exposed to ozonized air. Now all this is certainly mere playing; but the matter is interesting in a scientific point of view, inasmuch as dry strips of white filtering paper drenched with a weak solution of sulphate of manganese furnish us with rather a delicate and specific test for ozone, by means of which we may easily prove the identity of chemical, voltaic and electrical ozone, and establish with facility and certainty the continual presence of ozone in the open air. I have turned brown my test-paper within the electrical brush, the ozonized oxygen obtained from electrolysed water and the atmospheric air ozonized by phosphorus. The quantity of ozone produced by the electrical brush being so very small, it requires of course some time to turn the test-paper brown.

As it is rather inconvenient to write with an invisible ink, I will stop here; not however before having asked your kind indulgence for the many blunders and faults which my ozone bottle will no doubt bring to light before long.

Yours most truly,

Bâle, July 1, 1847.

C. F. SCHÖENBEIN.

XXXII. *On the Decomposition of Water by Platinum and the Black Oxide of Iron at a white heat, with some observations on the theory of Mr. Grove's Experiments.* By GEORGE WILSON, M.D.\*

THE remarkable discovery recently made public by Mr. Grove, that water in certain circumstances, when raised to a white heat, is resolved into its constituent gases, has na-

\* Communicated by the Chemical Society; having been read March 15, 1847.

turally excited much attention. It furnished the unexpected confirmation of the truth of an opinion expressed by James Watt so far back as 1783, that if steam could be made red hot [white hot] so that all its latent heat should be converted into sensible heat, either the steam would be converted into permanent air, or some other change would take place in its constitution\*.

In the greater number of Mr. Grove's experiments, water was raised in temperature through the medium of platinum; and it became a question accordingly, as Sir John Herschel and my friend Dr. Lyon Playfair suggested, how far the decomposition of water observed was owing to the mere heat of the metal, how far to the peculiar surface-influence, or so-called catalytic force, which has been so long recognized as possessed by platinum and the other noble metals. Dr. Playfair also referred to the fact, "that many bodies at high temperatures exhibited a great affinity for oxygen, which they did not possess at lower temperatures; as, for instance, silver, gold, and even platinum itself, which metals absorb oxygen when intensely heated, and give it out again on cooling. If the experiments had been tried in tubes of quartz or silica, they would not have been open to the objection which the use of so peculiar a metal as platinum appeared to involve†."

There was indeed one form of Mr. Grove's experiment not liable to the exception urged against those where platinum was used. He found it quite possible to decompose steam by sending Leyden-jar discharges through it, and refers the decomposition solely to the heat evolved by the electric spark. The same view has been suggested as not improbable by Faraday, in relation to the decomposition of water in the liquid form by electric discharges‡. With great diffidence, however, I would remark, that the spark decomposition of water cannot be regarded as an *experimentum crucis*. Although the electric spark cannot decompose steam electrolytically, we may not at once infer that it cannot decompose it in another way. I have no wish to assert that it can, but it is possible that it may, and a crucial experiment should be unexceptionable. Again: the spark discharge of a Leyden jar exerts a great disruptive force, and acts topically with much violence. There is reason moreover to believe that mechanical agitation or disturbance of a chemical compound can in many cases cause the separation of its elements. It may seem an extravagant idea to suppose that oxygen may be torn or detached from hydrogen by the action of a dis-

\* Phil. Trans. 1783, p. 416.

† Athenæum for September 19th, 1846, p. 966.

‡ Researches in Electricity, 3rd series, paragraph 337.

ruptive force on the molecules of water, as if chemical affinity were but a kind of mechanical cohesion, which may be overcome by division. On the other hand, however, it must not be forgotten, that we are now acquainted with a large number of fulminating compounds, which can be decomposed by friction, by a touch, or a stroke. These compounds are all fragile, and water is a very stable combination; but fragility and stability are but terms of degree, in relation to stability of union: and if it shall appear that a feeble mechanical force can overcome a small intensity of affinity, it will be acknowledged as quite possible that a powerful mechanical agency may overcome a great one. We have no means perhaps of making an unexceptionable experiment as to the decomposing power of mechanical force; for we cannot bring it into play without calling into action other agencies. If we touch, or rub, or strike a fulminate, for example, we cause the evolution of heat, and add its decomposing power to that of the mechanical impulse. It would be a mere *petitio principii*, however, to assume that the heat produced alone effects the decomposition observed. It seems to me, therefore, that the decomposition of steam by the electric spark furnishes a more complex problem for solution than the action of white-hot platinum on the same compound does; and that the experiments made with the metal are more likely to throw light on those tried with the spark, than to be explained by them.

Whilst thinking over these difficulties, and the objections to Mr. Grove's conclusions suggested by Herschel and Playfair, I had occasion to perform the familiar class-experiment of burning iron wire in oxygen. I observed with an interest I had not felt previously, although I had carelessly noticed the phænomenon before, that bubbles of apparently permanent gas rose from the globules of white-hot oxide of iron as they fell into the water. It seemed to me possible that this gas might be a mixture of oxygen and hydrogen separated by the influence of the metallic oxide, acting as platinum did in Mr. Grove's experiments. It was certain, moreover, that if this should prove to be the case, it would supply a powerful argument in favour of that gentleman's conclusion, which seems, in spite of all the objections noticed, in the highest degree probable, namely, that heat, apart altogether from the medium through which it is applied, can resolve water into its elements.

As the following experiments were made solely in the hope of substantiating Mr. Grove's view, which unfortunately, however, they leave exactly as they found it, I trust that gentleman will not consider their publication an interference with his researches. I was led to try them incidentally, and



abandoned them as soon as I found I could render Mr. Grove no assistance by means of them.

It would be difficult to conceive a more rapid and effectual way of raising a body to a white heat than that afforded by the combustion of iron in oxygen. I took for granted also (as it afterwards appeared, too hastily) that the metal could not but be saturated with oxygen and converted into a definite oxide, which would be chemically indifferent to each of the elements of water, and if it decomposed it at all, would reject both its constituents. The convenient way, moreover, in which the globules of oxide detach themselves and fall into the water, and the rapidity with which the whole process goes on, make it a very easy matter to collect in considerable quantity whatever gases are evolved. A stoppered bottomless jar of the ordinary construction for the iron-wire experiment, and of 291 cubic inches' capacity, was made use of in the following trials. Eighteen experiments were made with it, and from 100 to 110 grains of fused globules were obtained from each combustion. A test-tube, with a funnel fixed into it by a perforated cork, and filled with water, was arranged so as to receive the gas. In some experiments it was placed within the oxygen jar, so that the coil of wire when introduced hung close to it, a piece of tin plate being arranged so as to guide the globules within the edge of the inverted funnel. In the greater number of trials however the tube and funnel were placed outside of the vessel containing the oxygen, and an inclined plane of tin plate was so placed as to carry the globules past the edge of the jar, and within the mouth of the funnel. No difference of result was observed in experiments made in both ways, but the latter arrangement was preferred as more convenient, and as enabling more oxygen to be employed at each trial.

In all the experiments, permanent gas was evolved when the fused globules fell into the water. This statement is to be considered as applying to each combustion considered as a whole; for individual globules were frequently observed to give off no gas at all, or to evolve so very little, that it might be air separating from the water, in which it had previously existed in solution. The quantity of gas obtained at each combustion varied greatly. Sometimes as much as a cubic inch was procured, more frequently only half that quantity, and occasionally less. The globules from thick coils of wire gave off a larger volume of gas than those from thin ones.

Portions of the gas were transferred to a Grove's eudiometer over water, and exposed to a white-hot platinum wire. They did not kindle or detonate, nor were they sensibly diminished in volume. Other portions were subjected to

electric sparks and discharges in a syphon eudiometer over water, with the same negative results; but when air or oxygen was mingled with the gas, it exploded sharply with heated platinum or the electric spark. When a match was applied to the open end of a tube containing the unmingled gas, it burned rapidly with a pale blue flame, but did not explode. The gas given off during the action of the fused globules on water was not then a mixture of oxygen and hydrogen.

Its freedom from all but a trace of oxygen was ascertained in other ways. To one portion of the gas standing over water nitric oxide was added, but no ruddy fume or yellow coloration showed itself. When phosphorus was introduced into the gas, in one instance it did not smoke, but in the greater number of cases it fumed for a brief period, and occasioned an amount of contraction barely perceptible. The gas appeared to be nearly pure hydrogen. To ascertain if it certainly were so, a portion of it was carefully dried, by chloride of calcium, and transferred to a eudiometer over warm mercury. Dry oxygen was then added, and the mixture exploded. When the whole had cooled, the walls of the eudiometer appeared dimmed by a very thin layer of moisture, but the quantity of gas operated on was too small to admit of visible drops being produced. Another portion of the gas was mixed with half its volume of oxygen and fired by the electric spark. The contraction which followed explosion varied in different experiments, but was frequently such as to leave not more than one-twentieth part of the mixed gases unconsumed. Phosphorus smoked in this residue for a short time, showing that excess of oxygen had been made use of, and left a minute volume of gas which was not diminished by caustic potash, and must have been nitrogen.

It seemed possible that the trace of carbon present even in malleable iron might affect the quality of the gas resulting from the action of the globules of oxide on water, and that carburetted hydrogen, carbonic oxide or carbonic acid might be produced. It seemed desirable to know whether the latter were present or not, as the oxygen might have gone to form them. It was impossible to be certain that carbonic acid was absent, for the gas from the globules being necessarily collected over water, the temperature of which was low, carbonic acid would be retained in solution by that liquid. All that I can say on this point is, that lime-water was not rendered muddy or in the slightest degree opalescent by the gas. It was several times detonated with oxygen over lime-water, but the latter remained quite transparent, so that neither carbonic oxide nor carburetted hydrogen can have been present. In short, the gas evolved from water by the white-

hot globules of oxide of iron, was hydrogen mingled with a small quantity of air, previously no doubt in solution in water.

As only the hydrogen, then, of the water decomposed was obtained, it became necessary to account for the absence of the oxygen. I was tempted for a moment to think it possible that the black oxide of iron might have changed into the red oxide of the same metal, by combining with the oxygen not obtained in the elastic form: *ex. gr.* thus  $2 \text{Fe}_3 \text{O}_4 + \text{O} = 3 \text{Fe}_2 \text{O}_3$ .

But the proto-peroxide of iron is known to be a very stable compound, little if at all prone to become the peroxide; and it seemed more likely that unoxidized iron might be present in the fused globules, which occasioned the evolution of hydrogen when it came in contact with water. To ascertain this point, portions of the globules were dissolved in dilute muriatic and sulphuric acids, and were found in most cases to evolve hydrogen. Some specimens of the globules gave off not a trace of gas when they dissolved, and must have consisted of the definite oxide; a point of interest in connection with the fact already mentioned, that globules were frequently observed to drop into water without any bubbles of gas rising from them.

The volume of hydrogen however given off in some of the trials, when the product of combustion was placed in acid, was very considerable. A graduated gas jar was filled with dilute sulphuric acid, and inverted over a small capsule containing 100 grains of the crushed globules, which was placed in a basin also containing dilute acid. By this arrangement the gas was collected and measured at the same time, without risk of mixing with air, or necessity for watching the process, which is a slow one. 100 grains treated in this way gave off 16 cubic inches of hydrogen, corresponding to 9 grains of iron. The experiment was accidentally stopped at this point whilst the gas was still rising in undiminished quantity.

Metallic iron, then, was certainly present in many of the globules, and of this I had direct ocular demonstration. On crushing some of them in a mortar, they were found to separate into a shell of pulverizable oxide, and a core of iron which formed a nearly spherical pellet. In one case 50 grains of the globules were crushed, the pellets separated, and the residue placed in diluted sulphuric acid. It did not evolve a trace of hydrogen in the course of twenty-four hours. The pellets were then added to the same acid, and gave off 12 cubic inches of gas = 13.6 per cent. of iron in the globules\*. The shell of oxide is frequently imperfect or perforated, so

\* In none of the experiments was the thermometer or barometer specially observed, as minute accuracy was not aimed at.



that water or any other liquid penetrates to the iron core, and is subject to its influence. When this becomes known, it need not surprise us that most of the globules should rapidly decompose water. After observing this fact, I tried the effect of thin and thick coils of wire, and found that the latter invariably gave off the greater volume of gas. When the coil is so thin that the metal all oxidizes, no gas is evolved at all. A thick coil indeed furnishes a striking mode of illustrating to a class the principle of Lavoisier's mode of decomposing water, and forms a beautiful addition to the iron-wire experiment.

From these observations then, it would seem that white-hot oxide of iron cannot decompose water in the way white-hot platinum does. But before any conclusion can be drawn from this fact inimical to Mr. Grove's views, or favourable to the opinion that a specific property of the platinum has more to do with the decomposition of water than its mere temperature has, we should require to know how far the two white-hot bodies are to be considered as at the same temperature. In Mr. Grove's experiments, platinum is raised to as high a heat as it can bear without fusing. It must then be elevated to a temperature much above that necessary to make iron white hot, or to fuse its oxide, for our forges can melt iron and its oxides, but do not fuse platinum. It may also be remarked, that bright as the light emitted by burning iron is, it falls short in intensity of that given off by platinum on the verge of fusion. It seems accordingly probable, that during the combustion of iron in oxygen the temperature never rises high enough to confer upon the resulting oxide the power of decomposing water. The question admits of direct decision, by ascertaining whether oxide of iron, heated by the oxy-hydrogen blowpipe to as high a temperature as fusing platinum, acquires the power of decomposing water without appropriating to itself either of its elements. But it would have been an interference with Mr. Grove's own researches to have made experiments of this kind, and I have accordingly left the question undecided.

Meanwhile the experiments I have recorded are of some little interest, as at least showing that not only a white heat, but a high white heat, is essential to the successful performance of Mr. Grove's experiments. Unfortunately, we have not at present any method of measuring high temperatures which admits of ready application or secures great accuracy. "White heat" is in fact a vague expression for a range of temperature, of the extremes in either direction or extent of which we have no very precise knowledge. The power of the eye to measure the relative intensities of the light evolved



by white-hot bodies is very limited, and varies greatly in different individuals. But the experiments I have recorded seem to supply the means of so far at least defining the white heat requisite for the separation of the elements of water, inasmuch as they show that it must at least exceed the temperature necessary for the fusion of malleable iron or its black oxide. If, moreover, the decomposing powers of the electric spark be solely referable to its temperature, we seem entitled to conclude, from the experiments I have detailed, that the heat of the smallest spark that can decompose water is at least equivalent to that of fusing platinum. They appear also to warrant another conclusion. It was suggested by Dr. Leeson and by Mr. Hunt, that the bursting of steam-boilers might occasionally be owing to the metal they consist of becoming white-hot and decomposing water like platinum, with the rejection of both its elements\*. This ingenious suggestion seemed to myself, before making experiments with iron, likely to prove just; but as fusing white-hot iron appears unable to decompose water, otherwise than by combining with its oxygen, it is impossible that the walls of a boiler can ever be raised to a temperature sufficiently high to enable them to separate the elements of water in the way platinum does.

I may now be permitted to make some comments on the rationale of the results obtained by Mr. Grove. That gentleman, if I understand him aright, considers the decomposition of water by white-hot platinum not only, as assuredly it is, a remarkable and unexpected result, but as evidencing on the part of heat a power to produce opposite or dissimilar chemical effects in the same circumstances. He is reported in the *Athenæum* (Sept. 19th, 1846, p. 966) to have "announced his discovery that all the processes by which water may be formed are capable of decomposing water" (p. 966). If by this statement be simply meant, that heat combines oxygen and hydrogen into water, and decomposes water into these gases, it will be admitted to be a just conclusion; but it may be questioned, I think, whether Mr. Grove's experiments add anything to our knowledge of the power of heat to effect chemical changes, except in so far as they supply an additional very remarkable example of its twofold analytical and synthetical agency, which has been so long recognised. Hydrogen, which as a gas is probably the vapour of a very volatile metal, may be compared with mercury, also a volatile substance. If mercury and oxygen be heated together to the temperature of  $662^{\circ}$  F., they combine and form the red oxide of the metal. If this resulting oxide be raised to a low red heat,

\* *Athenæum*, Sept. 19th, p. 966.

it is decomposed into mercury and oxygen. In like manner, if hydrogen and oxygen be raised together to the temperature of  $660^{\circ}$  F.\*, they unite and form water. If the resulting water be raised to a white heat, it is resolved into hydrogen and oxygen. Both metals (?) present the same phænomena. At one temperature (nearly the same in both cases) combination with oxygen occurs; at a higher temperature, decomposition of the oxide happens. Many other examples might be given in illustration of the same fact. Such cases, however, do not seem to warrant a conclusion as to heat exhibiting anything like a polarity of force, by which I understand the manifestation in opposite directions of opposite powers of equal intensity. At all events, if the opposite effects of different *intensities* of the same agent be considered equivalent to a polarity of action, it is difficult to see what force may not be called a polar one. The decomposing and combining power of heat of different intensities, seems exactly comparable to the opposite effects of different intensities of mechanical impulse.

If two pieces of smooth glass are laid together and struck gently or compressed slightly, they unite or cohere. If the united pieces are thereafter exposed to a sharp blow or to great compression, the union is dissolved, or they are shattered to fragments. Here the same force effects mechanical synthesis and mechanical analysis. But in these contrasted actions, as seems to be the case also in Mr. Grove's experiments, the results are occasioned by a difference in degree of intensity of the same power, not as in the opposite effects of a polarizing force like electricity, by a difference in the kind of power which appears, whatever be its intensity. There is one form, indeed, of Mr. Grove's experiment which at first sight does not appear to admit of the explanation proposed in reference to the other trials—I allude to the decomposition of steam by the electric spark, which is well known to have the power of combining hydrogen and oxygen into water. A similar experiment was made in perhaps a still more instructive form in the latter part of last century by Beccaria†, Pearson and Van Troostwyk, and more recently by Wollaston‡, in his well-known decompositions of water with guarded poles. In certain of these trials it was found that Leyden jar discharges sent through water, decomposed it till the accumulation of permanent gas left the wires bare; after which the first spark that passed recombined the gases into water, which again covered the wire, when decomposition could

\* Graham's Elements, 1st edit. p. 259.

† Lettere dell' Elettrecismo, quoted in Lardner's Electricity, vol. i. p. 78.

‡ Faraday's Electrical Researches, series 3, paragraph 328.

anew be obtained. Here, to appearance, the same agent acting with the same intensity, alternately decomposed and re-composed water. For argument's sake, let it be acknowledged that the heat alone of the spark was the cause of chemical change. Nevertheless it may be questioned, whether it acted with equal intensity in both cases. The electric spark must be conceived, according to the results already given, to be at first at a high white heat, and whilst retaining this temperature we may believe it to possess a power of disuniting the elements of water, and of preventing their union. But as soon as the spark falls to the temperature of  $660^{\circ}$  F., it loses its power of decomposing water, and, on the other hand, acquires a power of uniting hydrogen and oxygen. Although therefore the spark is always *furnished* of the same intensity, its action may change, and even be reversed, as its intensity diminishes. Moreover, even when the spark is white-hot, it is only the amount of matter directly in its track that will be raised to a white heat. Contiguous portions will have their temperature much lower, so that in the case of hydrogen and oxygen, at some little distance from the route of the spark, the temperature will be  $660^{\circ}$  F., and there combination will begin, and ultimately extend through the whole mass of gas.

In like manner, when a platinum wire is made white-hot in a mixture of hydrogen and oxygen, it causes their combination. Here we may suppose that union occurs as soon as the temperature of the metal rises to  $660^{\circ}$  F., and before it acquires a white heat. Or if we were to arrange matters so that the wire should be made white-hot in a vacuum and hydrogen and oxygen afterwards admitted to it, still union of the gases should happen; for although the wire might prevent combination immediately around itself, at no great distance where the temperature was below  $700^{\circ}$  F. it would compel union. In all such experiments the combining effect of heat will be much more manifest than its decomposing power; not that perhaps the former is in reality greater than the latter, but because flame is propagated through a mixture of hydrogen and oxygen by a series of combustions. The hot wire or the electric spark kindles only the portions of gas immediately adjacent to it, but the combustion of those sets fire to the molecules contiguous to them, and these in their turn to their neighbours, till all are made to burn. Thus the flame travels after the original cause of combustion has ceased to operate directly, and the momentary action of a small spark, or the transient heat of a red-hot capillary wire may suffice to fire an infinitely large mass of hydrogen and oxygen. There is no provision for a similar propagation of decomposition through water or steam when either is made white-hot; the



absolute amount accordingly of disunion of the elements of water occasioned is very small.

If allowance, however, be made for the apparent difference in extent of effect which heat shows in uniting and in disuniting the elements of water, the phenomena otherwise seem referable solely to the intensity of the temperature to which hydrogen and oxygen are exposed. The opposite processes might go on simultaneously, union or disunion being determined simply by the different temperatures to which different portions of the gases were raised. At least it seems not improbable that if a mixture of steam and of hydrogen and oxygen were exposed to electric discharge, decomposition of the steam and combination of the hydrogen and oxygen might be effected by the same spark, provided the volume of steam were not large. In the track of the spark decomposition would occur, so long as a white heat prevailed. When the temperature fell, combination would happen where the spark had passed, if it had not already commenced in the neighbourhood of its direct route. Similar remarks apply *mutatis mutandis* to the action of a hot platinum wire on a mixture of steam with oxygen and hydrogen.

It may be objected to this view, that Mr. Grove decomposes steam in his eudiometer, and obtains a permanent bubble of gas, consisting of hydrogen and oxygen. The bubble however obtained in this way is very small, and could not probably be greatly increased. Mr. Grove has not mentioned how large a volume of hydrogen and oxygen he could obtain in the same eudiometer, by alternately boiling the water till the steam produced caused the liquid to fall below the wire, and allowing the steam to condense till the water rose above the metal. But I venture to say that no large volume of permanent gas could be procured by this process if the same eudiometer were employed many times successively. The combining action of the wire might not take effect on the hydrogen and oxygen when their quantity was small, and they were diluted through a large volume of steam, for in virtue of the law of diffusion, the molecules of hydrogen and oxygen would be separated from each other by molecules of water-vapour; but when the latter diminished in bulk, it seems impossible to doubt that kindling of the gases would occur.

Mr. Grove's experiments then do not appear to prove that heat of the same intensity is able in the same circumstances to form water and to decompose it. When therefore it is stated that water can be produced by the processes that disunite its elements, the word 'process' can only be understood to signify that the general arrangement in both cases is the



same, not that the intensity of the agent called into play, or its mode of action is identical. If this could be affirmed, we should be able to announce as a general proposition, that manifestations of the same force absolutely identical as to quality, quantity and intensity, could produce totally opposite results, which would be tantamount to affirming that unlike effects may flow from the same cause, without any alteration in the qualities or conditions of the latter.

The last observation I would make refers to the curious fact noticed by Mr. Grove, namely, that when a platinum wire is heated white-hot in steam, "in a few seconds a small bubble of gas is formed; but if the action be continued for a week, it does not increase in quantity\*."

Are we to suppose that the wire is at the same time decomposing water around itself, and producing water at a little distance, undoing in one place what it effects in another, so that no permanent accumulation of gas is allowed to take place? This is possible, but I think not likely. The observation made by Mr. Grove seems sufficiently explicable, on the supposition that as soon as the wire is completely enveloped in steam, the thermo-circulatory currents which the high temperature occasions in the vapour prevent it from remaining long enough in contact with the wire to become heated white-hot. The steam probably circulates endlessly around the wire without a trace of decomposition occurring in it. It seems not unlikely indeed that in Mr. Grove's experiments with his eudiometer it was not steam that yielded the hydrogen and oxygen obtained, but the last film of water below the wire, which could not escape from the metal, but tended rather, in consequence of its expansion, to rise towards it, and was thus compelled to acquire a white heat, and to break up into its elements. If this view be correct, an arrangement where a white-hot wire or sheet of platinum foil was kept grazing the surface of water, might be found to effect a continuous decomposition of the liquid in question.

It is no objection to this view that an electric spark decomposes steam readily, for the duration of the spark is so short, that there is no time for the production of thermo-currents, nor any possibility of the steam escaping from the powerful topical action of the discharge. The spark may be compared to fulminating silver, whose action is instantaneous and violent, but quite local,—the heated platinum to gunpowder, the effect of which is cumulative and more general.

\* *Athenæum*, Sept. 19th, 1846, p. 966.

XXXIII. *An account of a Discovery in the Theory of Numbers relative to the Equation  $Ax^3 + By^3 + Cz^3 = Dxyz$ . By J. J. SYLVESTER, Esq., M.A., F.R.S.\**

*First General Theorem of Transformation.*

**I**F in the equation  $Ax^3 + By^3 + Cz^3 = Dxyz$  . . . (1.)

A and B are equal, or in the ratio of two cube numbers to one another, and if  $27ABC - D^3$  (which I shall call the Determinant) is free from all single or square prime positive factors of the form  $6n + 1$ , but without exclusion of *cubic* factors of such form, and if A and B are each odd, and C the double or quadruple of an odd number, or if A and B are each even and C odd, then, I say, the given equation may be made to depend upon another of the form

$$A'u^3 + B'v^3 + C'w^3 = D'xyz;$$

where

$$A'B'C' = ABC$$

$$D' = D$$

$$u \cdot v \cdot w = \text{some factor of } z.$$

The following are some of the consequences which I deduce from the above theorem. In stating them it will be convenient to use the term *Pure Factorial* to designate any number into the composition of which no single or square prime positive factor of the form  $6n + 1$  enters.

The equations

$$\begin{aligned} x^3 + y^3 + 2z^3 &= Dxyz \\ x^3 + y^3 + 4z^3 &= Dxyz \\ 2x^3 + 2y^3 + z^3 &= Dxyz \end{aligned}$$

are insoluble in integer numbers, provided that the Determinant in each case is a *Pure Factorial*.

The equation  $x^3 + y^3 + Az^3 = 9Bxyz$  is insoluble in integer numbers, provided that the Determinant, for which in this case we may substitute  $A - 27B^3$ , is a pure factorial whenever A is of the form  $9n \pm 1$ , and equal to  $2p^{3i \pm 1}$  or  $4p^{3i \pm 1}$ , p being any prime number whatever.

I wish however to limit my assertion as to the insolubility of the equations above given. The theorem from which this conclusion is deduced does not preclude the possibility of two of the three quantities  $x, y, z$  being taken positive or negative *units*, either in the given equation itself or in one or the other of those into which it may admit of being transformed. Should such values of two of the variables afford a particular solution, then instead of affirming that the equations are insoluble, I should affirm that the *general solution* can be obtained by equations in finite differences†.

\* Communicated by the Author.

† Take for instance the equation  $x^3 + y^3 + 2z^3 = 9xyz$ . The Determinant

*Second General Theorem of Transformation.*

The equation  $f^3x^3 + g^3y^3 + h^3z^3 = Kxyz$  . . . (2.)  
may always be made to depend upon an equation of the form

$$Au^3 + Bv^3 + Cw^3 = Duvw,$$

where

$$ABC = R^3 - S^3$$

$$D = 3R;$$

and  $u, v, w =$  some factor of  $fx + gy + hz$ .

$$R \text{ representing } K + 6fgh$$

$$S \quad \dots \quad K - 3fgh.$$

I have not leisure to show the consequences of this theorem of transformation in connexion with the one first given, but shall content myself with a single numerical example of its applications:

$$x^3 + y^3 + z^3 = -6xyz$$

may be made to depend on the equation

$$u^3 + v^3 + w^3 = 0,$$

and is therefore insoluble.

It is moreover apparent that the Determinant of equation (2.) transformed is in general  $-27R^3$ , and is therefore always a Pure Factorial, and consequently the equation

$$f^3x^3 + g^3y^3 + h^3z^3 = Kxyz$$

will be itself insoluble, being convertible into an insoluble form, provided that  $K + 6fgh$  is divisible by 9, and provided further that  $(K + 6fgh)^3 - (K - 3fgh)^3$  belongs to the form  $m^3 \cdot Q$ , where  $Q$  is of the form  $9n \pm 1$ , and also of one or the other of the two forms  $2p^{3i \pm 1}$ ,  $4p^{3i \pm 1}$ ,  $p$  being any prime number whatever.

Pressing avocations prevent me from entering into further developments or simplifications at this present time.

It remains for me to state my reasons for putting forward these discoveries in so imperfect a shape. They occurred to me in the course of a rapid tour on the continent, and the results were communicated by me to my illustrious friend M. Sturm in Paris, who kindly undertook to make them known on my part to the Institute.

Unfortunately, in the heat of invention I got confused about 27·25 is a Pure Factorial: consequently if the solution be possible, since in this case the transformed must be identical with the given equation, this latter must be capable of being satisfied by making  $x$  and  $y$  positive or negative units. Upon trial we find that  $x=1$   $y=1$   $z=2$  will satisfy the equation. I believe, but have not fully gone through the work of verification, that these are the only possible values (prime to one another) which will satisfy the equation. Should they not be so, my method will infallibly enable me to discover and to give the law for the formation of all the others.

Here, then, under any circumstances, is an example, the first on record, of the complete resolution of a numerical equation of the third degree between three variables.

the law of oddness and evenness, to which the coefficients of the given equation are in the first theorem *generally* (in order for the successful application of my method as far as it is yet developed) required to be subject. I stated this law erroneously, and consequently drew erroneous conclusions from my Theorems of Transformation, which I am very anxious to seize the earliest opportunity of correcting. I venture to flatter myself that as opening out a new field in connexion with Fermat's renowned Last Theorem, and as breaking ground in the solution of equations of the third degree, these results will be generally allowed to constitute an important and substantial accession to our knowledge of the Theory of Numbers.

26 Lincoln's Inn Fields,  
August 24, 1847.

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XXXIV. *Experiment made at the Kew Observatory on a new Kite-Apparatus for Meteorological Observations, or other purposes\*.*

MR. W. R. BIRT (on the 14th of this month) took some kites, &c. to the Kew Observatory, for the purpose of endeavouring to ascertain how far it might be practicable to measure the force of wind at various elevations by their means, and (in the mere manipulation of his experiments) was assisted by Mr. Ronalds. After several trials, &c. they agreed that the sudden variations, horizontal and vertical, in the position of the kite, the great difficulty of making a kite which should present and preserve a tolerable approximation to a plane, that of measuring, with sufficient accuracy, at any required moment, its inclination, and lastly, the influence of the tail, would always tend to render the observation somewhat unsatisfactory. Mr. Ronalds then proposed to try the following method of retaining a kite in a quasi invariable given position. Three cords were attached to an excellent hexagonal kite of Mr. Birt's construction: one in the usual manner, and one on each side (or wing). The kite was then raised as usual; the two lateral cords were hauled downward by persons standing at the apices of a large equilateral triangle (described upon the ground) until the ascending tendency became considerable (even when the force of the wind was at its minimum), and the three cords were made fast to stakes or held in the hand. He had entertained no expectation of the favourable result of this simple and obvious contrivance. The place of the kite did not seem to vary so much as one foot in any direction, and it really appears to him probable that a very large kite or kites might be employed in this kind of manner *often* and very

\* Communicated by Mr. Ronalds.



cheaply as a substitute for a captive balloon in meteorological inquiries, or even (on a very extensive scale) for other requirements in military science, &c. An anemometer, a thermometer, an hygrometer, &c. of some registering kinds, &c., might be hauled up and lowered at pleasure (like a flag) by a person standing in the centre of the triangle (above referred to), and by means of a line passing through a little block attached to the kite. The cords and kite should of course be of pure silk, for the sake of lightness, combined with extreme strength, and the size and thickness in some measure adapted to the breeze or lighter air. The silk might be advantageously covered with a very light coat of elastic varnish.

XXXV. *On Transformations produced by Catalytic Bodies.*  
By LYON PLAYFAIR, Esq.\*

**B**ERZELIUS rendered a most useful work to science, when he collected into one class those varied phænomena of chemical action resulting from causes certainly very different from the ordinary manifestations of those affinities, which produce combinations or promote decompositions. This philosopher believes the power †, which causes decomposition without the acting body participating in its result, to be a distinct electro-chemical agency different from other recognised powers, and he named it the "Catalytic force." According to this view, catalytic bodies do not act by chemical affinity, but they excite inherent affinities in other substances, in consequence of which new combinations or decompositions ensue.

Mitscherlich ‡, adopting this view, considered a number of catalytic decompositions in detail, and showed the important influence exerted by the state of surface of bodies in favouring this peculiar action, which he denominates decomposition by *contact*. The examples, adduced in this interesting memoir, of the favourable action of an extended surface upon combination, fully prove that the physical condition of bodies exercises an important influence upon the action of this force; but they do not remove the necessity for studying the force itself, as it may either be a *vis occulta*, entirely distinct from powers already recognised, as Berzelius supposes, or may be modified forms of those in continual operation.

Liebig§ views the catalytic power as a dynamical action

\* Communicated by the Chemical Society; having been read April 5, 1847.

† *Jahresbericht*, xv. 237.

‡ Taylor's Scientific Memoirs, Part xiii.; or Pogg. *Ann.* xxxi. 281.

§ Liebig's Chem. of Agriculture, 4th edit., p. 284.

on the atoms of a complex molecule, conceiving that the activity of the atoms of a body in a state of motion may be communicated to those of another body in a state of rest. The atoms of a compound, according to this view, if in a state of exact statical equilibrium, arrange themselves according to new affinities, when the *vis inertiae* is overcome by motion. In proof of this view, Liebig carefully examines a large number of decompositions, and accounts for some of the most difficult transformations in organic chemistry.

But there are many instances, to which I shall have to draw attention in the present memoir, where catalytic decompositions ensue when there is no intestine motion in the atoms of the exciting body; and hence we cannot do more than consider motion as favourable to the development of dormant affinities, in a manner similar to the surface action described by Mitscherlich. The power of peroxide of hydrogen and of pyruvic acid to reduce oxide of silver is certainly a singular phænomenon, and appears favourable to Liebig's views; but the cause of the original decomposition of the peroxide of hydrogen cannot be ascribed to motion, as the atoms of the oxide of silver are not in that state, and those of the peroxide of hydrogen either not at all or only slightly so. Neither will it suffice to suppose that the escape of gas during such decompositions is due to the presentation of angular points from which the gas may escape\*, because solutions of alkalies equally effect the decomposition, according to Thenard†. The cause, therefore, which enables certain substances to hasten the decomposition of such bodies as peroxide of hydrogen or persulphuret of hydrogen, although favoured by the state of surface and by motion, is independent of mere physical condition.

In further proof of the importance of motion in causing combination or decomposition, Liebig cites the favourable effects of agitation on the precipitation of potash by tartaric acid. It may be questioned, however, whether this is not either a mechanical breaking up of a combination or the simple effect of cohesion. Thus when water is saturated with a gas, a brisk agitation with a rod causes the separation of bubbles of gas previously dissolved. The mechanical force may here be supposed to have broken up the compound molecule of water and gas by detaching the former, and thus enabling the gas to escape by its elasticity. In the precipitation of potash by tartaric acid, cohesion may effect the same result that elasticity does in the case of gas, the agitation knocking off the

\* *Ann. der Pharm.*, ii. 22.

† *Ann. de Chim. et de Phys.*, xlviii. 79.

atoms of water which are feebly attached. In fact we know that the addition of alcohol equally aids the precipitation, the action here being a chemical separation of the water, as in the other it is mechanical. The diminished solubility of the salt, after it has been influenced by cohesion and separated from water, has its counterpart in many similar instances; for example, in the small solubility of anhydrous sulphate of iron. The effect of agitation on a solution of sulphate of soda, saturated while hot and allowed to cool, I ascribe to the same cause. The supposed effect of cohesion or elasticity in these cases is nothing more than that constantly observed in ordinary phænomena, when the gravity of a substance is different from that of the medium in which it exists. The vesicles of water in the atmosphere may be so small that they float in it and produce fogs; but when aggregated together by the motion of the air, they form drops, which precipitate to the ground with a rapidity proportionate to their size: the converse of this is also true. Thus, the particles or aggregated atoms of carbonic acid in water may be so very small, that, with the slight affinity of the latter added, they may be enabled, when in a state of rest, to remain without resuming their elastic form; but agitation causes a larger system of aggregated atoms, and the gas now escapes in small bubbles.

The first instance of cohesion applies in the precipitation of tartar. At the moment of formation the particles may be so widely apart, that, aided by their slight affinity for water, they remain without aggregating to any considerable extent. Brisk agitation, and the presentation of an extended surface, effect their aggregation and cause a speedy precipitation. It may be that these are really instances of combination favoured by motion; but presuming that they are, the general argument is not affected, that other decompositions perfectly analogous are produced where the exciting body is in a state of rest.

The third theory of these decompositions is, that catalytic bodies act by exerting a feeble chemical affinity on one of the constituents of the body decomposed. This view was introduced by Mercer\*, and supported by several very ingenious experiments communicated to the British Association at its meeting in Manchester. One of these was, that protoxide of manganese had the singular power of hastening the oxidation of starch in nitric acid†. The metallic protoxide, from its

\* Reports of British Association, vol. xi. 2d Part, p. 32.

† The experiment is easily made by dissolving 1 ounce oxalic acid in  $\frac{1}{2}$  a pint of water at 180° F., and adding to this 1 oz. colourless nitric acid of

disposition to pass into the state of peroxide, aids the oxalic acid to decompose the nitric acid, the united affinities of both being able to accomplish what neither by itself could do. The protoxide remains unaffected at the end of the experiment, because, under the circumstances (the presence of acid), it cannot gratify its desire to become peroxide, and, therefore, it passes over its oxygen to the carbon, which escapes as carbonic acid. Mercer cited, as further examples, the action of protoxide of copper in eliminating oxygen from a solution of hypochlorite of lime, and of peroxide or binoxide of nitrogen in commencing the oxidation of a mixture of protochloride of tin and nitric acid. Mercer implied by these instances, that catalysis is an affinity of the catalytic agent for an element in the body acted upon, that affinity being feeble and incapable of gratification under the circumstances.

It would be advantageous to science if we could arrange under a known power the cases of decomposition which appeared so mysterious as to induce the great Berzelius to ascribe them to the action of a new force. It may not be possible in the present state of our knowledge to comprehend the whole of the instances observed, but, if most are included in one category, we have a right to suppose that the others may be embraced as our knowledge progresses. I shall therefore endeavour to show that many catalytic decompositions are merely cases of chemical affinity exerted under peculiar conditions.

In no instance of chemical union does there seem to be such a complete gratification of affinity as to suppress the attractions of the elements. The inherent affinities still remain more or less powerful, for, if it were not so, the compound would be permanent under all circumstances and not liable to further change by the action of external agents. When manganese unites with 1 atom of oxygen, the affinity of the metal for oxygen is not wholly merged, but is still strong enough to attach to itself 1, 2 or 3 atoms more oxygen. When the oxide is one of the lowest of the series, this affinity exhibits itself in a *basic* power by attaching itself to any complex highly oxygenized molecule, such as the oxygen acids, or of radicals playing the part of oxygen. When, on the other hand, the manganese or other radical becomes highly oxygenized, we find it possessing *acid* properties, that is, the

1.30 sp. gr. No action ensues on this mixture, but it immediately commences on the addition of a protosalt of manganese, which for simplicity may be the oxalate or nitrate. The action is also strikingly shown by heating a mixture of oxalic acid until the action commences, then diluting it till all action ceases. A little protosalt of manganese now added to the solution causes an immediate renewal of the oxidation.



additional atoms of oxygen, being less firmly attached, are capable of gratifying the disposition of a less oxygenized atom (the base) to attach itself to a higher oxide, or, to use the convenient phraseology of Graham, the base becomes zincous to the acid, which is now chlorous.

On heating the nitrates, nitric acid is not given off, but  $\text{NO}_4 + \text{O}$ . The decomposition readily results from the disposition of the base to appropriate more oxygen and pass into the higher oxides. If the base be oxide of nickel, the oxygen becomes attached to the oxide and remains; if, however, an oxide which has but a feeble affinity for oxygen at an elevated temperature, the elasticity of that element is able to overcome the affinity, which succeeded in breaking up the nitric acid. The final action is so obviously dependent upon the oxygenous part of the acid, as to make Schönbein believe that salts contain peroxides ready-formed; thus that  $\text{NO}_5$ ,  $\text{HO} = \text{NO}_4 + \text{HO}_2$ , or  $\text{PbO}$ ,  $\text{NO}_5 = \text{NO}_4 + \text{PbO}_2$ . This however is an unnecessary supposition, the previous view accounting sufficiently for the decomposition of a nitrate, so as to produce  $\text{NO}_4$  and  $\text{O}$ . Admitting this view to be correct in the expression that the preponderating quantity of a chlorous element in an acid renders the latter chlorous to a base, the mechanical attachment being to the chlorous element, we can understand why the number of atoms of oxygen in a base should regulate the number of atoms of acid attached to it. Thus  $\text{RO}$  presents only one chlorous element of attachment to the acid, and therefore the latter adheres to it in one proportion; whereas  $\text{R}_2\text{O}_3$ , which possesses three atoms of a chlorous element equally distributed round a zincous nucleus, presents three points of attachment, and therefore produces a salt  $\text{R}_2\text{O}_3, 3\text{A}$ . This view in result gives all the simplicity of the acid radical theory, both views entertaining the idea that the oxygenous atoms of the base and acid are attached to each other. We have certain instances, as for example  $\text{KO}$ ,  $\text{ClO}_5$ ;  $\text{PbO}$ ,  $\text{NO}_5$ , where the elastic atoms of oxygen combine as closely together as non-elastic atoms, such as lead or silver.

Although to aid conception we may suppose the atoms of oxygen of the base and of the acid to be in mechanical connexion, the true arrangement is probably not so, seeing that in a base there is always a part more zincous than the oxygenous atom, although the base as unity is zincous to the acid. We see many instances in chemistry of union of atoms in *pairs*, or what may be called *dual affinity*. This Graham\*

\* Trans. Royal Soc. Edin. vol. xiii.; Phil. Trans. 1837, p. 47 *et seq.*; Phil. Mag. Third Series, vol. xxiv. p. 401 *et seq.*

has proved to be the case with regard to atoms of water, and we know of numberless instances in the case of oxides. Thus RO uniting with oxygen forms  $RO_2$ . In this case  $RO + O$  corresponds to  $RO + A$ , the acid here representing the chlorous element from its oxygenous character. It is not necessary to suppose that A and O are associated in one continuous line, the probability being that the molecule may really be represented by ARO. Thus also in  $R_2O_3$ , where the  $O_3$  are probably grouped equally round  $R_2$ , there is room for three more of a chlorous element to gratify the dual affinity, and the general formula  $R_2O_3, 3A$  is the result, the 3A here representing three of a simple chlorous element. The result, as regards affinity, will still however be the same, the whole depending upon the attraction of the central nucleus R. It is therefore only for simplicity of expression in studying the phenomena of catalysis, that I view the atoms of oxygen of an acid as associated in mechanical continuation with the atoms of oxygen of the base, the *effect* being represented by this expression: the whole views of molecular or atomic constitution of bodies are in my opinion only convenient fictions to enable us to study the forces themselves, and the conception of a mechanical arrangement I only adopt as expressive of the manifestations of powers residing in matter.

To show that the tendency of bases to  $NO_5$ , even without being combined, is to attach themselves to the oxygenous part of the acid, a curious phenomenon observed by Mercer may be cited.

A portion of alumina may be taken and placed at the bottom of a vessel containing warm  $NO_5$ ; no action ensues, except partial solution; a slip of calico coloured in indigo-blue may now be introduced into the mixture, and remains unaffected in the clear acid, but is immediately discharged when pressed with a glass rod into the alumina. Here the alumina acts by placing the oxygen of the nitric acid in a state of tension without however succeeding in decomposing it, but the moment an assistant affinity comes into play, that state is shown by the decomposition of the nitric acid and oxidation of the indigo. The alumina in the presence of the acid could not oxidize (in fact, we know of no higher oxide), and therefore the indigo appropriates the oxygen. I find that various other oxides, such as calcined  $Cr_2O_3$  and  $SnO_2$ , have the same power, the latter showing this disposition more strongly than any of the other oxides. The best mode of trying these experiments is to heat a certain quantity of nitric acid, and then dilute it till indigo cloth ceases to be bleached. The oxide of tin is now added and allowed to fall to the bottom. On in-

roducing a slip of indigo-blue calico, the portion in the clear acid will be found to remain unaffected, while that in contact with the insoluble oxide will be bleached in a few seconds. That this decoloration of the indigo is due to the assistant affinity of another body acting in the same direction, *i. e.* also having a disposition to unite with oxygen, may perhaps best be shown by the following experiment:—Warm nitric acid is diluted to such extent that it just ceases to discharge indigo-blue calico; it is then divided into two portions, with slips of coloured calico in each, and through one of these binoxide of nitrogen is passed. In the latter the indigo becomes quickly bleached, while it remains unaffected in the former, the action obviously being due to the accessory affinity of the nitric oxide for more oxygen. In the same way indigo-blue is discharged during the decomposition of a nitrate by heat, other kinds of organic matter being oxidized under like circumstances; in these instances the decomposition of the nitric acid is much facilitated,—1. by the affinity of the base for oxygen; 2. the affinity of the organic matter for oxygen, which unites with it at the elevated temperature. There are many similar instances of this kind, where the behaviour of  $\text{NO}_2$  or  $\text{NO}_4$  as an assistant is too clearly contrasted with the action of other bodies to permit mistake. Thus urine when kept is unfit for the preparation of urea, that substance having been converted into carbonate of ammonia during the action of the air upon the mucus or colouring matter contained in the fluid. Colourless nitric acid unites with urea and may be heated with it without decomposition; but nitric acid containing any of the lower oxides of nitrogen, such as  $\text{NO}_2$  or  $\text{NO}_4$ , immediately decomposes urea into carbonic acid and ammonia\*. We cannot conceive that a lower oxide can more readily oxidize urea than a higher oxide, and hence we can only view the  $\text{NO}_4$  as aiding the urea to oxidize itself, as the mucus does in urine. In the same way, the action of pure nitric acid on colourless uric acid is to form alloxan, if the operation has been conducted so as to prevent the formation of nitrous acid ( $\text{NO}_4$ ) during the oxidation. But if  $\text{NO}_4$  has been evolved, or if the colouring matter of the urine be still contained in the uric acid, the products are only carbonate and oxalate of ammonia. The colouring matter of the urine and  $\text{NO}_4$  are thus seen to possess a similar action, which is exactly the same as that of protonitrate of manganese on a mixture of starch and nitric acid, no oxalic acid being formed in the presence of this salt, the only product of

\* A solution of urea in nitric acid is immediately decomposed with lively effervescence when a little  $\text{NO}_2$  is passed through it.

oxidation being carbonic acid. The  $\text{NO}_4$  or  $\text{NO}_2$  acts in these cases clearly by aiding the compound ready to oxygenate, but which, under the conditions, has not sufficient power to decompose the nitric acid without additional aid. The same explanation probably applies to the singular discovery of Professor Graham\*, that the addition of  $\text{NO}_4$  to non-accendible phosphuretted hydrogen renders it inflammable. In this case the two combined affinities produce the union of oxygen with one of the bodies. The presence of the small quantity of another compound of phosphuretted hydrogen in the spontaneously accendible gas, as described by Leverrier† and by Thenard‡, may probably act in the same manner.

The action of this compound ( $\text{PH}_2$ ) corresponding to amide ( $\text{NH}_2$ ) may be conceived so to disturb the attraction of the phosphorus to the hydrogen in the gas  $\text{PH}_3$  as to produce the inflammability. Both the elements of this gas are highly combustible, uniting with oxygen at a low temperature. Their mutual attractions are sufficiently strong to prevent the oxygen breaking up this union; but when the second body is present, the desire of  $\text{PH}_2$  for another atom of hydrogen may be supposed so far to draw the third atom of hydrogen from the  $\text{PH}_3$ , that oxygen has now the power to unite with the two inflammable elements. In disturbing the existing equilibrium, it is presumed to act just as a spark would do by elevating the already strong affinities of the two elements for oxygen. When a solution of hypochlorite of lime is poured into a solution of muriate of ammonia in excess, a very pungent volatile compound results, which has no bleaching properties, and therefore does not contain hypochlorous acid. The decomposition is expressed by the equation  $\text{NH}_4\text{Cl} + \text{CaO}, \text{ClO} = \text{NH}_2\text{Cl} + 2\text{HO} + \text{CaCl}$ . The volatile compound  $\text{NH}_2\text{Cl}$  has an affinity for hydrogen in order to pass into  $\text{NH}_4\text{Cl}$ . This body was well-fitted to test the view of the cause of the inflammability of phosphuretted hydrogen (even supposing  $\text{PH}_2$  is not spontaneously inflammable, as it is stated to be by Thenard). On placing gas (which had entirely lost its inflammability by standing several days over water), in contact with the above mixture, in about an hour it acquired the property of smoking strongly in the air, although it did not inflame spontaneously. This showed that the affinity of  $\text{PH}_3$  for oxygen was much elevated, although the attraction was not sufficient for inflammation.

\* Trans. Royal Soc. Edin. vol. xiii. p. 5.

† Ann. de Ch. et de Phys. lx. 174.

‡ Comptes Rendus des Séances de l'Académie des Sciences, t. xviii. pp. 252, 914; t. xix. p. 313.



There cannot be any doubt that the atoms of a body may be placed in a greater or less degree of tension by varying conditions. The experiments of Mr. Joule\* and myself on Allotropism have fully proved that the space occupied by the same body alters under different circumstances. It is therefore not an unreasonable assumption that the affinity of one body for a particular element may be sufficiently great to produce a tense state of the atoms without effecting decomposition†: hence the added affinity of a second body acting in the same direction may cause that change which each alone could not effect. Anything that disturbs the state of statical equilibrium in such a body will often effect its decomposition.

This accessory affinity is recognised when both bodies enter into union. Charcoal and chlorine decompose alumina at a red heat, though neither can do so separately. In the same way Boudault‡ has shown that a mixture of potash or soda and red prussiate of potash oxidizes various metallic oxides, while Mercer has for many years made use of this mixture to discharge indigo-blue on calico§. Red prussiate of potash ( $\text{Fe}_2\text{Cy}_6\text{3K}$ ) has a great disposition to attach to itself another atom of potassium to become yellow prussiate of potash ( $\text{Fe}_2\text{Cy}_6\text{4K}$ ). It cannot gratify this desire without aid; but when assisted by a substance having an affinity for the oxygen of the potash, and capable of appropriating it, decomposition follows. There are often cases in which the body exercising the accessory affinity may be unable to effect the union, either by the influence of unfavourable chemical conditions or of cohesion or elasticity. Thus, in the case with which we first started, the affinity of protoxide of manganese for oxygen aids in the decomposition of nitrate of protoxide of manganese, and sesquioxide of manganese remains. If the temperature during the decomposition

\* *Memoirs of Chemical Society*, vol. iii. p. 93.

† The alteration in volume is best seen in those oxides which contract and increase in specific gravity by the application of heat, for example, when the brown oxide becomes the green oxide of chromium. The two oxides must have a different molecular constitution, and this may be supposed to result from the elastic powers of one of its elements and the cohesive force of the other. The first effect of heat on oxide of chromium must be to expand the atoms of oxygen, and removing them further from the two atoms of chromium, permit the cohesive attraction of the latter to be gratified. Hence the compound acquires properties dependent upon cohesion, such as indifference to union and diminished solubility.

‡ *Journal de Pharmacie*, tome vii. 437. [*Phil. Mag. Third Series*, vol. xxvii. p. 307.]

§ *Phil. Mag. Third Series*, vol. xxxi. p. 126. In justice to Mercer, although this does not remove Boudault's claim of priority of publication, I cannot refrain from stating that the former chemist pointed out to me the oxidizing powers of the prussiates four or five years since.

be elevated, the oxygen resumes its elastic state and refuses to form this higher oxide, as in fact we know is the case in Mercer's experiment with oxalic acid and nitric acid, where the presence of hot  $\text{NO}_5$  is an unfavourable chemical condition to the existence of  $\text{Mn}_2\text{O}_3$ , and therefore it is not formed, but in its stead the oxygen is passed over to the organic matter, which is able to unite with it under the circumstances. A similar instance of the effect of such conditions is seen when the peroxides of copper, manganese or lead, are thrown into a solution of bleaching powder. The affinity of these oxides for an additional quantity of oxygen enables them to decompose the hypochlorite of lime, converting it into chloride of calcium. When the protoxides are used, this liberated oxygen unites and converts them to peroxides. The latter themselves have sufficiently strong affinity for oxygen to cause the decomposition to proceed; but not uniting with it, pure oxygen is given off in the gaseous state. Here elasticity has come into play, and being more powerful than the feeble chemical affinity, causes the oxygen to escape as a gas. When the solution is cool the gas goes off in a succession of small bubbles; but when hot, the escape is tumultuous, the heat aiding the oxygen to enter into the elastic state\*. A solution of chloride of lime evolves oxygen slowly at the boiling-point; but the decomposition is much accelerated by the accessory agents referred to.

The action of certain oxides upon peroxide of hydrogen is exactly similar to that on a solution of hypochlorite of lime. Thus peroxide of manganese, the protoxides of cobalt and lead, minium, peroxide of iron, and the protoxides of nickel, copper and bismuth, all exert this action on peroxide of hydrogen with a force indicated by their order†. In none of these cases does the oxide unite with a further proportion of oxygen. The violence of the action is however in proportion to their power of uniting with more oxygen. The first five oxides in the list have higher oxides of definite composition and of a certain degree of stability, with the exception of ferric acid; while the protoxides of copper and bismuth, although possessing the power of uniting with more oxygen, do not present superior oxides of a marked character. We should have

\* The best mode of instituting the experiment is to make a mixture of chloride of soda and caustic soda, heat this to a temperature near ebullition, and add sulphate of copper. The oxide of copper precipitated in the fine state of division causes such a copious evolution of oxygen gas that the contents are apt to be thrown out of the vessel: a mixture of chloride of lime and lime, or the ordinary unfiltered bleaching-powder of commerce, are also well-fitted to show the action.

† Thenard's *Traité de Chimie*, 6th edit. vol. i. p. 216.

expected the oxides of nickel and cobalt to have exerted the same power, but from Thenard's description of the former being in the state of a black powder, it may have been the oxide of increased specific gravity, to which attention has already been drawn\*. In all these cases the affinity is supposed to be sufficiently strong to break up the atoms of a body yielding to the slightest disturbance of its state of statical equilibrium. Two affinities are at play in these decompositions, viz. the attraction of the metallic oxide for oxygen and that of the water for the same body; both these affinities resist the union, and therefore, elasticity coming into operation, robs both oxides of the gas. The affinity causing the decomposition is so slightly preponderating in its influence, that a second cause coming into operation is quite sufficient to alter the conditions under which it was originally exerted, and to draw one of the elements of the body acted upon beyond the sphere of its affinity.

The balance of affinities in all such cases is so near that we not unfrequently find apparently contradictory effects resulting from their gratification. Thus the addition of oxide of silver to peroxide of hydrogen expels oxygen from the latter, but at the same time it is robbed of its own oxygen and reduced to the metallic state. In this case we have two feeble compounds instead of one, with affinities very nearly balanced, and with atoms so tense as to yield readily to the first disturbing cause. We can scarcely adopt as sufficient the explanation of Thenard and Mitscherlich†, that the reduction is due to the elevation of temperature accompanying the decomposition, because even when that is lowered by the addition of much water to the peroxide of hydrogen, the silver still becomes metallic.

It is a point yet undetermined, whether a lower oxide is to be considered as unity to a higher oxide, or whether all the atoms of oxygen are held by equal attractions. We know that tartaric acid is able to separate potash from nitric acid in forming a bitartrate, and yet acetic acid is sufficient to remove the second atom of potash from the neutral tartrate. But in a bibasic acid, like tartaric acid, it may be either atom of potash that is abstracted, and the superior affinity for the remaining one may be owing to attractions resulting after the expulsion of the first. Thus  $\text{MnO}_2$  may have its atoms of oxygen distributed round the central nucleus Mn, and held by equal attractions, and the stability of the red oxide produced by its calcination does not show that it pre-existed

\* Memoirs of the Chemical Society, vol. ii. p. 384, and vol. iii. p. 81.

† Poggendorff's *Annalen*, lv. 321.



in the black oxide, but merely that the attraction became stronger when one of the elements which divided it was removed. If it be admitted that the attraction of a radical for oxygen is equally divided between all the atoms of that element associated with it, the action to which we have alluded becomes comprehensible. In an oxide we have the attraction of affinity opposed by the elasticity of its oxygen and by the cohesive force of the metal. If  $a$  be the attraction of the central nucleus or radical,  $c$  the cohesive force of the metal, and  $e$  the elasticity of the oxygen, then the molecular formula of a protoxide will be  $\frac{a}{c+e}$ , of a sesquioxide  $\frac{2a}{2c+3e}$ , and of a binoxide  $\frac{a}{c+2e}$ . Now if, as in oxide of silver, the  $a$  and  $e$  are nearly equal, or the  $a$  only slightly preponderating, and the  $c$  or cohesive force very powerful, we can readily conceive that the added force of a second  $e$  may overcome the small amount of preponderating force in favour of  $a$ . Thus, when oxide of silver is placed in contact with peroxide of hydrogen, its affinity for more oxygen is sufficient to draw the second atom of oxygen beyond the sphere of attraction of H, and deliver it over to its own elasticity. But in doing this the attraction of silver for oxygen has been divided between its own oxygen and that of the peroxide of hydrogen. Scarcely at any time capable of retaining its own oxygen, this division of its attractive force has been fatal to the existence of its oxide, and the water *in statu nascens* at the same time exerting an affinity for the oxygen just ready to escape; all these causes combined result in the reduction of the silver\*.

When pyruvic acid is in contact with oxide of silver, it unites and forms a salt; but when acting on carbonate of silver, a certain quantity of oxygen also leaves the oxide during the escape of carbonic acid, and metallic silver remains†. As Liebig ‡ suggests, motion may aid this result; but were this the only explanation, we should expect that

\* During the passage of this paper through the press, Mr. Brodie, in a lecture at the Royal Institution, showed that peroxide of potassium reduces chloride of silver, the two atoms of oxygen passing off in the gaseous state, while chloride of potassium and metallic silver remain behind, a singular decomposition, when the behaviour of potash is remembered. But the action is strictly the same as that here described; the atoms of oxygen, being liberated at the same time, are presented to the silver, which, dividing its attractive force between them, is not able to overcome the influence of elasticity of the oxygen and its own cohesion, and therefore remains in a metallic state.

† Berzelius, *Lehrbuch der Chemie*, fifth edit. vol. iv. p. 231.

‡ Chemistry of Agriculture, 4th edit., page 283.



silver would constantly be reduced during the action of other feeble acids on carbonate of silver. If, however, we suppose that the pyruvic acid,  $C_6H_6O_5$ , from its affinity for more oxygen, exerts an attraction for that element at the moment of the liberation of the carbonic acid, the decomposition would be similar to those we have already considered, especially if the previous view of the molecular constitution of salts be admitted. In that case the oxygen of the oxide being attached to that of the carbonic acid, will be made highly tense during the escape of the latter, and may therefore be detached by a very feeble force, its elasticity finally overcoming the weak affinity. An extension of the explanation however strikes me as more probable, but it would be premature to insist upon it without being supported by experiments which I have not yet been able to conclude.

The action of metals and of charcoal on peroxide of hydrogen may be explained by the same feeble affinity. Alkalies also, from their attraction for oxygen, as indicated both by their capability of uniting with more oxygen and by their basic power or disposition to attach themselves to a compound behaving as an oxygenous or chlorous element, favour the decomposition of  $HO_2$ , while acids, on the other hand, render it more stable, perhaps, as Thenard himself suspected\*, from there being an inferior oxide ( $H_2O_3$ ?). In this instance the elasticity of the oxygen tends to conceal the play of affinities by preventing combination.

When the acting body is present in large quantity, or exhibits an increased surface, the action goes on with proportionate rapidity. Thus, when nitric acid is in contact with starch, the action is moderate until a certain quantity of peroxide of nitrogen has been evolved by decomposition, after which it proceeds with a violence difficult to control. The peroxide of nitrogen surrounding every particle of starch aids it in the decomposition of the nitric acid. That this is the real cause of the phenomenon may be proved by the following simple experiment. Nitric acid is heated with starch to a temperature at which the action has a tendency to commence but has not yet begun. A stream of  $NO_4$  or  $NO_2$  is then passed through the liquid, when action immediately begins with an activity proportionate to the quantity of gas added. The elevation of temperature due to the progressive action influences the decomposition, by causing the atoms of nitric acid to become more tense. Exactly the same accessory affinity is used by the manufacturer of oxymuriate of tin, when he adds a fragment of tin to the mixture of chloride of tin and

\* *Traité de Chimie*, p. 211.

nitric acid. The tin eliminating some nitric oxide quickens the action, which commences with difficulty with pure nitric acid; nitric oxide gas passed through the solution answers the same purpose.

This accessory affinity also enables oxide of copper or peroxide of manganese to evolve copious streams of oxygen from chlorate of potash in a state of fusion. The heat of fusion decomposes the compound slowly, but on adding a body having an affinity for the element acted upon by the heat (oxygen), the decomposition proceeds with greatly increased rapidity. We cannot ascribe this action to the presentation of points from which the gas may escape, as in the lowering of the temperature of ebullition by particles of sand, because silica has no influence in accelerating this decomposition\*.

In the examples previously given we have the decompositions aided by the tendency of one of the bodies to assume the elastic form. But when the body acted upon has two elements, one of which is influenced by elasticity, the other by cohesion, we find it peculiarly liable to be acted upon by external agents. Persulphuret of hydrogen is a compound of this class, and has been closely studied in its decompositions by Thenard†. The same bodies which decompose peroxide of hydrogen act catalytically upon this sulphuret. The decomposition cannot be due to points for the escape of gas, as suggested by Liebig‡, to explain the decomposition of peroxide of hydrogen, because solutions of the alkalies act with equal power. The sulphurets, especially those of the alkaline metals, decompose it very readily. As in the case of peroxide of hydrogen, the acids afford stability to its sulphur analogue. In the view of acids given, they are supposed to have become chlorous or electro-negative, representing and behaving as oxygen, and therefore exerting no affinity, we should anticipate that they would not show any disposition to break up an oxygenous compound or its analogue of sulphur. Another instance of accessory affinity is seen in the nitrosulphates§; the formula  $(RO, SO_2 + NO_2)$  given by Pelouze to these compounds does not allow us to understand their decompositions, which however becomes intelligible if we view nitrosulphuric acid as nitric acid, in which the fifth atom of oxygen has been replaced by one of sulphur  $(RO, NO_4S)$ . In this acid we have two elements—the nitrogen and the sulphur—sharing the oxygen, their mutual affinities being nearly balanced when the acid is united with an al-

\* Taylor's Scientific Memoirs, vol. iv. p. 9.

† *Ann. de Ch. et de Ph.* xlviii. 79.

‡ *Ann. der Pharm.* ii. 22.

§ *Ann. de Ch. et de Ph.* lx. 151.

kali, although in a free state, the sulphur exhibits a superior affinity, as shown by the decomposition which then results,  $\text{NO}_4\text{S} = \text{NO} + \text{SO}_3$ . Now any substance which acts as an accessory to the sulphur by aiding the withdrawal of oxygen from the nitrogen decomposes it. This instability is especially exhibited in  $\text{NH}_4\text{O}$ ,  $\text{NO}_4\text{S}$ ; the 3 atoms of hydrogen of the ammonia in their attraction for oxygen introducing another affinity, which accelerates decomposition. And, in fact, we do find that the same agents which so readily decompose the oxygenous compounds, chloride of lime and peroxide of hydrogen, do equally cause the disruption of nitrosulphate of ammonia into protoxide of nitrogen and sulphate of ammonia. Alkalies are an exception to this rule, as they render the nitrosulphates more stable, while they make the peroxide of hydrogen prone to decomposition; but the cases are different, the latter substance having none of the properties of an acid.

The basic character of alkalies, defined as their power of uniting with more oxygen, or with an acid playing the part of an oxygenous element, is illustrated by several curious decompositions. Thus, though grape-sugar reduces sulphate of copper with ease, cane-sugar alone does not readily do so, but when mixed with potash and boiled with the salt, suboxide of copper is produced, as in the mode of preparation of that oxide suggested by Boettger\*, or the reduction of chloride of silver as proposed by Levot†. Here the disposition of the organic matter to unite with oxygen is able to gratify itself when aided by the accessory affinity of the potash for oxygen. That the potash in this state acts by aiding the oxidation, is seen by heating  $\text{Cu}_2\text{O}$  with a solution of caustic potash, exposed to the air, when it oxidizes much more rapidly than when boiled with water alone‡. When suboxide of copper is dissolved in ammonia it oxidizes with surprising rapidity. In this instance the hydrogen of the ammonia adds to its disposition as an alkali to absorb oxygen. The quick oxidation is not merely due to the fact of

\* *Ann. der Pharm. und Chemie*, xxxix. 176.

† *Berzelius, Jahresbericht*, vol. xxv.

‡ This experiment may be simply made as follows:—Three shallow evaporating basins of the same size and form, each containing the same quantity of suboxide of copper, are taken, and to one is added a solution of potash or soda; to the second, a solution of chloride of manganese; to the third, common water, taking care that the same volume of each fluid is added. The whole are now placed on a sand-bath, so as to be exposed to equal temperatures, and stirred occasionally. The suboxide of copper in the basin containing chloride of manganese oxidizes very rapidly; that in contact with the potash more slowly; and that with simple water is scarcely effected when both the others have lost their red colour. These actions are strictly in accordance with theory.

the suboxide being in a state of solution, because the soluble salts of the suboxide do not oxidize with such extraordinary ease, nor is it to be expected that they should, if we admit that the acid itself plays the part of oxygen. The accessory affinity of alkalies for oxygen is exhibited in many other cases of chemical action. Thus, colouring matters, such as deoxidized logwood, Brazil-wood, peach-wood, japan, fustic and catechu are oxidized more rapidly in contact with alkalies than in water alone; and various dyeing principles, such as orcin and erythrin, absorb oxygen with great avidity in the presence of ammonia. Sugar may be boiled with potash without decomposition, but when air is admitted, formic, melassic, and glucic acids are produced. Hydruret of benzyle when exposed to air gradually absorbs oxygen and passes into benzoic acid, but in contact with potash this absorption is very much accelerated. The rapid decomposition of the gallates and of hematine in the presence of free alkali and air is a phenomenon of the same kind. In fact, numberless instances of this catalytic action of the alkalies are known to chemists.

We find the influence of an accessory oxidation in many cases of chemical union. Thus Campbell has shown\* that the transformation of cyanide of potassium into cyanate of potash is much accelerated by the presence of the iron in yellow prussiate of potash, the iron being converted into oxide during the transformation. Here the iron plays the part of the protoxide of manganese in the cases of oxidation already referred to, or it perhaps bears a more direct relation to the action of lead in communicating a tendency to the base metals to seize oxygen during the process of cupellation. The influence exerted by peroxide of manganese in first converting cyanide of potassium into cyanate of potash and afterwards into the carbonate of that base, is another instance of accessory affinity; for only a portion of the oxygen is derived from the oxide employed. The solution of an alloy of silver and platinum in nitric acid may be supposed to be a similar affinity. It is not necessary to believe that this is a case proving the communication of intestine motion to the atoms of platinum, by which it acquires the power of decomposing nitric acid†; for an equally simple explanation is given by assuming that the united affinities of platinum and silver are able to decompose nitric acid, both these affinities acting in one direction at the same time, and enabling the platinum to dissolve. We have only to suppose that the atoms of nitric acid are placed by the silver in a state of such tension that the platinum can

\* Phil. Mag. Third Series, vol. xix. p. 513.

† Liebig's Elements of Agriculture, 4th edit., p. 280.



now seize oxygen, which it could not do from the nitric acid when in a less tense state. The quartation of gold is obviously a phænomenon of the same kind. In these instances the interposing silver much reduces the cohesive or aggregative force of the platinum or gold, which opposes so strongly the action of nitric acid upon them. But when we have every atom of platinum or of gold separated by one of silver, great facility is given to the nitric acid to act upon these metals, especially when the silver at the same time aids them by its assistant affinity.

We have seen, in the consideration of the previous instances of catalysis, that the play of affinities was occasionally so nearly balanced, that a second disturbing cause determined the direction of the action. In the case of non-accendible phosphuretted hydrogen, the addition of another oxidizable body,  $\text{NO}_4$ , decided the union of oxygen with the gas. In accendible phosphuretted hydrogen the compound  $\text{PH}_2$  played the same part. When the accessory agent is present in small quantity, the preponderating affinity of the body acted upon shows itself in the result. But, as the action is due to two affinities nearly equal in amount, it is easy to conceive that the increased quantity of the accessory agent may exactly balance affinities, and that the catalytic phænomenon will be prevented. Thus one-twentieth of the volume of binoxide of nitrogen, according to Graham \*, added to accendible phosphuretted hydrogen, does not deprive it of inflammability, the bubbles of gas escaping into the air with a kind of explosion, although one-tenth volume of the same gas altogether prevents the accendibility. This nitric oxide, when pure, does not, like protoxide of nitrogen, render phosphuretted hydrogen spontaneously inflammable, the reason obviously being that its own affinity for oxygen is more powerful than that of the phosphuretted hydrogen. When added however in such small proportion to the accendible gas that the foreign constituent in it preponderates, then it becomes an accessory to the oxidation, though an increase of the quantity renders it more powerful, and prevents accendibility by itself seizing oxygen. Thus also larger volumes of gas, having an affinity for oxygen, but incapable like  $\text{NO}_4$  of gratifying that desire under ordinary circumstances, may exactly balance the feeble affinity of the foreign accessory body and prevent oxidation. Five volumes of hydrogen, 2 volumes of carbonic acid, 1 volume of olefiant gas, and 1 volume sulphuretted hydrogen, deprive 1 volume of phos-

\* Phil. Mag., Third Series, vol. v. p. 405.

phuretted hydrogen of its spontaneous inflammability\*. The very conception of a catalytic agent, on the view adopted, implies the exertion of an affinity, which is passed over or added to that of the body acted upon. If, therefore, a third body claim this added affinity, the increase of power being divided, may be insufficient to exert the force which it did when wholly applied to aid the affinity of one body. It may be this balancing of affinities which prevents the action of platinum on a mixture of oxygen and hydrogen. The platinum by its *surface affinity* condenses oxygen, and presenting it to hydrogen in a condensed form produces union. But in the presence of small quantities of certain oxidizable gases, such as sulphuretted hydrogen, carbonic oxide, and olefiant gases †, it ceases to exert this action, the assumption in this case being that the affinity of the added gases for oxygen balances that of hydrogen for the same gas.

This balancing of affinities may account for several phenomena otherwise inexplicable. On the decay of vegetable mould we find the hydrogen constantly diminishing in quantity until a certain period of decomposition, when the affinity of the carbon of the humus for its hydrogen balances the affinity of the surrounding oxygen. It seems to be the same balancing of affinities which renders corrosive sublimate so antiseptic in its properties; but, in this case, the balance results from the affinity of the second atom of chlorine in the bichloride of mercury for the hydrogen of the organic substance, thus preventing its union with oxygen. It is probable that the same affinity of chlorine for hydrogen causes turpentine and the volatile oils to act catalytically in exploding chloride of nitrogen. The chlorine attracted by the hydrogen of these substances is drawn without the sphere of its attraction for nitrogen, and a disruption of the elements consequently ensues, compounds such as this resting on the very verge of separation between physical and chemical attraction. The antiseptic action of corrosive sublimate is very different from that exerted by sulphurous acid and sulphate of iron, these bodies acting by their superior affinity for oxygen, and neutralizing the power of the ferments or accessory oxidizers present in the organic body.

There is no difficulty in applying these notions of catalysis to organic compounds, which from the complexity of their

\* The influence which the vapours of turpentine exert in preventing the oxidation of phosphorus in the air is probably another instance of this balancing of affinities.

† Faraday, *Phil. Mag.*, Third Series, vol. v. p. 405; Turner, *Jameson's Journal*, xi. 99 and 311.

molecules are peculiarly liable to change. If it once be admitted that an assisting affinity may exist in the sense defined in the present paper, then we see the same cause operating upon organic as well as inorganic molecules. When nitric acid acts on oxalic acid or starch, an inorganic body (a protosalt of manganese) lowers the temperature necessary for the oxidation, and exerts its influence until all the starch is converted into carbonic acid, being equally efficacious on the addition of more nitric acid and starch. Here the body acting as an assistant remains unchanged, and therefore continues its action *ad infinitum*, rendering it impossible to prepare oxalic acid from nitric acid and starch or sugar, carbonic acid being the only product\*. Had the assistant oxidizer passed from solution during the progress of the oxidation, it could not of course continue its favourable effect, and a new portion of it must have been added. Here the inorganic salt enables the sugar to oxidize itself from the surrounding medium just as yeast does, the only difference being that the yeast itself suffers change, and therefore can only continue its action for a limited period. It is exactly in the same condition as a mixture of nitric acid and binoxide of nitrogen made to act on protochloride of tin. A small portion of the latter added to such a mixture is oxidized, but when the solution is heated until all the  $\text{NO}_2$  is expelled, oxidation does not ensue on the addition of a new portion at the same low temperature as before. Now Saussure and Colin have shown that yeast only induces fermentation when it is in a position to absorb oxygen. It acts therefore strictly as binoxide of nitrogen, or a protosalt of manganese, in the previous instances, by adding its affinity for oxygen to that of the sugar, the added affinities of both completing the union. The only difference between these two decompositions is, that in one case the oxidizing agent is nitric acid, in the other it is water. The composition of sugar shows it to contain the elements of alcohol and carbonic acid *minus* an atom of water. In such a compound we have the affinity of carbon for hydrogen and of carbon for oxygen. The yeast by its nitrogen also exerts an affinity for hydrogen, and by its carbon for oxygen. The united affinities of the sugar and of the yeast acting upon water decompose it, its elements on their liberation being shared by the carbon of the sugar, for which it may be supposed to have the strongest affinities,  $\text{C}_{12} \text{H}_{11} \text{O}_{11}$

\* In this it resembles the action of oxalic acid in converting an unlimited quantity of oxamide into oxalate of ammonia, with this difference, that the oxalic acid, which causes the change, may not be the same, but a regenerated portion, while the salt of manganese always remains unchanged.

+  $\text{HO} = 4\text{CO}_2 + 2 (\text{C}_4 \text{H}_6 \text{O}_2)$ . To show the exact similarity of the two processes of oxidation when the assisting body is either organic or inorganic, I may cite the curious manufacturing process for oxidizing oils in the method of dyeing Turkey-red used in this country, and included in Mercer's patent for that colour. It consists in oxidizing oils by blowing hot air through them, the oils being in contact with a solution of a salt of copper or of bran; the contact of either of these solutions is found very materially to accelerate the oxidation. The catalytic action of oxide of copper in evolving oxygen from hypochlorite of lime was adduced as showing its affinity for more oxygen, and this feeble affinity is well known and used empirically by all calico-printers, who are in the constant habit of mixing a salt of copper with their colours for the purpose of *ageing* them more speedily; in other words, of causing them to unite with oxygen. This also is the assisting cause in Mercer's process for oxidizing oils; bran in solution answers the same purpose from its affinity for oxygen. The addition of common salt or muriate of ammonia favours the oxidation in all the cases referred to, the oxidation proceeding much more quickly in their presence. No sub-chloride is ever formed, the action being purely catalytic, and probably depending on the conversion of the salt of copper into a chloride, the chlorine of which may be supposed to exert a slight affinity for the hydrogen of the compound, thus withdrawing it somewhat from the sphere of its own special attractions in the body; the copper now aiding the chlorine, delivers the hydrogen more easily into the power of the oxygen of the atmosphere. It is therefore immaterial whether the body exercising the assistant affinity be organic or inorganic, if the conditions be favourable to the exercise of this influence. The action of a body in acetous fermentation on the transformation of brandy into vinegar must be recognised as a phenomenon of a like kind. We know that brandy may trickle without change over a large surface of wood shavings, through which air circulates at the heat of the human body, but that it is quickly converted into vinegar if brandy in the act of oxidation be mixed with it. Here the added ferment exerts its assisting affinity in precisely the same way as the salt of copper, when it aids the oxidation of oils or colours, or as protonitrate of manganese or peroxide of nitrogen during the oxidation of starch. The conversion of hydrogen and oxygen into water by the action of fermenting silk, cotton, or woody fibre, as observed by Saussure, is obviously a phenomenon of the same kind, and can only be exerted slowly and in the immediate vicinity of the assisting oxid-



zers, just as a ball of spongy platinum silently effects the union of these two gases.

In these cases we must admit that the action is independent of a state of intestine motion of the atoms of one compound molecule imparted to those of another, or, if we do not allow this, we must create two new powers and separate decompositions caused by inorganic bodies from those produced by organic compounds, although all the phenomena of the decomposition show them to belong to one category.

In a body in a state of such incessant change as the blood of living animals, it would naturally be expected that an added agency, such as that described, would render it prone to abnormal actions and oxidations, and in fact we do recognise by all the recent progress in the study of public hygiene that the addition of any oxidizing miasm or putrid matter to the blood does produce those changes which are known by their results in the different forms of disease. These and other catalytic agents no doubt exercise most important influence on the processes of animal life and on the action of medicaments on the system, but it would be foreign to the object of this paper to examine them in detail.

The limits of a paper such as this compel me to avoid including many other instances of catalytic decompositions which come under this explanation, or of drawing special attention to those which cannot be included in the present state of our knowledge. Thus diastase, acting on starch, converts it into sugar, but we have so little knowledge of the composition or properties of the first body, that it would be unwarrantable to embrace a case such as this. But in analogous changes produced by bodies which are understood, the same power is recognised. Sulphuric acid in converting starch into grape-sugar offers an example of combination which may fairly be examined by the same method employed in investigating other decompositions. Graham has shown\* that heat is evolved even on the addition of the 48th atomic proportion of water to sulphuric acid, or, in other words, that the affinity of that acid for water is not gratified as long as our instruments of research can follow the change. This is merely another proof of the doctrine with which I started, that there is no evidence of such a complete gratification of affinity as ever to merge entirely the attractions of the elements of any body. In the case referred to, the development of heat on each successive addition proves that the water is condensed on entering into union with the acid. When the heat of the sulphuric acid is arti-

\* Phil. Mag. Third Series, vol. xxii, p. 334.

ficially increased, this compound is broken up, for distillation drives off the water and concentrates the acid. Now when starch is in the presence of this weak combination of sulphuric acid and water, at a temperature at which the latter is just able to exert its affinity and again have it destroyed by heat, it is not at all extravagant to suppose that the starch may seize the water in its nascent state at the moment of expulsion, or even that it may be able to unite with the last atoms of the series of acid and water when presented in that condensed state, although it cannot do so when the water is free and not nascent. Any such union would explain the transformation of starch into grape-sugar, the change merely being in the acquisition of water,  $C_{12}H_{10}O_{10} + 4HO = C_{12}H_{14}O_{14}$ . The action here is not the same, but the very reverse of that which ensues in the preparation of æther. In the one case the sulphuric acid abstracts water, in the other it is the means of adding it, and the difference of the action depends on the relative strength of the acids employed. Without at all giving an opinion in favour of the necessity for the formation of sulphovinic acid, as supposed by Liebig\*, or as to its not being an essential condition, as argued by Mitscherlich†, the final result is simply of the order now under consideration. In this decomposition the sulphuric or phosphoric acid is so strong that it combines with the water instead of yielding it, and the elevation of temperature essential to the change may either be due to the formation and after decomposition of sulphovinic acid, or it may be simply owing to the necessity of rendering the molecule of alcohol tense by heat, the elasticity of the æther and water both tending to break up the hydrate, the decomposition of which is determined by the presence of the strong acid now also aiding and abstracting the water. The final result is certainly purely catalytic in whatever light it is considered, although there may be more than one step in the process.

In conclusion, facts have been brought forward to show that there is at least as much probability in the view that the catalytic force is merely a modified form of chemical affinity exerted under peculiar conditions, as there is in ascribing it to an unknown power, or to the communication of an intestine motion to the atoms of a complex molecule. Numerous cases have been cited in which the action results when the assisting or catalytic body is not in a state of change, and attempts have been made to prove by new experiments that the catalytic body exercises its peculiar power *by acting in*

\* Geiger's *Pharmacie*, vol. ii. p. 711 *et seq.*

† *Lehrbuch der Chemie*, vol. i. p. 247 *et seq.*

*the same direction* as the body decomposing or entering into union, but under conditions in which its own affinity cannot always be gratified. The catalytic body is therefore a substance which acts by adding its own affinity to that of another body, or by exerting an attraction sufficient to effect decomposition under certain circumstances, without being powerful enough to overcome new conditions, such as elasticity and cohesion, which occasionally intervene and alter the expected result.

At the same time the theory is far from being fully proved; but if I have succeeded in rendering probable that the catalytic force is only chemical affinity recognised under an aspect which chemists have not been accustomed to view it, and exerted under conditions which can only be developed by close attention to details, it will not have been useless to direct increased study to this interesting class of phænomena.

XXXVI. *On Quaternions; or on a New System of Imaginaries in Algebra.* By Professor Sir WILLIAM ROWAN HAMILTON, LL.D., V.P.R.I.A., F.R.A.S., Corresponding Member of the Institute of France, and of other Scientific Societies in British and Foreign Countries, Andrews' Professor of Astronomy in the University of Dublin, and Royal Astronomer of Ireland.

[Continued from vol. xxx. p. 461.]

33. **F**OR the sake of those mathematical readers who are familiar with the method of co-ordinates, and not with the method of quaternions, the writer will here offer an investigation, by the former method, of that general property of the ellipsoid to which he was conducted by the latter method, and of which an account was given in a recent Number of this Magazine (for June 1847).

Let  $x y z$  denote, as usual, the three rectangular co-ordinates of a point, and let us introduce two real functions of these three co-ordinates, and of six arbitrary but real constants,  $l m n l' m' n'$ , which functions shall be denoted by  $u$  and  $v$ , and shall be determined by the two following relations:

$$u(l'l' + mm' + nn') = l'x + m'y + n'z;$$

$$v^2(l'l' + mm' + nn')^2 = (ly - mx)^2 + (mz - ny)^2 + (nx - lz)^2;$$

then the equation

$$u^2 + v^2 = 1 \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

will denote (as received principles suffice to show) that the curved surface which is the locus of the point  $x y z$  is an ellipsoid, having its centre at the origin of co-ordinates; and conversely this equation  $u^2 + v^2 = 1$  may represent any such ellip-

soid, by a suitable choice of the six real constants  $l\ m\ n\ l'\ m'\ n'$ .  
At the same time the equation

$$u^2 = 1$$

will represent a system of two parallel planes, which touch the ellipsoid at the extremities of the diameter denoted by the equation

$$v = 0;$$

and this diameter will be the axis of revolution of a certain circumscribed cylinder, namely of the cylinder denoted by the equation

$$v^2 = 1;$$

the equation of the plane of the ellipse of contact, along which this circular cylinder envelopes the ellipsoid, being, in the same notation,

$$u = 0:$$

all which may be inferred from ordinary principles, and agrees with what was remarked in the 29th article of this paper.

34. This being premised, let us next introduce three new constants,  $p, q, r$ , depending on the six former constants by the three relations

$$2p = l + l', \quad 2q = m + m', \quad 2r = n + n'.$$

We shall then have

$$l'x + m'y + n'z = 2(px + qy + rz) - (lx + my + nz);$$

and the equation (1.) of the ellipsoid will become

$$\begin{aligned} & (ll' + mm' + nn')^2 \\ &= (l^2 + m^2 + n^2)(x^2 + y^2 + z^2) \\ & \quad - 4(lx + my + nz)(px + qy + rz) \\ & \quad + 4(px + qy + rz)^2 \\ &= (x^2 + y^2 + z^2) \{ (l - x')^2 + (m - y')^2 + (n - z')^2 \}, \end{aligned}$$

if we introduce three new variables,  $x', y', z'$ , depending on the three old variables  $x, y, z$ , or rather on their ratios, and on the three new constants  $p, q, r$ , by the conditions,

$$\frac{x'}{x} = \frac{y'}{y} = \frac{z'}{z} = \frac{2(px + qy + rz)}{x^2 + y^2 + z^2}.$$

These three last equations give, by elimination of the two ratios of  $x, y, z$ , the relation

$$x'^2 + y'^2 + z'^2 = 2(px' + qy' + rz');$$

the new variables  $x', y', z'$  are therefore co-ordinates of a new point, which has for its locus a certain spheric surface, passing through the centre of the ellipsoid; and the same new point



is evidently contained on the radius vector drawn from that centre of the ellipsoid to the point  $x y z$ , or on that radius vector prolonged. We see, also, that the length of this radius vector of the ellipsoid, or the distance of the point  $x y z$  from the origin of the co-ordinates, is inversely proportional to the distance of the new point  $x' y' z'$  of the spheric surface from the point  $l m n$ , which latter is a certain fixed point upon the surface of the ellipsoid. This result gives already an easy and elementary mode of generating the latter surface, which may however be reduced to a still greater degree of simplicity by continuing the analysis as follows.

35. Let the straight line which connects the two points  $x' y' z'$  and  $l m n$  be prolonged, if necessary, so as to cut the same spheric surface again in another point  $x'' y'' z''$ ; we shall then have the equation

$$x''^2 + y''^2 + z''^2 = 2(px'' + qy'' + rz''),$$

from which the new co-ordinates  $x''$ ,  $y''$ ,  $z''$  may be eliminated by substituting the expressions

$$x'' = l + t(x' - l), \quad y'' = m + t(y' - m), \quad z'' = n + t(z' - n);$$

and the root that is equal to unity is then to be rejected, in the resulting quadratic for  $t$ . Taking therefore for  $t$  the product of the roots of that quadratic, we find

$$t = \frac{l^2 + m^2 + n^2 - 2(lp + mq + nr)}{(x' - l)^2 + (y' - m)^2 + (z' - n)^2};$$

therefore also, by the last article,

$$t = \frac{x^2 + y^2 + z^2}{l^2 + m^2 + n^2 - 2(lp + mq + nr)};$$

consequently

$$t^2 = \frac{x^2 + y^2 + z^2}{(x' - l)^2 + (y' - m)^2 + (z' - n)^2};$$

and finally,

$$(x'' - l)^2 + (y'' - m)^2 + (z'' - n)^2 = x^2 + y^2 + z^2. \quad (2.)$$

Denoting by  $A, B, C$ , the three fixed points of which the co-ordinates are respectively  $(0, 0, 0)$ ,  $(l, m, n)$ ,  $(p, q, r)$ ; and by  $D, D', E$ , the three variable points of which the co-ordinates are  $(x', y', z')$ ,  $(x'', y'', z'')$ ,  $(x, y, z)$ ;  $A B E D'$  may be regarded as a plane quadrilateral, of which the diagonals  $AE$  and  $BD'$  intersect each other in a point  $D$  on a fixed spheric surface, which has its centre at  $C$ , and passes through  $A$  and  $D'$ ; so that one side  $D'A$  of the quadrilateral, adjacent to the fixed side  $AB$ , is a chord of this fixed sphere. And the equation (2.) expresses that *the other side  $BE$  of the same plane quadrilateral, adjacent to the same fixed side  $AB$ , is a chord of a fixed ellipsoid,*

if the two diagonals  $AE$ ,  $BD'$  of the quadrilateral be equally long; so that a general and characteristic property of the ellipsoid, sufficient for the construction of that surface, and for the investigation of all its properties, is included in the remarkably simple and eminently geometrical formula

$$\overline{AE} = \overline{BD'}; \dots \dots \dots (3.)$$

the locus of the point  $E$  being an ellipsoid, which passes through  $B$ , and has its centre at  $A$ , when this condition is satisfied.

This formula (3.), which has already been printed in this Magazine as the equation (10.) of article 30 of this paper, may therefore be deduced, as above, from generally admitted principles, by the Cartesian method of co-ordinates; although it had not been known to geometers, so far as the present writer has hitherto been able to ascertain, until he was led to it, in the summer of 1846\*, by an entirely different method; namely by applying his calculus of quaternions to the discussion of one of those new forms† for the equations of central surfaces of the second order, which he had communicated to the Royal Irish Academy in December 1845.

36. As an example (already alluded to in the 32nd article of this paper) of the *geometrical* employment of the formula (3.), or of the equality which it expresses as existing between the lengths of the two diagonals of a certain plane quadrilateral connected with that new construction of the ellipsoid to which the writer was thus led by quaternions, let us now propose to investigate geometrically, by the help of that equality of diagonals, the difference of the squares of the reciprocals of the greatest and least semi-diameters of any plane and diametral section of an ellipsoid (with three unequal axes). Conceive then that the ellipsoid, and the auxiliary sphere employed in the above-mentioned construction, are both cut by a plane  $AB'C'$ , on which  $B'$  and  $C'$  are the orthogonal projections of the fixed points  $B$  and  $C$ ; the auxiliary point  $D$  may thus be conceived to move on the circumference of a circle, which passes through  $A$ , and has its centre at  $C'$ ; and since  $AE$ , being equal in length

\* See the Proceedings of the Royal Irish Academy.

† In reprinting one of those new forms, namely the following quaternion form of the equation of the ellipsoid:

$$(\alpha\epsilon + \epsilon\alpha)^2 - (\beta\epsilon - \epsilon\beta)^2 = 1,$$

a slight mistake of the press occurred at p. 459, vol. xxx. of this Magazine, which however, with the assistance there given by the context, can scarcely have embarrassed the reader. In the preceding page, for a hyperboloid of one sheet, touching the same cylinder in the same *sheet*, should have been printed, . . . . in the same *ellipse*.

to  $BD'$  (because these are the two equal diagonals of the quadrilateral in the construction), must vary inversely as  $BD$  (by an elementary property of the sphere), we are to seek the difference of the squares of the extreme values of  $BD$ , or of  $B'D$ , because the square of the perpendicular  $BB'$  is constant for the section. But the longest and shortest straight lines,  $B'D_1$ ,  $B'D_2$ , which can thus be drawn to the auxiliary circle round  $C'$ , from the fixed point  $B'$  in its plane, are those drawn to the extremities of that diameter  $D_1C'D_2$  of this circle which passes through or tends towards  $B'$ ; so that the four points  $D_1C'D_2B'$  are on one straight line, and the difference of the squares of  $B'D_1$ ,  $B'D_2$  is equal to four times the rectangle under  $B'C'$  and  $C'D_1$ , or under  $B'C'$  and  $C'A$ . We see therefore that the shortest and longest semi-diameters  $AE_1$ ,  $AE_2$  of the diametral section of the ellipsoid, are perpendicular to each other, because (by the construction above-mentioned) they coincide in their directions respectively with the two supplementary chords  $AD_1$ ,  $AD_2$  of the section of the auxiliary sphere, and an angle in a semicircle is a right angle; and at the same time we see also that the difference of the squares of the reciprocals of these two rectangular semi-axes of a diametral section of the ellipsoid varies, in passing from one such section to another, proportionally to the rectangle under the projections,  $B'C'$  and  $C'A$ , of the two fixed lines  $BC$ ,  $CA$ , on the plane of the variable section. The difference of the squares of these reciprocals of the semi-axes of a section therefore varies (as indeed it is well-known to do) proportionally to the product of the sines of the inclinations of the plane of the section to two fixed diametral planes, which cut the ellipsoid in circles; and we see that the normals to these two latter or cyclic planes have precisely the directions of the sides  $BC$ ,  $CA$  of the *generating triangle*  $ABC$ , which has for its corners the three fixed points employed in the foregoing construction: so that the auxiliary and *diacentric sphere*, employed in the same construction, touches one of those two cyclic planes at the centre  $A$  of the ellipsoid. If we take, as we are allowed to do, the point  $B$  external to this sphere, then the distance  $BC$  of this external point  $B$  from the centre  $C$  of the sphere is (by the construction) the semisum of the greatest and least semi-axes of the ellipsoid, while the radius  $CA$  of the sphere is the semidifference of the same two semi-axes: and (by the same construction) these greatest and least semi-axes of the ellipsoid, or their prolongations, intersect the surface of the same diacentric sphere in points which are respectively situated on the finite straight line  $BC$  itself, and on the prolongation of that line. The remaining side  $AB$  of the same fixed or generating

triangle ABC is a semidiameter of the ellipsoid, drawn in the direction of the axis of one of the two circumscribed cylinders of revolution; a property which was mentioned in the 32nd article, and which may be seen to hold good, not only from the recent analysis conducted by the Cartesian method, but also and more simply from the geometrical consideration that the constant rectangle under the two straight lines BD and AE, in the construction, exceeds the double area of the triangle ABE, and therefore exceeds the rectangle under the fixed line AB and the perpendicular let fall thereon from the variable point E of the ellipsoid, except at the limit where the angle ADB is right; which last condition determines a circular locus for D, and an elliptic locus for E, namely that ellipse of contact along which a cylinder of revolution round AB envelopes the ellipsoid, and which here presents itself as a section of the cylinder by a plane. The radius of this cylinder is equal to the line BG, if G be the point of intersection, distinct from A, of the side AB of the generating triangle with the surface of the diacentric sphere; which line BG is also easily shown, on similar geometrical principles, as a consequence of the same construction, to be equal to the common radius of the two circular sections, or to the mean semiaxis of the ellipsoid, which is perpendicular to the greatest and the least. Hence also the side AB of the generating triangle is, in length, a fourth proportional to the three semiaxes, that is to the mean, the least, and the greatest, or to the mean, the greatest, and the least, of the three principal and rectangular semidiameters of the ellipsoid.

[To be continued.]

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### XXXVII. *Notices respecting New Books.*

*Notice of a Memoir on Meteors of various sorts.* By T. I. M. FORSTER, F.R.A.S., &c. Bruges, 1846.

EXPERIENCED in observing and in treating of these phenomena, Dr. Forster refers his readers to his former communications of them, and to the numerous articles in the Royal Society's Transactions, as well as in the Gentleman's and Philosophical Magazines.

He carefully examines the theory of phosphorescent jets of gas rising unperceived while traversing the low and damp strata of the atmosphere, but becoming ignited as soon as they reach a sufficiently dry stratum. The ignition is then supposed to run down the column of gas, and reveal the several bends it had been subjected to by various currents of wind. The occasional explosions may be explained by supposing the running fire to reach a spot overabounding in hydrogen, instances not unfrequent after heavy rains.



It was not till the 10th of August 1811 that the idea of their periodicity occurred to Dr. Forster, when he and his father counted some hundreds, and by their journal perceived their recurrence on that same day. Indeed, in copying a curious old manuscript calendar, he found the 10th and 18th of August called *stellibundæ* and *meterodes*; but he acknowledges their frequency at all times and in all places.

Inclined to assign them a gaseous origin, our author has yet, in deference to the learned men who differed from him, endeavoured to relate fairly the various arguments in favour of their several theories.

*Aristotle* regarded meteors as arising from exhalations denoting an approaching change of weather. *Theophrastus* thought they prognosticated wind from the quarter towards which they rushed. And *Aratus* agreed with him, especially if they left long lingering tails, in which he was imitated by *Virgil*. *Lucan* in his *Pharsalia* rather confounds meteors with the fixed stars. *Homer* compares the descent of Minerva to the rush of a meteor.

Passing over the middle ages, when meteors were feared as indications of Divine anger, we find that in the seventeenth century electricity began to be suspected, and was supported by the highest names of that æra. Then the magnificent meteor of the 18th of August 1783 brought out the elaborate paper by Dr. Blagden in the *Philosophical Transactions* for the following year. As to their velocity, it varies so much that this element cannot suffice to decide from what height they fall. The meteor above alluded to moved at only six miles per second when at about ninety miles above our heads. Cavallo estimated its diameter at 3200 feet, and its elevation at 560 miles. Certainly the explosion not being heard for ten minutes after it was seen is a sufficient proof of its distance. The general electric state of the atmosphere that year over half the globe is well known, by the remarks made in consequence of the violent earthquakes that occurred.

In support of the theory that meteors are occasioned by the ignition of columns of inflammable gas, Dr. Forster mentions the *ignis fatuus*, and the flitting lights that are seen in May on cabbages. Many naturalists regard meteors as one of the various phænomena attributable to electricity, and some expected to find that they chiefly pointed to the magnetic pole.

Many roofs of thatch have been ignited by the fall of meteors upon them, and this must be the explanation of towns recorded to have been burnt by fire from heaven. The explosion of the meteor of the 25th of September 1846, was heard *a few* seconds after it was seen: but if, instead of the ambiguous term *a few*, spectators would count slowly, they would afford a much nearer approach to the true time elapsed, especially if they would afterwards count at the same rate when they can compare with a seconds watch, or with a clock. (A.S.) The tail of that meteor was larger than usual, and lasted longer, some persons stating fifty seconds, others some minutes. More precise details are requisite. It was at first whitish, then purplish, and lastly red, when it became curved, and faded in a serpentine

form. This last phænomenon was observed in another instance about twenty years since. Even the luminous arc of the 28th of September 1828 might, our author thinks, be a still more dilatory tail of a meteor that had shot across our hemisphere just before sunset, and for that reason was not perceived. In July 1799 Dr. Forster's father saw a meteor cross the sky from south to north, then return southward, and finally bend to the north-west. Another peculiarity is that of rising in the sky instead of descending, which has been reported as occurring sometimes near the equator, where they are very numerous. And Dr. Forster himself saw a whitish globe stationary for two seconds, and then turn a fine red.

A shower of small meteors is recorded to have occurred on the 25th of April 1095; and Dr. Forster saw an approximation to this on a bright winter night in 1832, inasmuch as the whole firmament was in a glow from an immense number of very fine luminous tails nearly parallel from E.N.E. to W.S.W. They might deserve the name rather of streaks, no heads being visible. The duration of each might not exceed a second, but the phænomenon altogether lasted a quarter of an hour and then ceased suddenly. And in November 1830 he saw a similar multiplicity of little streaks, but crossed by others at right angles. Another peculiarity was described by a clergyman near Epping, that of seeing a meteor, after descending to the earth, undergo a sort of reverberation by rising in an oblique direction, and then break into sparks.

Among the numerous authors who have treated of this subject, perhaps M. Quetelet's catalogue is the most complete, with the exception of his omitting the interesting meteor of 1783. M. Arago and M. Biot have also treated the subject ably.

A copious journal of meteors has been kept in Dr. Forster's family from 1767, but no periodicity was suspected till the 10th of August 1811; though then, on looking back through the journal, it was perceived that there had been a great preponderance in the Novembers ever since 1799, and in the Augusts from 1779. When employed a few years after to construct perennial calendars, Dr. Forster indicated a number of meteors as a phænomenon to be expected on the 10th of August.

This became confirmed in 1831 by other observers, and they added the second period of the 13th of November. M. Quetelet now adds April and December, while others suggest January, May, June and July. He thinks their usual height in the atmosphere is from sixteen to twenty leagues or more, though they are occasionally seen slanting very near the ground. The most numerous sort, distinguished by the name of *étoiles filantes*, may revolve in trajectories by swarms, forming a belt round the sun, which we have occasionally to traverse. Then, owing to the earth's motion, these luminous corpuscles would naturally, as they have been observed to do, appear to "have their point of divergence towards  $\beta$  Camelop. in August, and towards  $\nu$  Leonis in November, agreeing with our annual motion in the ecliptic." According to the known laws of optics, the swarm would seem to separate in radii as we neared them, and,

owing to the compound velocities, seem to tend from N.E. to S.W.

Although meteors differ very much from each other in some instances, it is very difficult to classify them; but an abundance of them seems connected with a change of weather, and especially with cirrostratus and cirrocumulus clouds. As to their direction, though they sometimes converge towards one point, they rush at others towards every point of the compass. He therefore wavers only between an electric and a gaseous origin,—quoting electric experiments referred to in England by the Abbé Bertholon, and gaseous ones by Constable, as having produced excellent imitations.

In the terrible night of the 7th of July 1834, a crowd of nimbi collected around Vesuvius about 9 o'clock, shooting their lightnings down towards the mountain accompanied by rain and hail. The lightning was sometimes bluish and sometimes reddish.

As to the periodicity of meteors, Dr. Forster finds that there are decided changes in the electrometers also on the 10th of August and 13th of November; and the greatest number he ever saw fell on the 10th of August 1811, just after a violent storm; but when a storm has happened some time before, the meteors are fewer at the two periods observed. Also if one or more large meteors occur, there are no small ones afterwards for a proportionate time, as if the atmosphere had been cleared of the requisite material. Also it may be remarked in general, that the winter and the higher latitudes are least prolific of them.

Fiery balls do not often occur, but are very powerful. Thus the one seen in France and in England the 17th of July 1771, must have been at an elevation of fifty-four miles, and the report of its explosion was not heard till two minutes after its occurrence, like the rolling of thunder; but the observatory windows at Paris were “*ébranlées*.” It appeared larger and brighter than the  $\Delta$ , and its swiftness was estimated at twenty-four miles per second.

From the quickly-increasing rarity of our atmosphere, Arbuthnot thinks that at the height of sixty miles (the estimated height of the meteor in 1718) the air is 30,000 times purer than on the level of the sea. Yet Pringle estimated the height of the meteor of 1738 to have been ninety miles. The diameter of some globes has been estimated at  $1\frac{1}{2}$  mile.

### XXXVIII. *Proceedings of Learned Societies.*

#### ROYAL SOCIETY.

[Continued from p. 77.]

June 17, “**R**ESARCHES on the Function of the Intercostal Muscles and on the Respiratory Movements, with some remarks on Muscular Power, in Man.” By John Hutchinson, M.R.C.S. Communicated by Sir Benjamin Brodie, Bart., F.R.S., &c.

The object of this paper is to demonstrate by models and dissections the action of the intercostal muscles.

After premising an account of the views of several eminent physiologists, and in particular those promulgated by Haller, the author shows that they resolve themselves into the general opinion that the scalene or other muscles of the neck fix the first rib, in order to enable the two sets of intercostal muscles to act either separately or conjointly, as inspiratory or expiratory muscles. He then proceeds to state the proofs that the intercostal muscles possess an action which is independent of any other muscle, and also independent of each other, so that any of the twelve ribs may be elevated or depressed by them either separately or conjointly. He demonstrates the nature of this action by means of models, producing oblique tensions between levers representing the ribs, and allowing of rotation on their centres of motion; and he shows that such tension in the direction of the external intercostal muscles, elevates both the levers until the tension ceases, or the position of the bars by proximity obstruct each other. If the tension be exerted in a contrary direction, as in the internal intercostal muscles, the bars are both depressed. This movement was demonstrated by a model. It was farther shown that two tensions decussating can, according to the position of the fulcra, be made to act as associates or antagonists to each other. Such motions are to be considered with reference to the fulcra, bars with one fulcrum common to each having no such action; and the author accordingly draws the following conclusions:—

1st. All the external intercostal muscles are true inspiratory muscles, elevators of the ribs, and with this act they dilate the intercostal spaces, thus increasing the cavity of the chest.

2nd. The internal intercostal muscles have a double action; the portions situated between the cartilages are associates in action with the external layer, and act as elevators of the cartilages, while the portion between the ribs are depressors, or antagonists of the external layer, and are here true expiratory muscles; with this they decrease the intercostal spaces.

3rd. These muscles can elevate or depress the ribs independently of any other muscle, fixing the first or last rib. Any one lamella, or series of muscles, can, as required, independently perform inspiration or expiration at any one of the twenty-two intercostal spaces.

4th. In inspiration, the intercostal spaces increase, with a shortening of the muscle; and in expiration, they decrease their perpendicular distance, with a shortening of the muscle.

5th. All parallel intercostal muscles, acting with uniform force, concur in the same effect, whether near the fulcrum or more distant from it, and these muscles gain power with their increasing obliquity as well as speed.

In the third part of the paper an account is given of the difference between the external thoracic space and the internal pulmonic space. The respiratory movements are described in health and disease, and it is shown that the chest is rarely enlarged at two places at one and the same time.



In conclusion the author conceives that he has established the following propositions:—

1st. Costal breathing may be distinguished from abdominal by determining which part is first put in motion, and the kind of respiration may be designated according to the name of such part.

2nd. Healthy costal breathing begins with the motion of a superior rib, which is followed by that of the lower ones in succession.

3rd. Ordinary respiration in men is abdominal, in women, costal; extraordinary breathing is the same in both sexes.

4th. Any of the ribs, from the twelfth to the first, may carry on respiration.

5th. Diseased respiration is of various kinds; the movements may be symmetric or not symmetric, costal or abdominal; all or none of the ribs may move; the abdomen may or may not move; the chest may dilate in all its dimensions at one and the same time; costal and abdominal breathing may alternate with one another; costal motion may be undulating or not; and all these may be combined in one, which the author terms "*hesitating breathing*;" and lastly, the quantity of air breathed is diminished when there exists pulmonary disease.

"On the Structure and Development of the Liver." By C. Handfield Jones, M.B., Cantab. Communicated by Sir Benjamin C. Brodie, Bart., F.R.S., &c.

The author gives a detailed description of the structure of the liver in animals belonging to various classes of the animal kingdom. He states that in the Bryozoon, a highly organized polype, it is clearly of the follicular type; and that in the Asterias, the function of the liver is probably shared between the closed appendage of the stomach and the terminal cæca of the large ramifying prolongations of the digestive sac contained in the several rays. Among the Annulosa, the earthworm presents an arrangement of the elements of the hepatic organ, corresponding in simplicity with the general configuration of the body, a single layer of large biliary cells being applied as a kind of coating over the greater part of the intestinal canal. In another member of the same class, the Leech, in which the digestive cavity is much less simple, and presents a number of sacculi on each side, these elements have a very different disposition; and the secreting cells, although some remain isolated, for the most part coalesce to form tubes, having a succession of dilatations and constrictions, and finally uniting and opening into the intestine. In Insects, the usual arrangement is that of long curved filamentary tubes, which wind about the intestine; these, in the meat fly, are sacculated throughout the greater part of their course, till they arrive quite close to the pylorus, where they open; near their origin they appear to consist of separate vesicles, which become gradually fused together, but occasionally they are seen quite separate. The basement membrane of the tubes is strongly marked, and encloses a large quantity of granular matter of a yellowish tinge, with secreting cells; another portion of the liver consists of separate cells lying in a granular blastema, which cells, in a later stage

of development, are seen to be included in vesicles or short tubes of homogeneous membrane, often coalescing and exhibiting a more or less manifestly plexiform arrangement; this portion of the liver is regarded by Mr. Newport as really adipose tissue. The author has termed it the *Parenchymatous portion* of the liver, on account of its general appearance and mode of development, though he has not been able to determine whether the tubes always originate from it. Among the Arachnida, the follicular type of arrangement prevails; and the same is the case with the Crustacea, the follicles in these last being distinctly visible to the naked eye. In Mollusca also, we find the follicular arrangement universally to obtain; yet in certain cases the limiting membrane of the follicles cannot be shown to exist, and the author therefore thinks that its importance is probably not great, but that it serves chiefly to fulfil the mechanical function which its synonym "*basement*" indicates. The quantity of retained secretion in the liver of molluscs seems clearly to imply that the bile in them is not an excrementitious fluid; it is used slowly on account of the imperfect character of the respiration.

In passing from the Invertebrata to the Vertebrate division of the animal kingdom, and beginning with the class of Fishes, a great change is immediately manifest in the form and character of the biliary organ; it is now a gland of solid texture, to which the term *parenchymal* is justly applied. Two portions may be distinguished in it, namely, the secreting parenchyma, consisting of delicate cells, or very often of nuclei, granular and elaborated matters in great part, and the excreting ducts, which, though completely obscured by the surrounding bulky parenchyma, may yet be satisfactorily demonstrated, and traced often to their terminal extremities in the following manner. If a branch of the hepatic duct be taken up in the forceps, it may be dissected out without much difficulty from the surrounding substance, which is very soft and yields readily to gentle manipulation; when a trunk is in this way removed and placed under the microscope, a multitude of minute ramifications are seen adhering to it; among these not a few may be discovered, which do not appear to have suffered injury; some are occasionally seen terminating by distinctly closed extremities; more usually the duct becomes very minute and gradually loses all definite structure, appearing at last like a mere tract of granular matter; in either case there is no communication by continuity with the surrounding parenchyma. Large yellow corpuscles, peculiar cells, and a considerable quantity of free oily matter usually existing in the liver of various fishes, seem generally to indicate a great superiority in the amount of secretory over that of excretory action, and to betoken clearly the feeble intensity of the aërating function.

In Reptiles, there is the same arrangement in the liver, namely, a secreting parenchyma of cells and an apparatus of excretory ducts, which have the same essential characters as those of fishes; but there exists very frequently in the parenchyma remarkable dark corpuscles, which appear to be masses of retained biliary matter,

*Phil. Mag. S. 3. Vol. 31. No. 207. Sept. 1847.*

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the import of which, in the situation they occupy, is doubtless the same as that of the similar masses existing in fishes.

In Birds, the parenchyma of the liver is remarkably free from oily or retained biliary matters; it often consists almost wholly of free nuclei and granular matter, with scarcely a single perfect cell; the excretory ducts often greatly resemble those of reptiles, sometimes rather those of mammalia; the essential character is, however, always the same, namely, that they terminate without forming any important connexion with the parenchyma.

In Mammalia, the parenchyma of the liver consists usually of perfect cells, which are arranged often in linear series of considerable length, radiating from the axis of each lobule; these unite at various points with each other, so as to present a more or less decidedly plexiform appearance. Each lobule, as described by Mr. Kiernan, is separated from the adjacent ones by the terminal twigs of the portal vein, and to a greater or less extent by a "fissure," though in most animals the lobules are continuous with each other both above and below the fissure. The elaboration of the secreted product seems to be most completely effected in the cells adjoining the margins of the lobules, which are often seen to contain a larger quantity of biliary matter than those in the interior, and to be apparently in the act of discharging it into the fissure; the margin of the lobule then presents an irregular surface with large globules of the secretion clustering together all over it. The capsule of Glisson surrounding the vessels in the portal canals gives a fibrous investment to those surfaces of the lobules which are towards the canal; but when it has arrived in the fissures, it forms a continuous membrane lining the surfaces of opposite lobules; this membrane is often truly homogeneous, and closely resembles the basement tissue: there appears occasionally to be a delicate epithelium on its free surface; but this, as well as the membrane itself, is often absent, when the margin of the lobules is in that condition which has just been described and which may be termed *active*. The minute branches of the hepatic duct as they approach their termination undergo a remarkable alteration in their structure; they lose their fibrous coat, which blends itself with the membranous expansions of the capsule of Glisson; their basement membrane becomes gradually indistinct, and at last ceases to exist, and the epithelial particles no longer retain their individuality, but appear to be reduced to mere nuclei, set very close together in a faintly granular basis substance. The mode of their termination is not uniformly the same; frequently they present distinctly closed rounded extremities, between one and two thousandths of an inch in diameter; at other times they seem to cease gradually in the midst of fibrous tissue, the nuclei alone being disposed for some little way in such a manner as to convey the idea of a continuation of the duct. These ducts can seldom be discerned in the fissures, but have several times been seen in the "spaces," where several fissures unite; they do not form anything like a plexus between the lobules. From the anatomical relation of the ducts to the parenchyma, and from the circumstance that a

distinct vessel conveying a different kind of blood is distributed to the hepatic duct, as soon as the liver assumes the parenchymal form, it seems probable that the mode in which the secreted bile is conveyed out of the organ, is by its permeating the coats of the minute ducts in obedience to an endosmotic attraction, which takes place between the bile in which the ducts may be said to be bathed, and a denser (perhaps mucous) fluid formed in their interior. The large quantity of oily matter frequently existing in a free state in the secreting parenchyma of the liver, which must be regarded as a product of secretory action, seems to suggest the idea, that a certain quantity of the biliary secretion may be directly absorbed into the blood, and in this manner conveyed away from the organs, just as occurs in the thyroid body, suprarenal capsules, and other glands unprovided with efferent ducts.

With respect to the development of the liver, the author considers the opinion of Reichart to be decidedly the correct one, namely, that its formation commences by a cellular growth from the germinal membrane, independently of any protrusion of the intestinal canal. On the morning of the fifth day, the œsophagus and stomach are clearly discernible, the liver lying between the heart, which is in front, and the stomach which is behind; it is manifestly a parenchymal mass, and its border is quite distinct and separate from the digestive canal; at this period, the vitelline duct is wide, it does not open into the abdominal cavity, but its canal is continued into an anterior and posterior division, which are tubes of homogeneous membrane, filled, like the duct, with opaque oily contents; the anterior one runs forwards, and forms behind the liver a terminal expanded cavity, from which then passes one offset, which, gradually dilating, opens into the stomach; a second, which runs in a direction upwards and backwards, and forms apparently a cæcal prolongation; and a third and fourth, which are of smaller size, arise from the anterior part of the cavity and run to the liver, though they cannot be seen to ramify in its substance; at a somewhat later period, these offsets waste away, excepting the one which is continued into the stomach, and then the mass of the liver is completely free and unconnected with any part of the intestine. As the vitelline duct contracts, the anterior and posterior prolongations of it become fairly continuous and form a loop of intestine, the posterior division being evidently destined to form the cloaca and lower part of the canal. The final development of the hepatic duct takes place about the ninth day by a growth proceeding from the liver itself, and consisting of exactly similar material; this growth extends towards the lower part of the loop of duodenum, which is now distinct, and appears to blend with the coats of the intestine; around it, at its lower part, the structure of the pancreas is seen to be in process of formation. The further progress of development of the hepatic duct will, the author thinks, require to be carefully examined, but the details he has given in this paper have satisfied him of the correctness of the statement that the structure of the liver is essentially parenchymal.



XXXIX. *Intelligence and Miscellaneous Articles.*

SUGGESTIONS FOR THE OBSERVATION OF THE ANNULAR ECLIPSE, OCT. 9, 1847, MADE BY THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, OXFORD, JUNE 26, 1847.

THE following directions and suggestions, relative to the ensuing annular eclipse of the sun, which will take place Oct. 9, 1847, are proposed for the assistance of less-practised observers, or those who may not have better information at hand, but who may nevertheless render great service by noticing and recording, as well as circumstances permit, any of the various points herein alluded to.

I. As a general direction as to the limits within which the eclipse can be seen annular in England and Ireland, if on any map a line be drawn through Greenwich and Gloucester and produced, it will give the northern limit at which the eclipse ceases to be annular.

A line parallel to the last, through Padstow in Cornwall on the west, and Torbay on the east (which will extend across the channel to Havre, &c., and passes just below Cape Clear on the west), will be the line along which the eclipse is both annular and *central*.

The southern limit lies wholly below England.

II. As a rough guide to the *time*, the commencement of the annulus will be nearly at 7<sup>h</sup> 23<sup>m</sup> A.M. (civil reckoning) for the extreme south-west of Ireland, at 7<sup>h</sup> 24<sup>m</sup> for a line through Land's End and Milford Haven, at 7<sup>h</sup> 25<sup>m</sup> through the Isle of Wight and Reading, at 7<sup>h</sup> 25<sup>m</sup> 50<sup>s</sup> for Walmer (Greenwich mean time).

III. For the observations requisite, a telescope of very moderate power is best. As the annulus will not last more than three or four minutes, those unaccustomed to such observations should be cautioned against attempting to observe *all* the phenomena, or they may thus run the risk of observing *none*. If possible several observers should combine for the purpose, and *each* agree to attend to *one*, or *some few* of the phenomena.

IV. To obviate some of the difficulties arising from the rapid passage of the phenomenon, the observer may be referred to Capt. Smyth's Cycle (i. 141, 146), where some valuable practical hints are thrown out for tranquillizing the observer's nerves in so transitory a phenomenon; especially by previously making a careful drawing of the spots (if any) existing on the sun's disc, which may be made useful in marking and ascertaining the progress of the eclipse.

V. With the view of correcting the moon's tabular north polar distance and semidiameter, it is peculiarly desirable that observations should be made along or near the line (passing through Greenwich and Gloucester) on which the eclipse is barely annular. At some of these the eclipse will be completely annular, and here the following observations should be made:—

The time of beginning of annularity and end of annularity should be observed. As the duration only is required, a common watch showing seconds will suffice for this purpose.

If possible, by means of a graduated pearl scale or other equivalent means, the breadth of the narrowest part of the annulus should be measured several times about the middle of the time of the annular appearance, as well as it can be estimated.

At other places the eclipse will not be completely annular, and here the principal object must be to make several measures of the distance between the cusps about the time when that distance is smallest. This measure may probably be made by means of a graduated pearl scale, or by means of a divided object-glass applied in front of the object-glass of the telescope, or by the use of a common sextant.

VI. As to the particular points of physical interest to which attention should be directed, they may be stated as follows:—

1. It will be desirable in general to notice *the fact* of the appearance of what are denominated “beads” and “threads” by the late Mr. Baily and others, just before and after the completion of the annulus.

For details of older observations the observer should consult

Ast. Soc. Memoirs, i. 142–146, x. 10–17, 33–38.

The beads were observed by Mr. Baily, *ib.* x. 210, in 1842, when they were not seen by Mr. Airy, *ib.* x. 218.

They were observed by Prof. Henderson at Edinburgh. Ast. Soc. Notices, v. 186.

2. Whether in the neighbourhood of the cusp the limb either of the sun or moon appears distorted?

Whether the beads appear *steady* or *waving*, *disappearing* and *reappearing*, &c.?

See the observations of Mr. Caldecott at Trevandrum, Ast. Soc. Notices, vi. 81.

Whether they present any peculiar changes when viewed through differently coloured glasses, the observer alternating the colours, which should be as dissimilar as possible, such as red and green?

See Silliman's Journal, Jan. 1842.

3. Whether they are seen when the eclipse is projected on a *screen*?

In this way Prof. Chevallier saw none when others with coloured glasses saw them. Ast. Soc. Notices, v. 186.

4. The drawing out of the beads into *threads* when very near junction; and whether they *waver* and *change*, and the number of them?

See Ast. Soc. Mem., x. 15–17, 39; *waving* and *changing*, *ib.* x. 12, 13; not seen in 1842 by Mr. Baily, Notices, v. 210.

5. Whether before and after the formation of the threads the moon's dark disc is *elongated* towards the point of contact?

This was observed, *ib.* x. 29; and *wavy motion* in the limb, *ib.* x. 12, 14, 30.

6. The beads are ascribed by some to *lunar mountains*: What mountains exist at that part of the limb?

See Ast. Soc. Mem., x. 9, 16, 30–36.

7. The exact intervals of time elapsed between the first and last complete contact, and that of the first and last formation of beads or other irregularities in or about the cusps, should be determined. The difference of the times being all that is wanted, a good ordinary watch will be sufficient.

The remarkable fact of a *recurrence* of cusps observed by Mr. Airy in 1842, and his explanation of it, should be attentively considered. See Ast. Soc. Notices, v. 296.

8. If possible, accurate *measures* should be taken of the *apparent diameter* of the dark disc of the moon upon the sun, which may be expected to be greatly less than the truth, owing to the irradiation of the sun's light.

9. It should be noticed whether any external *luminous arch* is formed over the part between the cusps, a little before the first junction and after the final separation, and the *colour* of the light.

It was observed, and appeared *brown* to De Lisle (Phil. Trans., 1748, 490), *reddish* in other cases (Ast. Soc. Mem., i. 144, x. 37), and *purple* in others (ib. x. 16).

#### ON THE PREPARATION AND COMPOSITION OF THE SALTS OF ANTIMONY. BY M. E. PELIGOT.

*Sulphates of Antimony.*—When oxychloride of antimony ( $\text{ClSb}^3\text{O}^3$ ) is treated with hot concentrated sulphuric acid, a salt is formed which is deposited in acicular crystals, hydrochloric acid being at the same time evolved. This salt, as well as another sulphate to be described, can only be obtained in a dry state by long remaining *in vacuo*, or in perfectly dry air upon porous plates of pipe clay. These plates were heated to redness before the crystalline magma was placed upon them, and they were left to cool in air deprived of moisture. This method of drying yields products which usually contain a slight excess of sulphuric acid. If however the points of contact between the salt to be dried and the absorbent earth be renewed from time to time, and the absorption goes on for several months, compounds of sufficient purity to remove all doubts of their true composition may be obtained.

One hundred parts of the sulphate of antimony, obtained by common sulphuric acid and oxychloride of antimony, gave—

Sulphuric acid . . . . .	51.9
Oxide of antimony (by carbonate of ammonia) . . . .	50.2

The composition of this salt is therefore—

$4\text{SO}^3$ . . . . .	2000	51.2
$\text{Sb}^3\text{O}^3$ . . . . .	1912	48.8
	<hr/>	<hr/>
	3912	100.0

Another specimen gave 53.1 of sulphuric acid, and 44.3 of oxide of antimony.

Another sulphate of antimony was obtained in the form of small brilliant crystals, by treating sesquioxide of antimony with Nordhausen sulphuric acid. After remaining ten months on the dried clay, it gave—

Sesquioxide of antimony . . . .	63.0	64.3
Sulphuric acid . . . . .	37.1	35.0

The formula  $2\text{SO}^3, \text{Sb}^3\text{O}^3$  gives 65.6 oxide of antimony and 34.4 sulphuric acid.

Mixtures of these salts in different proportions were also obtained ;

but no analysis indicated the existence of the compound  $3\text{SO}^3$ ,  $5\text{Sb}^2\text{O}^3$ , which, according to Berzelius, would be the neutral sulphate of antimony.

On treating the above-described salts with hot water, a subsalt is obtained, the composition of which is represented by the formula—

		Calculation.	Experiments.
$2\text{Sb}^2\text{O}^3$ .....	3824	88.4	88.6
$\text{SO}^3$ .....	500	11.6	11.4
	<u>4324</u>	<u>100.0</u>	<u>100.0</u>

The analysis of two other specimens is correctly represented by the formula  $2\text{Sb}^2\text{O}^3, \text{SO}^3, 2\text{HO}$ .

*Nitrate of Antimony.*—This salt was obtained in the form of pearly crystals by dissolving the oxide in cold fuming nitric acid, and adding water to the solution. Its composition is  $2\text{Sb}^2\text{O}^3, \text{NO}^3$ .

*Oxychlorides of Antimony.*—Powder of Algaroth was prepared by treating chloride of antimony with cold water. After some days the mass became crystalline; when well-washed its composition agreed with the analyses which have served to fix the formula of this compound. This formula is more simply replaced by  $\text{Cl Sb}^2\text{O}^3$ .

When the sesquichloride of antimony, or rather the sesquioxide dissolved in a great excess of hydrochloric acid, is treated with hot water, another oxychloride is obtained, which, on the cooling of the liquor, precipitates in dense brilliant crystals. Its composition is represented by the following formula:—

		Calculation.	Experiments.	
$\text{Cl}$ .....	443	10.6	11.1	11.4
$4\text{Sb}$ .....	3224	77.3	76.5	76.8
$\text{O}^3$ .....	500	12.1		
	<u>4167</u>	<u>100.0</u>		

This compound consequently must bere presented by the formula,  $\text{Cl Sb}^2\text{O}^3 + \text{Sb}^2\text{O}^3$ .

*Tartrates of Antimony.*—By allowing a syrupy solution of tartrate of antimony, obtained by dissolving the oxide of the metal in tartaric acid, to remain for a long time, large transparent crystals of tartrate of antimony were obtained. The mother-water, after the separation of the crystals, furnished more afterwards by spontaneous evaporation.

This salt is very soluble in water. It is deliquescent in a moist atmosphere. Its composition is represented by the following formula:—

		Calculation.	Experiments.	
$\text{C}^{16}$ .....	1200	19.6	18.9	19.0
$\text{H}^{16}$ .....	200	3.2	3.5	3.5
$\text{O}^{29}$ .....	2800	46.0		
$\text{Sb}^2\text{O}^3$ .....	1912	31.2	31.5	
	<u>6112</u>	<u>100.0</u>		

At  $320^\circ\text{F}$ . this salt lost 23.1 per cent. of water.



On decomposing the formula as follows, the loss of twelve equivalents of water represents 22 per cent. of the weight of the salt—



On pouring alcohol into a concentrated solution of the acidulous tartrate of antimony, a precipitate is obtained which, when dried at 320° F., yielded 16.4 of carbon and 1.3 of hydrogen. The composition of this salt is represented by the formula  $C^4 H^2 O^8, Sb^2 O^3, HO$ , which requires 17.2 of carbon and 1 of hydrogen. The salt which M. Peligot analysed contained a little more water than the quantity required by this formula, but not enough to allow of the addition of another equivalent.

*Acidulous Tartrate of Antimony and Potash.*—This salt was described by M. Knapp, who obtained it by mixing solutions of tartaric acid and tartarized antimony. The salt which was analysed by M. Peligot was in very regular crystals. It yielded—

Carbon .....	19.5	18.7
Hydrogen .....	2.7	2.7
Sesquioxide of antimony	31.0	

The formula  $C^{16} H^4 O^{16}, Sb^3 O^3, KO, 8HO$  represents its composition. It gives—

Carbon .....	19.1
Hydrogen .....	2.3
Sesquioxide of antimony ..	30.5

According to M. Knapp it contains one equivalent less of water.

*Oxalate of Antimony.*—M. Peligot prepared this salt by four processes:—1st, by boiling in a solution of oxalic acid oxide of antimony prepared from the chloride by carbonate of ammonia; 2nd, by treating the powder of Algaroth with oxalic acid; 3rd, by pouring hydrochloric acid into a hot solution of the double oxalate of potash and antimony; the oxalate of antimony precipitates in the state of a crystalline powder; 4th, by adding oxalic acid to a solution of the same double salt.

The oxalates of antimony obtained by these processes are similar in composition. The author attempted, but in vain, by varying the proportions, to obtain other compounds of oxalic acid and oxide of antimony. This salt is crystalline and insoluble in water. It is decomposed by boiling water into oxalic acid, which dissolves, and sesquioxide of antimony.

Its composition is represented by the following formula:—

		Calculated.	Experiments.		
C <sup>4</sup> .....	300.0	10.2	10.1	10.6	10.6
O <sup>6</sup> .....	600.0	20.6			
Sb <sup>2</sup> O <sup>3</sup> ....	1912.9	65.4	66.7	65.6	
HO .....	112.5	3.8	3.8	4.5	4.0
		2925.4	100.0		

*Double Oxalate of Potash and Antimony.*—The preparation and analysis of this salt are very difficult. The salt obtained by M. Pe-

ligot was crystallized in transparent prisms; it is readily soluble, and is decomposed by a large quantity of water.

The quantity of water in this salt appeared to vary from unknown causes, but apparently dependent on the temperature at which the salt crystallizes. The formula appeared to be  $7C^2O^3, Sb^2O^3, 3KO, 6HO$ . This gives as the composition of 100 parts of the salt—

Carbon .....	13·9
Water .....	9·0
Oxide of antimony .....	25·7
Potash .....	23·5

M. Peligot obtained—

Carbon.....	13·7	14·3	14·4	14·0
Water .....	9·7	9·2	10·1	8·9
Oxide of antimony ....	25·7	26·2	24·8	

*Ann. de Ch. et de Phys.*, Juillet 1847.

#### ACTION OF HYDROCHLORIC ACID IN THE FORMATION OF OXALIC ACID.

M. Kopp states that the presence of hydrochloric acid in nitric acid is peculiarly favourable to the formation of oxalic acid. The resins of benzoin and Tolu, treated with pure nitric acid, yield no oxalic acid; but with an impure acid it is obtained. Pure nitric acid occasions the formation of terebic acid only, in acting upon oil of turpentine, and to oxypicric acid, in oxidizing the gum-resins. By using nitric acid containing much hydrochloric acid, oxalic acid only is obtained under the same circumstances.—*Ibid*, Juillet 1847.

#### PROJECTION OF ALDEBARAN ON THE MOON.

At the British Association in Oxford a question arose respecting the apparent projection of Aldebaran on the disc of the moon in occultations. Prof. Airy and Dr. Forster stated having seen this phænomenon, which Prof. Struve seemed disposed to attribute to to some mal-adjustment of the telescopes. On looking back, however, to the Philosophical Magazine, it will be found that this appearance has been three or four times recorded; as well as some other circumstances calculated to show that the light of different stars is very differently refracted. See Phil. Mag. for April and May 1824.

#### THE PUFF PARLIAMENTARY:—DISINFECTION.

The art of puffing has not yet exhausted its resources; and a Parliamentary Report well got up, printed at the expense of the public, and from which extracts may go the round of the newspapers, seems to be the last and boldest device for the purpose, which however has been fearfully exposed in the Dublin Quarterly Journal of Medical Science.

The Times newspaper in a leading article of the 20th of August, felicitates itself on having “the pleasant task of giving what publicity it may to a discovery made by a French gentleman, M. Ledoyen,

a Parisian chemist, in concert, it would appear, with a Mr. F. C. Calvert, who seems to have received his education as a chemist at Paris, and who is now lecturer at the Royal Institution of Manchester. This discovery, which, under the auspices of Lord Morpeth, has been submitted to the most searching tests by Dr. Southwood Smith, Mr. Toynbee and Mr. Grainger, promises fair to be *one of the greatest boons ever conferred on suffering humanity*. The discovery is nothing less than the means of disinfecting all foetid animal substances and gases by a liquid which is very cheap, simple, and can be applied by any person with the greatest facility.

"The three medical gentlemen appointed by Lord Morpeth to inquire into the real value of M. Ledoyen's discovery, present us in their report with a dismal catalogue of the offensive and dangerous vapours from animal and vegetable substances which at all hours infect the air we breathe, in a greater or less degree, accordingly as we more or less neglect their impure origins." "The Commissioners state that they have tried the effect of this fluid,—1, on substances already in a state of decomposition; 2, on substances undergoing that process; 3, on night soil; 4, on impure air. In every instance excepting the second these experiments have been attended with the *most miraculous result*." "It would almost seem that *some mysterious power* had sent us M. Ledoyen and his discovery to compensate for the shortcomings of the Premier and Lord Morpeth\*."

So far *The Times*.—We now give a few extracts from the *Dublin Journal*, and refer our readers to the article which it contains for the details of the means by which these puffs have been procured, and for a full account of the matter.

This boasted discovery professes to furnish "the means of disinfecting all foetid animal substances and gases by a liquid which is very cheap, simple, and can be applied by any person with the greatest facility. It disinfects night-soil, not destroying but increasing vegetation, more particularly as regards agriculture, completely preventing the disease in potatoes when the land is manured with disinfected night-soil. It disinfects hospital-wards of miasma; also cellars, water-closets, and buildings infected by impure gases. It disinfects sailors suffering from fever on board of vessels; it will also disinfect ships at sea, and under quarantine. It disinfects patients suffering with infectious disorders and wounds, also dead bodies, so that they may be kept nearly a month; also different parts of the body can be kept for the purposes of dissection, for coroners' inquests, &c."

\* No wonder that competitors should have started up asserting their claims to so wonderful a discovery. Mr. W. Maddick thus begins his letter to the Editor of the *Times*; of whose judgement in matters of science he seems to have a most exalted opinion:

"SIR,—All the world knows that a laudatory notice in your columns is a very high honour; and as in your excellent leader of yesterday you have highly eulogized Messrs. Ledoyen and Calvert for their alleged discovery, I appeal with confidence to the proverbial justice of *The Times*, &c. &c."

"I boldly claim originality in this matter, and challenge these gentlemen, or any other," &c., &c.

"There is not a word of evidence in the document before us as to the influence of this solution of nitrate of lead in curing, or 'disinfecting,' as they call it, by its vicinity, fever or other infectious diseases. Of course no professional man (except Dr. Southwood Smith) could bring himself to support such an absurdity as that would amount to. With respect to the potatoe disease, Dr. Smith has been even less guarded. He manured portions of his garden with his disinfected night-soil, and finds that potatoes grown on these spots are finer than elsewhere. He says, 'I have this day had specimens of them examined by Mr. Alfred Smeë, who pronounces them to be at present perfectly healthy.' What! not a single *Aphis vastator*! Oh, genius of humbug! how numerous are thy votaries! Truly, successful speculation constitutes the idolatry of this age, and the wonder-workings of pseudo-science its superstition."

"Let us now briefly pass in review some of the evidence detailed in this precious document—some of the 'Letters and Reports received by the Chief Commissioner of Woods and Forests,'—set forward in a parliamentary folio, gravely ordered to be printed by the British senate, and consequently paid for by the country. Always premising that we do not deny to this, in common with many other chemical substances\*, the power of destroying some unpleasant odours, or, to deal more in the phraseology of the Report, *stinks*. But against the disgraceful quackery with which this book abounds,—a quackery not equalled by the most offensive and indecent advertisement,—and the humbug of presenting such a book to the country, we loudly and strongly protest."

"We have already alluded to the circumstance that this imposture has been attempted to be bolstered up by the testimony of night-men, dissecting-room porters, ward-men, and other respectable authorities of a similar kind. Some of the experiments made by these intellectual and educated individuals may amuse our readers, as they have doubtless enlivened the House of Commons. Speaking of the contents of a privy—

"'William Fenwick did, as you gentlemen saw, *taste* it, and William Dyer put some over his eyes without injuring them: if it had not gone through your process, it would have *blinded* him!!'

"We cannot however pursue a strain of levity when we come to examine the part which a physician of repute has taken in this transaction. Dr. Southwood Smith, not content with bearing his share in the fooleries of the Report already spoken of, volunteers his individual testimony as to the efficacy of the fluid in obviating contagion among the medical and non-medical attendants on the sick.

"'Whatever difficulties,' he writes, 'your Lordship may have encountered in obtaining the necessary powers to make even any com-

\* Sulphate of copper, nitrate of copper, chloride of copper, super-nitrate of bismuth, nitrate of lead, nitrate of silver, chloride of gold, protochloride of tin, perchloride of tin, nitrate of mercury. This fluid has been examined by Dr. Aldridge, and found to be a solution of nitrate of lead. Sir W. Burnett has introduced the chloride of zinc for similar purposes in the navy.



mencement of a system of prevention by the removal of the causes of fever, you have in your own hands, and have had for some months, the sure and certain means of preventing the extension of fever to the immediate attendants on the sick.'

"In the columns of newspapers, in the pages of journals, on the covers of magazines, in the corners of railway guides, placarded on dead walls and bankrupts' shop-windows, dropped into the hat at public meetings, thrust into the hand in streets, and forced upon the attention at every turn, we thought all the modes of puffing quack advertisements and indecent labels, either in prose or rhyme, had been exhausted: but we find that we were mistaken. A novelty in this department has been introduced by Colonel Calvert; and in the pages of a parliamentary report\* we see puffs as gross, and language as indelicate, as any that disfigure the lowest newspapers."

We can only add an expression of our regret that an important public cause, that of sanitary improvements, should have to encounter prejudices raised against it from the exaggerations, misrepresentations, quackery, and jobbing which are too manifest in the conduct of some of its advocates.

#### A GRANT OF 200*l.* TO MR. WILLIAM STURGEON.

We are glad to learn, from a communication dated Downing Street, 12th August, from Colonel Grey, the private secretary of Lord John Russell, that his Lordship has been pleased to grant the sum of 200*l.*, from the Royal Bounty Fund, to Mr. William Sturgeon of this town. Mr. Sturgeon was formerly lecturer on experimental philosophy at the Hon. East India Company's Military Academy, Addiscombe; and since his residence in Manchester, now extending over a number of years, he has been superintendent of the Victoria Gallery, delivering various courses of lectures there; and subsequently he filled the office of lecturer to the Manchester Institute of Natural and Experimental Science. For a long series of years Mr. Sturgeon has honourably distinguished himself by his investigations and discoveries in the various branches of electrical science, especially in electro-magnetism and thermo-electricity.

#### OBSERVATIONS ON CREATINE. BY M. HEINTZ.

About two years ago I described a peculiar substance which I had discovered in the normal urine of man. From subsequent investigations I find that this substance is identical with that which M. Chevreul found in meat broth, to which he gave the name of creatine, and the presence of which in the fresh muscular flesh of different animals has recently been shown by Liebig.

The most advantageous method of procuring the substance is that subsequently pointed out by M. Pettenkofer; it consists in adding

\* That Parliamentary Reports are sometimes made vehicles of privileged detraction and calumny the public are already aware. A late instance with regard to the Greenwich Observatory has been exposed by the Astronomer Royal.

to the alcoholic extract of the urine an alcoholic solution of chloride of zinc; in a short time a deposit is formed, which contains the creatine in combination with the chloride of zinc, together with a small quantity of phosphate of zinc. These two substances are separated by boiling water, which dissolves the first, but is without action upon the latter. The pure creatine is obtained from the aqueous solution of its combination with chloride of zinc by precipitating the zinc with hydrosulphate of ammonia; after having evaporated the filtered liquid as far as possible without a precipitate being formed in the boiling solution, absolute alcohol is added to it, when the creatine is immediately deposited in the form of small crystals, resembling those obtained in operating upon the alcoholic solution of the aqueous extract of meat.

After having washed these crystals with alcohol, I recrystallized them from water. The elementary analysis of the pure crystals led to the following formula,  $C^8 H^9 N^3 O^4 + 2HO$ , which is the same as that advanced by M. Liebig.

When creatine enters into combination with chloride of zinc, it parts with 2 atoms of water besides the water of crystallization, and in exchange takes up 1 atom of this salt. This combination is represented by the following formula,  $C^8 H^7 N^3 O^2 + ClZn$ , and the atomic weight of creatine is consequently 1412.5.

From the experiments of M. Liebig it results, that of all the organs of the animal body it is only the muscles which yield creatine. Now, as I have proved its presence in the urine of man and animals, it appears placed beyond all doubt that this substance is formed in the muscles, that it is absorbed by the lymphatics or blood-vessels, and is finally secreted by the kidneys, like urea, &c. We may therefore conclude that creatine should henceforth be placed amongst the *excrementitious* substances; and consequently it is barely probable that it constitutes one of the most important alimentary principles of meat broth, as M. Liebig is inclined to think. Is it not rather one of the ultimate products of the chemical actions, the presence of which we have great reason to suspect in the act of muscular contraction?—*Comptes Rendus*, March 22, 1847.

#### THE NEW PLANET IRIS.

The following letter to The Times appeared on Wednesday, Aug. 18th.

SIR,—In addition to the Berlin maps, which we have revised, and in some instances corrected, ecliptical charts of stars down to the tenth magnitude have been formed for some of the hours of right ascension, which it is Mr. Bishop's intention to publish as soon as they are completed. On the 13th of August I compared Wolfer's map with the heavens, and was surprised to find an unmarked star of 8.9 magnitude in a position which was examined on June 22 and July 31 without any note being made. The mere existence of a star in a position where before there was none visible, would not have been sufficient to satisfy me as to its nature; because during an eight months' search I have met with very many variable stars,—a

class which I believe to be far more numerous than is generally supposed. But, on employing the wire micrometer we were enabled in less than half an hour to establish its motion, and thus to convince ourselves that I had been fortunate enough to discover a new member of the planetary system. It may appear to many of your readers rather bold to announce the existence of a new planet from the detection of so small an amount of motion as 2 s. 5 in. R.A. ; but such is the firm mounting of the large refracting telescope, and the perfection of the micrometers (for which we have to thank Mr. Dollond), that a far smaller change would have been sufficient to convince us as to the nature of the object in question. Mr. Bishop has fixed upon Iris as an appropriate name for the new planet ; and we hope that astronomers generally will join with us in its adoption. The following are all the observations we have yet made :—

	O. M. T.			R.A. of Iris.			
	h.	m.	s.	h.	m.	s.	
Aug. 13,	9	39	46	19	57	30.38	13 27 21.5
— 13,	10	37	24	19	57	28.41	13 27 27.6
— 14,	9	23	58	19	56	38.30	13 29 14.0
— 15,	9	0	39	19	55	47.64	13 31 4.3

I remain, Sir, your most obedient Servant,

Mr. Bishop's Observatory, Regent's Park,

J. R. HIND.

Aug. 17.

We have been favoured with the following additional information by Mr. Hind :—

The planet was observed by Mr. Rümker at Hamburg, on Aug. 20, and by Prof. Gauss at Göttingen and Prof. Encke at Berlin, on Aug. 21. M. Leverrier announced the discovery to the Paris Academy of Sciences on Aug. 16, giving at the same time a general view of the various hypotheses which have been started respecting the group of small planets. The orbit of Iris appears to be very excentric, and the period longer than that of any other asteroid ; but further observations are required for the accurate determination of the elements.

Prof. Schumacher's " Planeten-Circular " was despatched from Altona on August 20, so that we may expect a general series of meridian observations at the various European observatories during the present apparition of the planet.

#### SUGGESTIONS FOR PROMOTING THE SCIENCE OF METEOROLOGY.

*To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,

As I find the Meteorological Society is defunct, I beg leave to suggest that in order that the science of meteorology may be improved and promoted, and not left to chance, and in order that uniformity in the observations may be obtained, I propose that at the several railway stations, the head clerk, or the cleverest man on the premises, be supplied gratis with proper instruments, and that these instruments should all be supplied by the same maker ; then will they all start fair, upon certain data, which by the present system cannot be done. And as my friend Mr. Luke Howard has suggested to

me, would not the electric telegraph be a capital means of transmitting the intelligence of a thunder or hailstorm, or any change that has taken place in any part of the kingdom where railways obtain, and by that means unravel nature's secret with regard to meteorological phenomena?

But as those stations should be provided with every requisite for taking all the necessary observations, so as to form a compendious series of meteorological remarks, you will ask who is to furnish the means? In answer to that I would say, could not the Royal Society do that, and might not the British Association take the concern under their fostering care? You will also say, would not the attention necessary to be paid to these observations lead to inattention in respect to the trains? I hope not; and I believe, before long, such improvements will be made in railways as to make it nearly a physical impossibility for accidents to occur.

Boston, July 3, 1847.

SAMUEL VEALL.

# METEOROLOGICAL OBSERVATIONS FOR JULY 1847.

*Chiswick.*—July 1. Light clouds: fine: overcast. 2. Slight drizzle: cloudy. 3. Overcast: clear. 4. Very fine: clear: cloudy. 5. Sultry. 6. Very fine. 7. Overcast: slight shower. 8. Rain: cloudy: clear. 9. Cloudy and fine. 10. Overcast: clear. 11, 12. Very fine. 13. Sultry. 14—16. Excessively hot. 17. Thunder, lightning and heavy rain all the morning: fine: cloudy. 18. Cloudy. 19. Slight showers. 20. Overcast and fine. 21. Very fine. 22. Heavy clouds: clear at night. 23, 24. Very fine. 25. Overcast. 26. Clear and fine. 27, 28. Very fine. 29. Sultry. 30, 31. Very fine.

Mean temperature of the month ..... 65°·84

Mean temperature of July 1846 ..... 65 ·46

Mean temperature of July for the last twenty years ..... 63 ·08

Average amount of rain in July ..... 2·36 inches.

*Boston.*—July 1—3. Cloudy. 4, 5. Fine. 6. Fine: half-past 2 p.m. thermometer 76°. 7. Fine: rain early this morning. 8. Cloudy: tremendous storm of thunder, lightning and rain p.m. 9. Fine. 10, 11. Cloudy. 12. Fine: 4 p.m. thermometer 81°. 13. Fine. 14. Cloudy. 15. Cloudy: 3 p.m. thermometer 74°. 16, 17. Cloudy. 18—21. Fine. 22. Rain. 23. Cloudy. 24—27. Fine. 28, 29. Cloudy. 30, 31. Fine.

*Sandwick Manse, Orkney.*—July 1. Cloudy. 2, 3. Fog: fine. 4. Damp: cloudy. 5. Cloudy: fog. 6. Fog. 7. Drops. 8. Rain: clear. 9. Bright: fine. 10. Fog: bright: fine. 11. Bright: fine. 12. Clear: fine. 13. Damp: cloudy. 14. Bright: showers. 15. Clear: fine. 16. Bright: fine. 17. Cloudy. 18. Rain. 19. Drizzle: damp. 20. Drizzle: cloudy. 21. Drizzle: fog. 22. Showers: rain. 23. Cloudy: showers. 24. Cloudy: fine. 25. Fine. 26. Bright: drizzle. 27. Rain: cloudy. 28. Showers. 29. Showers: clear. 30. Bright: showers. 31. Bright: rain.

*Applegarth Manse, Dumfries-shire.*—July 1. Very fine: thunder. 2—4. Very fine. 5. Very fine: mackerel sky and sultry p.m. 6. Very fine. 7, 8. Heavy showers: thunder. 9. Cloudy and threatening. 10. Rain. 11. Rain: fog p.m. 12. Fine, but cloudy. 13. Very fine: fog early a.m. 14. Heavy dew: very fine. 15. Very fine: shower and thunder. 16. Cool and breezy: thunder. 17. Very fine: air elastic. 18. Very fine: drizzle p.m. 19, 20. Very fine. 21. Fine, but cloudy: shower and thunder. 22. Showers: refreshing. 23. Fair and fine. 24. Fair and fine, but dull. 25. Shower early a.m.: fine. 26. Fine bracing air. 27. Cloudy: threatening: thunder. 28. Fair, but cloudy. 29—31. Fair, but cloudy: unsettled.

Mean temperature of the month ..... 61°·55

Mean temperature of July 1846 ..... 59 ·20

Mean temperature of July for twenty-five years ..... 58 ·14

Average rain for twenty years ..... 3·91 inches.



Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at Chiswick, near London; by Mr. Veall, at Boston; by the Rev. W. Dunbar, at Applegarth Manse, Dumfries-shire; and by the Rev. C. Clouston, at Sandwick Manse, Orkney.

Days of Month.	Barometer.						Thermometer.						Wind.			Rain.		
	Chiswick.		Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Dumfries-shire.		Orkney, Sandwick.		Chiswick, 1 p.m.	Dumfries-shire.	Orkney, Sandwick.	Chiswick.	Dumfries-shire.	Orkney, Sandwick.
	Max.	Min.	8 1/2 a.m.	9 a.m.	9 p.m.	8 1/2 p.m.	Max.	Min.	8 1/2 a.m.	Max.	Min.							
1847. July.																		
1.	30.284	30.274	29.84	30.29	30.28	30.38	72	53	59	75 1/2	50 1/2	56	ne.	calm	nnw.	.....	.....	.....
2.	30.269	30.189	29.80	30.29	30.18	30.34	64	51	59	68 1/2	53 1/2	55	ne.	calm	calm	.....	.....	.....
3.	30.147	30.040	29.72	30.12	30.00	30.14	73	53	58 1/2	74	44	54 1/2	ne.	calm	sw.	.....	.....	.....
4.	30.011	29.949	29.46	29.95	29.85	29.98	83	43	65	76	49	52	se.	w.	e.	.....	.....	.....
5.	29.946	29.900	29.46	29.83	29.82	29.93	87	53	70	73	54	55	s.	s.	se.	.....	.....	.....
6.	29.896	29.820	29.40	29.80	29.76	29.86	88	59	74	71 1/2	59	56	sw.	calm	ese.	.....	.....	.....
7.	29.855	29.737	29.30	29.65	29.68	29.81	75	52	73	68	54	55	sw.	s.	ese.	.....	.....	.....
8.	29.929	29.856	29.40	29.74	29.74	29.80	76	47	65	68	56 1/2	58	s.	n.	calm	.....	.....	.....
9.	30.115	30.016	29.50	29.80	29.85	29.82	77	57	68 1/2	66 1/2	54	62	sw.	w.	calm	.....	.....	.....
10.	30.131	30.120	29.58	29.90	29.93	29.94	79	61	69	70	55 1/2	59	sw.	calm	calm	.....	.....	.....
11.	30.151	30.132	29.59	29.93	30.00	29.93	83	56	73 1/2	66	59	64 1/2	sw.	nw.	sw.	.....	.....	.....
12.	30.170	30.165	29.62	30.02	30.09	30.06	90	55	75	68 1/2	59	66	nw.	calm	calm	.....	.....	.....
13.	30.195	30.184	29.67	30.11	30.12	30.16	90	59	76 1/2	68 1/2	59	62	w.	calm	se.	.....	.....	.....
14.	30.193	30.137	29.67	30.15	30.12	30.15	93 1/2	59	70	73	54 1/2	69 1/2	w.	calm	nw.	.....	.....	.....
15.	30.140	30.112	29.64	30.10	30.03	30.21	88	58	64 1/2	78 1/2	56 1/2	58	e.	e.	calm	.....	.....	.....
16.	30.047	29.982	29.56	30.02	30.03	30.17	82	58	70	65 1/2	58	57	ne.	calm	calm	.....	.....	.....
17.	29.984	29.946	29.54	30.05	30.00	30.12	72	58	64	69 1/2	50	58	ne.	e.	w.	.....	.....	.....
18.	30.038	30.027	29.56	30.00	30.00	30.04	71	49	65	70	48	54	e.	calm	s.	.....	.....	.....
19.	29.938	29.853	29.50	29.92	29.87	29.94	70	52	67	71	56	56	ne.	calm	nw.	.....	.....	.....
20.	29.870	29.866	29.42	29.87	29.83	29.98	76	58	68	69	55 1/2	55	ne.	calm	calm	.....	.....	.....
21.	29.878	29.867	29.41	29.78	29.72	29.84	79	58	68	70 1/2	54	57	w.	e.	calm	.....	.....	.....
22.	30.167	30.020	29.46	29.68	29.91	29.68	73	45	64	64	58	60	sw.	w.	wnw.	.....	.....	.....
23.	30.228	30.193	29.70	30.09	30.11	30.02	74	44	65	63 1/2	50	56 1/2	nw.	nw.	w.	.....	.....	.....
24.	30.149	30.016	29.62	30.08	29.98	30.08	76	53	67	66	46	59	ne.	w.	calm	.....	.....	.....
25.	29.947	29.906	29.47	29.94	29.91	30.06	67	46	67	70	51 1/2	58	ne.	e.	se.	.....	.....	.....
26.	30.028	29.929	29.46	29.90	29.96	29.93	75	45	65 1/2	71	51	60	n.	nw.	w.	.....	.....	.....
27.	30.075	30.064	29.54	30.00	29.90	29.81	81	50	65	67	49	53	n.	nw.	w.	.....	.....	.....
28.	30.058	30.045	29.53	29.87	29.88	29.72	77	57	65	65	57 1/2	55	w.	calm	wnw.	.....	.....	.....
29.	30.076	30.052	29.55	29.95	29.98	29.87	88	54	68	69 1/2	56	55 1/2	w.	calm	sw.	.....	.....	.....
30.	30.035	29.997	29.50	29.89	29.82	29.74	80	48	70	67	56 1/2	61	sw.	calm	sw.	.....	.....	.....
31.	30.058	30.050	29.54	29.88	29.83	29.75	84	48	69	65	52	57 1/2	sw.	calm	sw.	.....	.....	.....
Mean.	30.068	30.014	29.54	29.954	29.941	29.978	78.82	52.87	67.3	69.3	53.8	57.90				0.79	0.86	1.032.56





THE  
LONDON, EDINBURGH AND DUBLIN  
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[THIRD SERIES.]

OCTOBER 1847.

**XL. Fourth Memoir on Induction.** By M. ELIE WARTMANN, Professor of Natural Philosophy in the Academy of Geneva\*.

[With a Plate.]

[Continued from vol. xxx. p. 272.]

§ XIV. *On the Commutators employed to render voltaic currents discontinuous, and to separate currents of induction.*

116. **I**T is exactly a century since a remedy for various ailments was first sought in the electric fluid. The first experiments were made at Geneva by Prof. Jallabert in 1747†. At a later period, when the voltaic battery was invented, its physiological effects were studied, and they are now employed for the cure of various affections, such as obstinate ulcers‡, dumbness§, deafness||, blindness¶, tic-doloureux\*\*, paralysis††, &c. Lastly, since the discovery of magnetic and electric induction in 1831 by Mr. Faraday, it has been found that the induced currents, as well as the electrical discharges of the Leyden jar, have an extremely short duration, and produce greater shocks than batteries of a large number of elements. The idea therefore has occurred of rendering the current of the electromotor apparatus discontinuous, to approximate it

\* Communicated by the Author.

† *Expériences sur l'Electricité*, p. 127. 8vo. Geneva, 1748.

‡ Becquerel, *Traité de Physique*, vol. ii. p. 638. Paris, 1844.

§ Namias, *De alcuni effetti dell' elettrico sopra l' Animale Economia*, &c., p. 27. Venice, 1841.

|| *Giornale per servire ai progressi della Patologia et della Terapeutica*. Gennajo, 1843, p. 108. *Giornale delle Scienze Mediche di Torino*, vol. iv. p. 430.

¶ *Giornale per servire*, &c., December 1841, p. 658. *Biblioteca Italiana*, fascicolo 25, p. 12, &c.

\*\* Zantedeschi, *Trattato della Elettricità*, vol. ii. p. 525.

†† *Giornale di Fisica*, &c. di Pavia, decade II. vol. vii. p. 284; and vol. viii. p. 219. *Annali delle Scienze del Regno Lombardo-Veneto*, January and February 1833, &c.

*Phil. Mag.* S. 3. Vol. 31. No. 208. Oct. 1847.

R



to the cases of induced currents. Dr. Neeff of Frankfort on the Maine, in 1835\*, and M. Masson in the following year†, have made very conclusive experiments on this subject. An instrument described by M. Poggendorff under the name of *inversor*‡, is intended to render the current of an ordinary battery at the same time discontinuous and in an alternate direction through a given conductor.

117. At the present day the employment of induced currents seems to become more and more general. In place of the original magnets employed in the apparatus of Ritchie§, Pohl||, Pixii¶, Saxton\*\*, Clarke††, Störer‡‡, and others, a simple voltaic pair has been substituted, and an instrument has been constructed, of small size, easy of transport, and producing almost unlimited effects, called an *electro-electric machine*, or a *shock-machine*. M. Bonijol constructs this machine with such perfection that it has been generally adopted, and there is at the present day scarcely an hospital where it is not found. It is employed in the treatment of a multitude of nervous affections; in that of amaurosis§§, in assisting parturition|||, and as a diagnostic to ascertain the state of vitality of the foetus.

118. I have had more than one opportunity of convincing myself that many persons make use of the shock-machine without understanding either its construction or its theory. This machine, arranged on a different plan, might be rendered both more intelligible in its mode of action and more useful to the physicist and the physiologist. I will point out some of the cases in which it may be employed, and afterwards the arrangement applicable to each of them.

119. A voltaic current being given, it may be proposed—

1. To render it discontinuous, without changing its direction, in a conductor  $\alpha$ ;

\* *Das Blitzrad, ein Apparat zu rasch abwechselnden galvanischen Schliessungen und Trennungen*. Pogg. Ann., vol. xxxvi. p. 352, and vol. xlv. p. 104.

† *Comptes Rendus de l'Acad. des Sciences de Paris*, vol. iv. p. 456.

‡ Pogg. Ann., vol. xlv. p. 372 and 385. § Phil. Trans., Oct. 1833.

|| Pogg. Ann., vol. xxxiv. p. 185 and 500.

¶ *Ann. de Ch. et de Phys.*, vol. l. p. 322.

\*\* Phil. Mag. N. S. vol. ix. p. 360.

†† Ibid. p. 262.

‡‡ Pogg. Ann., vol. lxi. p. 417, 1844.

§§ Cunier, Dr., *Annales d'Oculistique*, vol. xii. and vol. xvi., where will be found a memoir by Dr. Høring On the Employment of the Electro-magnetic rotatory apparatus in Diseases of the Eyes.

||| See on this subject, P. Kerz, *De electro-magnetismi vi et usu in arte obstetriciâ*. Bonn, 1846.—J. A. Schmidt Müller, *Handbuch der medizinischen Geburtshülfe*.—T. Radford, *Galvanism applied to the treatment of uterine Hæmorrhage*. Manchester.—Von Kilian, *Die Geburtslehre von Seiten der Wissenschaft und Kunst*.—*Neue zeitschrift für Geburtskunde*, von H. Bursh, d'Outrepont, &c., vol. xvi. No. 26, &c.

2. To render it discontinuous, and in alternately contrary directions.

This current being employed to react on a wire B, near the conductor A, it may be required—

3. To isolate the direct currents, induced from the closing of the circuit A ;

4. To isolate the inverted currents, induced on breaking this circuit ;

5. To emit these currents successively, giving them the same direction ;

6. To emit them alternately in contrary directions, just as they are produced directly.

It is known that there is a reaction of the induced currents on the principal current. We may therefore desire—

7. To collect the totality of their reaction ;

8. To avail ourselves only of the reaction of the direct induced currents ;

9. To avail ourselves only of that of the inverted induced currents ;

10. To collect only the induction of the inductor on itself.

120. Physicists have studied the majority of these cases ; but the mechanical instruments which they have imagined and described under the names of *disjunctors*\*, *tachytrope*†, *rheotrope*‡, *gyrotrope*§, or *commutator*||, are scarcely applicable except to one or other of the first two categories. The most complete of these instruments, reinvented in Paris seven years after having been described and employed in Germany, is composed of *four* isolated wheels on the same axis, the outline of which presents successively metallic and ivory arcs, against which press conducting springs. The axis is set in motion by means of a handle or tooth-wheel. Sometimes the interval of the teeth is left void, and the spring in escaping determines the opening of the circuit. Other commutators are formed with needles arranged on isolated axes, in such a manner that one is immersed in mercury at the instant when the other

\* Dove, *Magneto-elektrischer Apparat zum Hervorbringen inducirter Ströme gleicher Intensität in von einander vollkommen getrennten Drähten* Pogg. Ann., vol. xliii. p. 511. 1838.

† Dove, *Ueber den Gegenstrom zu Anfang und Ende eines primären*. Pogg. Ann., vol. lvi. p. 251.

‡ Masson et Breguet, *Mémoire sur l'induction*. Ann. de Ch. et de Phys., vol. iv. p. 134. 1842.

§ Pogg. Ann., vol. xxxii. p. 539 ; and vol. xxxiv. p. 185 and 500. 1834-35.

|| Jacobi, *Sur l'application de l'Electro-magnetisme au mouvement des machines*, § VII. Potsdam, 1835. Taylor's Scientific Memoirs, vol. i. p. 503. Archives de l'Electr., vol. iii. p. 244.

comes out of it\*. These different systems are complicated, and subject to several inconveniences. The rheotrope, which I shall proceed to describe, and which is especially applicable to electro-electric machines, combines with the advantage of

\* On the 18th of June, 1840, I communicated to the Society of Physics and Natural History of Geneva an apparatus of this kind, the construction of which presents no difficulty, and which is deposited in the Cabinet of Physics in the Academy of Lausanne. The following is a description of it:—  
“My commutator is composed of a pure copper stem *ab* (Plate II. fig. 1), intersected in the middle by a piece of ivory *c*: the latter is hollowed into the nut of a screw, in such a manner that the two halves of the stem screw into it. Between these metallic extremities some sealing-wax is run, in order to isolate them entirely. The cylinder thus formed is arranged horizontally, and each of its branches is furnished with symmetrical pieces at equal distances. These pieces are two copper teeth *ef*, placed perpendicularly and at a right angle on the axis; then a copper circle *g*. Lastly, to one of the extremities of the stem is fixed a pulley *h*, in the groove of which there runs a cord *i*, which again passes over a lower pulley *k*, which is much larger, vertical, and moveable by means of the handle *m* in one of the supports of the apparatus.

“The six projecting pieces of copper dip into a glass vessel *n* (fig. 2), placed on two small horizontal bars *o*; it presents six isolated compartments full of mercury. The extreme circles remain immersed in this liquid during the entire rotation of the stem, the arrangement of the teeth causing one to be immersed whilst the neighbouring one is not. It is easy to regulate the quantity of mercury in the troughs so that the immersion of the one may correspond exactly to the exit of the other.

“Supposing it be desired to emit into a rheometer the two induced currents, giving to them the same direction, it is sufficient to bring the extremities of the wire in which the induction is produced in the extreme compartments reserved for copper circles. The ends of the wire of the multiplier are tied to bars of copper connecting the troughs *ef*, *ef*, corresponding on the right and left of the isolator *c* to the needles fixed at a right angle. So likewise on connecting the extremities of the rheometric wire only with the troughs *ff*, or with the troughs *ee*, it is evident that the direct or inverted induced currents only may be collected.

“I have combined with this arrangement one which M. Bonijol has employed in some of his apparatus. It consists (fig. 3) of planting one of the ends of the stem in a flattened wooden cylinder *r*, on which a spring *s* presses, passing into a circular cylinder *t* of hard wood, and the free extremity of which *u* is placed by the rotation of *r* in contact with an amalgamated metallic capsule *x*, or is removed from it. Then, by connecting the spring on one side, and the capsule on the other, with the wire which the direct current of the pile traverses, we obtain by the simple rotation of the stem any number of inductions.

“This apparatus enabled me to discover that the thermo-electric currents are capable of induction like the hydro-electric currents. I employ a single bismuth-antimony pair, the solder of which is kept at 100° by steam. The bismuth extremity is connected with the spring *s*, the antimony extremity with a wire covered with silk, which makes seventy turns on a frame, and terminates at the capsule *x*. On the same frame is rolled an isolated and finer copper wire which makes 1200 coils (110.), and both ends of which terminate in the troughs *p q*. The induced circuit is closed by a very delicate rheometer (*5 a*), which deviates *five* degrees and more,

being more simple, and consequently less subject to derangement, that of not requiring the employment of mercury, and of serving to solve all the cases above stated.

121. H (Plate II. fig. 4) is a reel on which two *insulated* wires are wound; one the inductor A, by which the current of the battery  $p\ n$  is made to pass; the other the induced B, intended to become the seat of the currents of induction. Three brass wheels  $r, s, t$ , of the same diameter, are isolated from one another on a common axis; their circumference presents an equal number of parts alternately of metal and wood. Two metallic springs  $a, b$  are fixed against the wheel  $r$ , in such a manner that the first leans against a conducting arc, and the other against an insulating arc. The wheels  $s$  and  $t$  are each pressed by two springs  $c\ d, e\ f$ , similarly arranged. The central metallic parts of the three wheels are in constant communication with the springs  $g, h, i$ .

122. If it be desired to collect the voltaic current always in the same direction after having rendered it discontinuous, it is sufficient to connect the spring  $i$  with the pole  $p$  by a wire  $\alpha$ , and the other pole  $n$  with the spring  $f$ , by means of any conducting wire different from the wires A and B wound upon the reel. If it is wanted to obtain, as with the *inversor*, the discontinuous current in directions alternately contrary, we must join the springs  $c$  and  $e$  as well as the springs  $d$  and  $f$ , and then connect the extremity of the conjunctive wire of the battery with the spring  $h$ .

123. When it is desired to employ currents of induction, the contact of the extremity  $l$  of the inducting wire with the pole  $n$  is established permanently, and that of the extremity  $m$  with the spring  $f$ . Now, to isolate the direct currents induced at the closing of the circuit A, we have only to connect the ends  $x$  and  $y$  of the wire B respectively with the springs  $b$  and  $g$ .—To isolate the inverted currents, we unite  $x$  with  $h$  and  $y$  with  $d$ .—To cause the direct and the inverted currents to pass one after another in the same direction through the rheometer G, for example, we connect the springs  $a$  and  $c$  with the end  $s$  of the wire of the instrument, the springs  $b$  and  $d$  with the end  $t$ , the extremity  $x$  with the spring  $h$ , and the extremity  $y$  with the spring  $g$ .—To collect the induced currents alternately in contrary directions, just as they are produced directly,

when it is traversed by direct and inverted currents in the same direction." (See the Transactions of the Helvetic Society of Natural Sciences for 1840, pp. 173, 195.)

Prof. Dove has demonstrated thermo-electric induction by a different process. His researches were made at the same time as mine, and in an independent manner. (See Pogg. *Ann.*, vol. xlix. p. 97. 1840.)



we disconnect the extremities  $x$  and  $y$  of the wire B from the springs of the rheotrope.

124. Lastly, if we propose to employ the reaction of the induced wire B on the inductor A, and that of the inductor wound in a helix on itself, we substitute for the wire  $\alpha$  the body which is to be subjected to the effects of these reactions. We then employ one of the four arrangements above described (123.), according as we wish to obtain the totality of influence of the two currents induced in the same direction, or in directions alternately opposed, or again, the separate influence of the direct or the inverted currents. The simple induction of the inductor on itself is obtained with a reel with a single wire in place of the conductor  $\alpha$ , and the arrangement described (122.).

125. It remains for me to give some details on the construction of the rheotrope. The three metallic wheels  $r, s, t$  (fig. 5) present on their periphery twelve hollows filled in with hard wood. These heterogeneous wheels have been worked together by the lathe; they are each 0<sup>m</sup>·80 in diameter, and 0<sup>m</sup>·06 in thickness. A metallic tooth of the middle wheel  $s$  exactly corresponds to one isolating part of the extremes  $r$  and  $t$ . They are placed on the same brass axis  $kl$ , which is turned by a winch  $n$  or a tooth-wheel. The spring  $i$  and the wheel  $t$  are in metallic contact with the axis. The wheels  $r$  and  $s$  are, on the contrary, each isolated from it by an ivory ring covered externally with a brass cylinder. These two cylinders bear the wheels, and are constantly pressed by springs  $g, h$ , which embrace them on a semi-circumference. The three springs  $g, h, i$  terminate on the three heads  $g', h', i'$ , by means of which they can communicate together. Lastly, the six springs  $a, b, c, d, e, f$  are made of plates of hammered copper; they are fixed to the base of the instrument by screws,  $r', s', t'$ , the heads of which, similar to  $g'$ , and pierced like them with two holes, can receive the metallic wires intended to establish a connexion between the different wheels. These springs are cleft in order that the groove may facilitate the adjustment of their length. Above they bear a screw (fig. 6) in the part which has to rest on the circumference of the wheels; the opposite notch allows of regulating the elasticity of the spring and the degree of friction. The play of these pieces may thus be regulated with minute precision.

126. If it is not wanted to impart the same direction to the two induced currents, the apparatus may be simplified by giving it only two wheels. One is reserved to render the current of the battery intermittent; the other is joined to the induced wire; and according as there is coincidence or alter-

nation in the closing of the two circuits, only either the direct or the inverted currents are received. This double effect may be obtained by changing the point of contact with one of the springs, or by varying the position of one of the wheels on the axis relatively to the other. Two wheels do not permit of giving the same direction to the direct and the inverted currents; because as it is evident that the induced circuit must communicate with the two wheels when the principal current is closed, a part of this current may proceed from the wire of induction and modify the effect of the direct induced current.

127. Lastly, if it be desired to isolate only the inverted induced currents, the rheotrope may be reduced to a single wheel. It is sufficient for the proposed object to open the induced circuit when the inducing circuit is closed, and *vice versa*. But this arrangement would not be suited to isolate the direct induced currents, because it would be necessary to close simultaneously the two circuits, and the voltaic current would be propagated in the double channel presented to it.

128. It will be found convenient to mark letters on the different pieces  $g'$ ,  $h'$ ,  $i'$ ,  $r'$ ,  $s'$ ,  $t'$ , and to repeat them at the extremities of the metallic conductors employed to connect these pieces. These conductors will be fixed to the interior of the lid of the case which contains the whole electro-electric machine; and a brief direction will indicate which ought to be employed to produce the effects corresponding to the different possible cases.

129. It is understood that the commutator with three or with two wheels is applicable to all magneto-electric machines, telegraphs, clocks, &c., whose motive principle is the electricity of the magnet or of the battery.

#### § XV. *Employment of induced currents to restore sensation.*

130. The cases of nervous weakness which have yielded to a judicious application of electro-physiological shocks and discharges are too well ascertained to admit of any question. Since the marvellous effects of æther have been known, I have proposed to several physicians the employment of the electro-electric machine, or at least of intermittent currents of very short duration, to obviate the dangers which the injection of too strong a dose of this liquid, or a too prolonged inhalement of it, might produce. I have made some experiments\* with a view to verify the accuracy of my expectations; and although they are so few as to require to be repeated and varied, I shall

\* In company with Dr. A. P. Prevost, and Mr. Schnetzler. I take this opportunity of thanking these gentlemen for their zealous cooperation.

give them here, because similar results have recently been announced by M. Ducros\*.

131. The animals subjected to experiment were a rabbit three months old, a chicken nine months old, and some frogs of both sexes. They are all very sensitive to electric shocks. The action of æther upon them is also very powerful, especially on the frogs, which should not be moistened with this liquid.

132. The rabbit and the chicken appeared to have recovered their sensation sooner under the influence of the shocks of induction than by simple exposure to the air. In the frogs no difference in this respect was remarked.

133. The ætherization was effected by plunging the animal into a glass cylindrical vessel, in which boxes were arranged furnished with sponges moistened with æther; it was covered with a piece of linen dipt in water. The internal atmosphere was removed from time to time by removing the covering.

134. The most remarkable case was presented by the chicken. A quantity of æther, more than sufficient to produce insensibility, was injected into its rectum. When it arrived at this state, two or three shocks of the electro-electric apparatus (110.) were passed from one wing to the opposite leg, which shocks were effected by a Grove's pair; immediately the eyes opened. On continuing the discharges in a very intermittent manner, the animal was seen to struggle, to rise on its feet, and then to fly to the end of the laboratory, relapsing gradually into an insensible sleep under the influence of the portion of injected æther which had not as yet produced its effect.

135. The rabbit and the chicken were subjected to several successive ætherizations. The former, young and weak, died six or seven hours after the fourth trial (injection). At the end of fifteen hours its body was stiff, as if death had resulted from natural causes. Its nerves exhibited the softening mentioned by some anatomists. The chicken, on the contrary, survived, and even on the following day laid an egg with a soft shell. It subsequently produced several others perfectly healthy. It did not appear to feel the effects of the shocks or injections to which it had been subjected. It ate corn greedily, and the rabbit lettuce leaves, as soon as the stupefaction produced by the æther had terminated.

136. Experiments were made on the frogs and the chicken; one while with the effect of the induced currents successively

\* *Comptes Rendus de l'Académie des Sciences de Paris*, sitting of the 22nd of February 1847, p. 286.

direct and inverted, at another with inverted currents only, employing the arrangement above described (127.). There was no perceptible difference between the two methods of electrifying, even on circulating the inverted currents from the feet to the wings, or *vice versâ*.

§ XVI. *Action of Induced Currents on Albumen.*

137. Brande was the first who pointed out the coagulation of albumen on the positive pole of the battery. M. Matteucci, in treating of the physiological action of electric currents\*, says, that if the pole which was first positive be rendered negative, the albumen is not seen to redissolve, and that consequently an electric current may very well produce a cataract, but not destroy it. On the other hand, Prof. Zantedeschi affirms that he has seen the liquefaction of the albumen at the negative pole†. Repeated experiments have never shown me this return to the fluid state, and lead me to adopt entirely the conclusion of the celebrated physiologist of Pisa.

138. The coagulation of albumen does not present any remarkable phase, when, under the immediate influence of a battery, we substitute either direct or inverted induced currents, or the voltaic current rendered intermittent and strengthened by the reaction of the induction which it has engendered in its own conductor and in the neighbouring conductor (124.). But the phænomenon changes when the liquid is traversed by induced currents in alternate directions.

139. Through the inducing wire A of an electro-electric machine furnished with a bundle of iron wires, I passed the current of five Grove's pairs of 0<sup>m</sup>.1 square surface. The extremities *x* and *y* of the induced wire B (fig. 7) terminated in cups *gg* full of mercury. The circuit was closed by two platina wires *a*, *b* of 1<sup>mm</sup> in diameter, one part immersed in the cups, the other in the glass *o* full of the white of egg. The latter immediately coagulated around each wire, especially round that which communicated with the extremity of the circuit B, from whence proceeded the inverted induced current, and which corresponded to the positive termination of the rheophorus A. At the end of a few minutes some bubbles of gas appeared on the circumference of the coagulum. Some, having increased in volume, rose lightly to the surface of the viscous medium in which they were formed. The albumen, riddled with holes, by which the gas escaped and continued

\* *Lezioni sopra i fenomeni fisico-chimici dei corpi viventi*, p. 173. Pisa, 1844.

† *Trattato del Magnetismo et della Elettività*, vol. ii. p. 511. Venice, 1845.



to be disengaged, turned black in several places: then a series of *luminous sparkles*, and lastly real sparks of a bright yellow glittered on the whole immersed part of the platina wire. At the same time the induced wire B was heated around the reel, the metallic pieces of the rheotrope rose in temperature, and the upper sides of the glass, not filled with the albumen, were coated with aqueous vapour.

140. This remarkable phænomenon is doubtless complicated. The coruscations do not dart from one wire to the other in the liquid; they are seen along the wire. I thought at first that the combustion (for it was such) only took place on one of the electrodes (139.); but on repeating the experiment many times, I saw it alternate on both of them according as I reversed the poles of the battery, or present itself first upon one wire, then upon the other, without the direction of the current being changed; or lastly appear upon only one of them, whatever changes were made in the positions of the rheophori and the extremities of the induced circuit. I attribute this latter case, which only occurred when the surface of the albumen was covered with a layer of æther, to the difference of the conditions of contact of the two platina electrodes with the liquid: one, in fact, was then only covered with a slight coagulum, whilst the other gave rise to a considerable quantity of gas. These gases were collected on the æther in a tube traversed by a platina wire cemented at its top. They presented neither free carbonic acid, nor oxygen, nor hydrogen. I think that they were a mixture of oxide of carbon and carburetted hydrogens.

141. The albumen solidified around platina conductors acquires the consistence of very soft glue; it is ductile, brownish, even blackish, and diffuses a marked odour of burnt horn or phosphoretted hydrogen. The platina does not take the pulverulent appearance nor black colour which are communicated to it by discontinuous alternate currents in other media; it preserves its metallic appearance. With the assistance of Prof. Marignac I analysed the coagulum; it contained no trace of platina. There is therefore here no catalytic action.

142. These various remarks lead me to think that, in circumstances of imperfect conductibility of the albumen, and of great power in the induced currents employed, the immersed wires become heated when the coating of coagulum and of gaseous bubbles has put a new obstacle to the passage of the alternate currents (an obstacle rendered evident by the elevation of temperature of the external circuits), whence results a true igneous decomposition and a burning, under the influence of oxygen in a nascent state, of combustible elements exposed.

143. Whatever value this opinion may have, it seems to me that the decomposition of albumen by the passage of very intense induced currents is a fact which deserves the serious attention of physicians and physiologists. The presence of this body in the blood, in urine, in the eye, in amniotic liquors, &c., requires caution in the employment of violent alternate currents.

144. The appearances which I have described equally take place in the albumen extracted from new-laid eggs, immersed for some hours in the vapour of æther. They *appear* even to be developed there more easily.

145. It is perhaps well to add, that the production of these bright coruscations indifferently on the two electrodes negatives any explanation founded on a different polarity of the platina wires, and all analogy with the phænomena investigated by MM. Gassiot\*, Hare†, and Neeff‡.

Geneva, June 18, 1846.

XLI. *On eliminating the Signs in Star-Reductions.*

By S. M. DRACH, F.R.A.S.

*To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,

THE subject of this paper was broached by the Astronomer Royal in the Monthly Notices of the Royal Astronomical Society for January 1847. I beg to propose the following extension, eliminating even the indices of the logs. employed.

Let  $A = E - P$ ,  $B = F - Q$ ,  $C = G - R$ ,  $D = H - S$ ;  
 $a = e - p$ , &c.  $a' = e' - p'$  for decl., or  $= p' - e'$  for N. P. Dist.,  
 $P$ ,  $p$ ,  $p'$ , &c. are numerical constants afterwards determined.

$$\text{Corr. R.A.} = \Sigma Aa = \Sigma Ee - \Sigma eP - \Sigma Ep + \Sigma Pp:$$

$$\text{Corr. N.P.D.} = \Sigma Aa' = \Sigma Ee' - \Sigma e'P - \Sigma Ep' + \Sigma Pp'.$$

Let  $P = 28.75$ ,  $Q = 30.5$ ,  $R = 1.35$ ,  $S = 20$  (R.A. given in time).

I. Right ascension,  $p = 2^s$ ,  $q = 2$ ,  $r = 30.5$ ,  $s = 2$ .

\* *Archives de l'Electricité*, vol. iii. p. 240.

† Silliman's American Journal, January 1841. I succeeded several years ago in melting in an intermittent manner an iron wire of 2<sup>mm</sup> diameter, employed as a negative electrode on the surface of impure mercury in which a copper wire bound to the positive pole is immersed. Twenty Daniell's couples, or forty smaller Bunsen's, suffice for this experiment.

‡ *Archives des Sciences Physiques et Naturelles*, vol. i. p. 30.

$$\left. \begin{aligned} \Sigma Pp - \Sigma eP &= -78^s.994 + 41^s.912 \\ &\quad \left\{ 1 + \frac{1}{15} \sec \delta \sin \alpha + 14^h 53^m 14^s \right\} \\ &\quad + 32^s.937 \left\{ 1 + \frac{1}{15} \tan \delta \sin \alpha + 14^h 18^m 49^s \right\} \end{aligned} \right\} . \quad (J.)$$

$$\left. \begin{aligned} -\Sigma Ep &= -308^s.392 + 55^s.422 \{ 1 + \sin \odot + 42^\circ 32' 4'' \} \\ &\quad + 1.324 \{ 1 + \sin 2\odot + 55^\circ 29' \} \\ &\quad + 30.5(1-t) + 21.254 \{ 1 + \sin \oslash + 60^\circ 30' 44'' \} \\ &\quad + 0.217 \{ 1 + \sin 2\oslash + 235^\circ 52' \} . \end{aligned} \right\} \quad (K.)$$

Sum =  $\Sigma Aa - \Sigma Ee = -387^s.386$  + periodical terms.

II. North Polar Distance,  $p' = 2$ ,  $q' = 2$ ,  $r' = 30.5$   $s = 2$ .

$$\left. \begin{aligned} \Sigma Pp' - \Sigma e'P &= -74''.849 + 12''.476 \cos \delta + 41''.912 \\ &\quad \{ 1 + \sin \delta \sin \alpha + 8^h 53^m 14^s \} \\ &\quad + 32''.937 \{ 1 + \sin \alpha + 8^h 18^m 49^s \} \end{aligned} \right\} . \quad (J'.)$$

$$-\Sigma Ep' = -\frac{\Sigma Ep}{15} = -308''.392 + \&c. \quad (K'.)$$

Sum =  $\Sigma Aa - \Sigma Ee = -383''.241$  + periodical terms.

Now if we add to (J.) and (J'.) the constant 180, and to (K.) the constant 420 seconds, there will be only *positive* quantities, and we shall have merely to subtract  $10^m$  or  $10'$  from the mean place; the corrections being

$$\begin{aligned} E &= 28.75 - 18.732 \cos \odot & e &= 2 + \frac{1}{15} \cos \alpha \sec \delta \\ & & e' &= 2 - .434 \cos \delta + \sin \alpha \sin \delta \\ F &= 30.5 - 20.420 \sin \odot & f &= 2 + \frac{1}{15} \sin \alpha \sec \delta \\ & & f' &= 2 - \cos \alpha \sin \delta \end{aligned}$$

$$\begin{aligned} 10G &= 13.5 + 10t - 3.43 \sin \oslash \&c. & 0.1g &= 3.35706 + 0.1337 \sin \alpha \tan \delta \\ & & 0.1g' &= 3.05 - 2.0055 \cos \alpha \end{aligned}$$

$$\begin{aligned} H &= 20 - 9.250 \cos \oslash \&c. & h &= 2 + \frac{1}{15} \cos \alpha \tan \delta \\ & & h' &= 2 + \sin \alpha \end{aligned}$$

It follows that the index of the first set in logarithms is constantly *unity*, and that of the third set constantly *zero*, permitting the omission of the latter. From  $86^\circ 10'$  S. dec. to  $88^\circ 50'$  N. dec.  $e, f, g, h$  will have their values range between 1 and 10, and their indices therefore always *zero*; these may also be omitted. Now of the 8377 British Association Ca-

talogue stars, only sixteen fall out of this category in 1850; viz.

Urs. Min. 2320, 4070, 4150, 4165, 6281, 6320, 6999, 7184, Octans, 71, 2878, 5936, 5959, 6793, 7020, 7713, 8072; a satisfactory result, as these polar segments  $= \frac{1}{447} \text{th} = \frac{19}{8377} \text{ths}$  of the spherical surface.

With my constants, seven of those sixteen (2320, 4070, 4150, 5936, 6281, 6320, 7713) have all their R.A. coefficients positive, the others have some negative. Indeed, near the pole the annual change is so great, as to render *greater constants* to include a dozen of these sixteen needless.

The saving in the above 8362 stars permits an additional column, although five fig. logs. are required, giving a result as far as  $0^s.01$  or  $0''.01$ .

App. R.A. = mean R.A. at epoch + yearly precession + proper motion + ephemeral quantity.

+  $Ee + Ff + Gg + Hh$  + stellar quantity  $- 10^m$ .

App. N.P.D. = similar quantity  $- 10'$ ,

which  $10'$  might be already included in the mean place, as in the planetary tables. The ephemeral quantity (depending on the day of the year) is the same in R.A. *time-seconds*, or N.P.D. *space-seconds*.

Possibly these hints may be useful before reprinting the British Association Catalogue.

S. M. DRACH.

London, Sept. 2, 1847.

## XLII. On the Molybdate of Lead. By Mr. JOHN BROWN\*.

**M**OLYBDATE of lead was first analysed by Klaproth †, who proceeded in the following manner:—

100 grains of the mineral finely pounded were treated with dilute hydrochloric acid, and the whole of the silica was thus separated. Upon cooling, the greater part of the chloride of lead was deposited in fine crystals. The clear supernatant liquor was then drawn off, and when sufficiently concentrated the remaining chloride of lead was deposited. The whole of the chloride was then carefully collected together, dried and weighed. Its weight was 74.5 grs. From this the quantity of oxide of lead was ascertained, which was 64.42 grs. Every 100 grains of molybdate of lead contain therefore 64.42 grs. of oxide of lead. When the solution had thus been freed from lead, it was concentrated by evaporation. Nitric acid was

\* Read before the Philosophical Society of Glasgow, April 28, 1847, and communicated by Dr. R. D. Thomson.

† *Beiträge zur chemischen Kenntniss der Mineralkörper*, vol. i. 265.



then added to the solution, which immediately became of a fine blue colour. When sufficiently concentrated, a quantity of molybdic acid separated. The solution was then evaporated to dryness, and the molybdic acid remained in the form of a fine citron-yellow powder, which when completely dried weighed 34.25 grs.

The constituents therefore of 100 parts of the purest crystals of Carinthian molybdate of lead are—

Oxide of lead . . .	64.42	59.59	} corrected from the chloride.
Molybdic acid . . .	34.25	34.25	

As Klaproth did not know the true composition of chloride of lead, the quantity of PbO given above is wrong. Calculating the quantity of oxide from the quantity of chloride which he obtained, we get 59.59 per cent. of oxide of lead, which is very near the theoretical quantity, or 60.57. But the great error is in the molybdic acid. What Klaproth considered as silica, was very probably molybdic acid, as that acid is not entirely soluble in hydrochloric acid; and as he apparently deducted this as impurity, he gets too little molybdic acid. He also does not mention how he washed out the molybdic acid from the chloride of lead. It could not well have been done with water, for chloride of lead is soluble to a great extent. This is a great point of imperfection in the analysis.

II. This mineral was next subjected to a close examination by Charles Hatchett, Esq., whose analysis is recorded in the *Philosophical Transactions* (vol. xviii.), of which the following is a summary:—

250 grs. of the ore, freed from as much impurity as possible, were put into a glass flask, and digested with sulphuric acid for some time under a strong heat. When the solution cooled, the clear liquor was drawn off, and the residual sulphate of lead washed by subsidence. This process was repeated several times. The acid solutions were then filtered, and the filtered liquid neutralized by caustic ammonia. After standing for twenty-four hours a pale yellowish-coloured precipitate fell down, which was collected on a filter, washed and dried: its weight was then 4.20 grs. It had a yellowish colour, and when dissolved in hydrochloric acid gave a blue precipitate with yellow prussiate of potash.

Part of the clear blue solution, which was composed of sulphate and molybdate of ammonia, was then put into a retort and evaporated down, the rest of the solution being added as the liquid in the retort evaporated; the whole was then dried and strongly heated. In this manner all the sulphate of ammonia was driven off, whilst the molybdate of ammonia was decomposed into molybdic acid and ammonia, the former

of which remained in the retort: the molybdic acid then weighed 95 grs. The sulphate of lead formerly obtained was then treated in the following manner:—It was boiled with four ounces of carbonate of soda in solution; the powder was then washed, and nitric acid much diluted was poured on it. The whole dissolved except a small quantity of silica, which was thrown on a filter: this when washed and dried weighed .7 gr. The acid was then exactly neutralized with caustic potash, which precipitated the lead as oxide: this, when washed and dried, weighed 146.00 grs.

The oxide of lead was then dissolved in nitric acid, and sulphuric acid was added. After standing for some time the solution was filtered and the filtered liquor saturated with  $\text{NH}_3$ : after standing for some time a small quantity of peroxide of iron was precipitated, which when filtered and dried weighed 1.0 gr. This, when added to the former quantity of peroxide of iron, makes the quantity 5.2 grs., and the quantity of oxide of lead 145 grs.

The composition of 250 grs. of molybdate of lead is therefore—

		per cent.
Oxide of lead . . .	145.0	58.00
Molybdic acid . . .	95.0	38.00
Peroxide of iron . .	5.2	2.08
Silica . . . . .	.7	.28
	<hr/> 245.9	<hr/> 98.36

If the iron and silica be subtracted as impurities, this analysis is very correct; but the method is very tedious and inconvenient, and requires very great care.

III. The next person who turned his attention to this mineral was Göbel\*.

100 grs. of the mineral were digested with dilute hydrochloric acid with the assistance of heat: upon cooling, the lead was deposited in the form of chloride. These crystals were then collected together and dried; the weight was then found to be 72.5 grs., which is equivalent to 59 grs. of oxide of lead. The solution freed from lead was evaporated to dryness: when perfectly dry a small quantity of nitric acid was added, and the solution was again dried. It was then heated to redness in a close vessel and weighed: its weight was thus found to be 40.5 grs.

100 grains contain therefore—

\* Schweigger's *Journal für Chemie und Physik*, vol. xxxvii, 71.

Oxide of lead . . .	59.0	58.0	} corrected from the chloride.
Molybdic acid . . .	40.5	40.5	
	<u>99.5</u>	<u>98.5</u>	

This method is essentially the same as that used by Klaproth. The result however is much nearer the truth. Göbel however gets too much molybdic acid and too little oxide of lead. This was probably owing to some of the chloride of lead not being obtained, as it is soluble to a great extent in water (1 in 152 of water)\*; and the analyst does not state how he washed the chloride of lead free from molybdic acid.

IV. The methods hitherto employed being liable to very great objections, the molybdate of lead was analysed by another method, which had proved successful in the hands of Mr. William Parry last year in the Glasgow College laboratory.

26.84 grs. of the mineral finely pounded were boiled for a considerable time with nitric acid and filtered. The undecomposed mineral, along with a quantity of molybdic acid, remained on the filter. This was then completely washed: ammonia was then poured into the filter. The molybdic acid was thus dissolved, and the insoluble matter remained on the filter; this was then washed, dried, ignited and weighed. The weight of the insoluble matter in 26.84 grs. was 1.15 gr.

The solution containing the molybdate of ammonia was then evaporated to dryness, and heated to redness in a close vessel. The greater part of the molybdic acid was thus obtained. Its weight was 6.76 grs.

The first washings from the molybdic acid and the insoluble matter were then concentrated. Caustic ammonia was added in order to neutralize the excess of acid, and afterwards sulphohydret of ammonia was added in excess. In this manner the lead was precipitated in the form of sulphuret, while the tersulphuret of molybdenum was redissolved in excess, giving the solution a deep red colour. The sulphuret of lead was then thrown on a filter, and washed with water containing sulphohydret of ammonia. When completely washed, the sulphuret of lead was dissolved in muriatic acid, and after boiling for some time was filtered to get rid of the sulphur. The filtered liquor was then concentrated, and the lead precipitated by means of oxalate of ammonia. The precipitated oxalate of lead was then thrown on a filter, washed and dried. By ignition the oxalate of lead was converted into the oxide;

\* I found in two experiments that 3963 grs. of water at 60° dissolved 26.2 grs. Pb Cl = 1 in 151, and 4260 grs. HO dissolved 27.6 grs. Pb Cl = 1 in 154 HO.

the quantity of which in 26·84 grs. was thus found to be 16·20 grs., which is equivalent to 60·35 per cent. of oxide of lead.

The next thing to be obtained was the rest of the molybdic acid. This was contained in the washings from the sulphuret of lead in the form of tersulphuret of molybdenum. When the solution was sufficiently concentrated, it was made slightly acid by means of nitric acid: a brownish-coloured precipitate fell down, which is tersulphuret of molybdenum. This was then thrown on a filter and washed. It was then dried at  $212^{\circ}$  and weighed: its weight was 3·37 grs. From this and the previous quantity of molybdic acid the quantity per cent. was calculated, which was 39·30 grs.

According to this analysis, the composition of molybdate of lead is—

Molybdic acid . . .	39·30
Protoxide of lead . .	60·35
	<hr/> 99·65

V. In the course of the preceding analysis it was observed that the sulphohydret of ammonia exercised a powerful solvent action on the mineral itself. The following new method of successfully analysing this mineral was therefore adopted.

23·0 grs., after being reduced to a very fine powder, were digested with the aid of heat in sulphohydret of ammonia. The solution became immediately of a deep red colour, owing to the tersulphuret of molybdenum which was held in solution by the sulphohydret of ammonia, while the lead was precipitated as sulphuret, and fell to the bottom in the form of a black powder. The clear supernatant liquor was then drawn off, and a fresh portion of sulphohydret of ammonia was added. This, after standing for some time, was thrown on a filter, and washed with water containing sulphohydret of ammonia. The tersulphuret of molybdenum passed through in solution, while the sulphuret of lead remained on the filter. When this was completely washed it was dissolved in dilute muriatic acid, which took up the sulphuret of lead and left the undecomposed matter along with the sulphur. These were then thrown on a filter and washed: the whole was then burnt. The sulphur was thus driven off, while the insoluble matter remained. The insoluble matter in 23 grs. amounted to ·24 gr., whilst in the former analysis it amounted to 1·15 in 23 grs.

When the washings from the sulphur were sufficiently concentrated, the lead was precipitated by means of ammonia and oxalate of ammonia. The oxalate of lead was then thrown on a filter and weighed. The quantity of oxide of lead in



22·76 grs. amounted to 13·71 grs., which is equivalent to 60·35 grs. per cent.

The next point was to precipitate the tersulphuret of molybdenum. This was done by making the solution in sulphohydret of ammonia slightly acid by means of muriatic acid. The tersulphuret went down in the form of a brownish-coloured precipitate. This was then thrown on a filter, dried, ignited and weighed. The quantity in 22·76 grs. was thus found to be 9·91 grs., which is equivalent to 39·13 per cent. of molybdic acid.

The constituents therefore of molybdate of lead according to this analysis are—

Molybdic acid	. .	39·19
Lead protoxide	. .	60·23
		<u>99·42</u>

Phosphates and arseniates of lead were decomposed in the same manner; and it is evident this process would also answer with antimonates, vanadates and selenites.

	Klaproth.	Hatchett.	Göbel.	Parry.		J. Brown.			Theory.
Molybdic acid ...	34·25	38·00	40·50	*40·40	39·88	39·30	*40·64	39·19	39·13
Protoxide of lead	59·59	58·00	58·00	59·60	59·56	60·35	59·86	60·23	60·87
Peroxide of iron	.....	2·08							
Silica .....	.....	·28							
	93·84	98·36	98·50	100·00	99·44	99·65	100·00	99·42	100·00

#### Note by Dr. R. D. THOMSON.

*Test for Arseniates, &c.*—I may notice a simple and quick method of testing minerals containing arsenic in its various forms, phosphates, molybdates, vanadates, &c. A few grains of the mineral to be examined are to be finely pulverized in an agate mortar and introduced into a test-tube, and boiled with bisulphohydret of ammonia for a few minutes. The mineral is partially decomposed; the sulphuret of lead precipitates, while sulphuret of arsenic, &c. is dissolved by the excess of the reagent. The tube is then allowed to stand at rest, and the supernatant liquor poured off or filtered. The excess of bisulphohydret of ammonia being removed by evaporation, the yellow sulphuret of arsenic precipitates. A molybdate is

\* In these analyses the lead only was ascertained, and the deficiency was taken as molybdic acid.

The two last analyses were made by means of sulphohydret of ammonia, the three preceding by nitric acid.

detected at once by the fine orange-red colour which the reagent assumes when it is heated in contact with that mineral.

A vanadate gives a dark colour, but possessing less of the red shade than the molybdate. The liquor filtered from the sulphuret of lead containing the vanadium in solution has a green colour, becoming blue by the addition of hydrochloric acid. Hence it appears that arsenic dissolved in bisulphohydret of ammonia does not alter the colour of that reagent, while the liquor gives a precipitate of orpiment by concentration. Molybdenum and vanadium, on the other hand, render that reagent reddish, and give brown precipitates by concentration. The liquor filtered from the sulphuret of molybdenum is colourless, or its hue is similar to that of the reagent, while the liquor derived from the vanadium precipitate is *green*.

I have succeeded in decomposing a sufficient amount of these minerals for quantitative analysis by the preceding process when they have been carefully pounded and lævigated. The process is particularly advantageous in the analysis of molybdate of lead, where the use of nitric acid for dissolving the mineral is objectionable in consequence of its tendency to form the molybdate of molybdenum, and where hydrochloric acid, by producing a chloride of lead, renders the employment of an inconvenient quantity of water necessary. I have found this process for testing very convenient where it was desirable to use minute quantities of crystals, and where rapidity is an object in view, as in examining a large collection of minerals of the preceding description; and I mention it for the sake of those who may possess in their cabinets minerals of this nature which they may desire to test, since it may be found a useful adjunct to the blowpipe test.

The bisulphohydret affords a simple distinguishing test between metallic arsenic and antimony, when spots have been received on porcelain by Marsh's process. Arsenic dissolves in the reagent, and leaves a yellow stain by evaporation. Antimony dissolves and leaves an orange stain. For this experiment it is convenient to use the inside of the cover of a porcelain crucible.

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XLIII. *On Fossil Calamites found standing in an erect position in the Carboniferous Strata near Wigan, Lancashire.* By E. W. BINNEY\*.

**T**HE fragmentary condition of the great bulk of fossil plants found imbedded in the coal measures has led many geologists to suppose that they had been drifted from adjoin-

\* Read before the Literary and Philosophical Society of Manchester, July 6, 1847, and communicated by the Author.

ing lands, and had not grown in the position in which they are now found. But although it is certain that plants which have been drifted by water generally present a broken appearance, it is equally true that plants grown upon the spots where they are now found, having been laid low by the action of currents of water, or weighed down and buried by the weight of mud or silt that had fallen upon them, afford similar appearances, so that great care must be taken before we conclude that a plant has not grown on the place where it is now found merely because we find it in fragments.

A few years ago the whole of the fossil flora was generally supposed to have been drifted. The first plant that was excepted from this rule and recovered its proper place was the *Stigmaria*, whose long stringy rootlets prevented it from being so conveniently drifted by currents as the advocates of the *drift* hypothesis could desire; therefore it was allowed to have grown where it is found.

When numbers of *Sigillariæ* were found standing erect on seams of coal, and their roots had not been traced to their extremities, it was at first attempted to refer them to accident, like the snags now found in the Mississippi and other rivers. However, a more careful observation of these fossils, and the great number in which they were found, at length induced geologists to admit that they must have grown where they are now met with. The discovery of the trees at Dixon Fold on the Manchester and Bolton Railway by Mr. John Hawkshaw, F.G.S., and so ably described by the late Mr. Bowman, F.G.S., in the first volume of the Transactions of the Manchester Geological Society, mainly contributed to establish this view, which has been since clearly proved by the certainty that *Stigmariæ* are the roots of *Sigillariæ*, as the fossil trees of St. Helens and Dukinfield testify.

As yet, however, *Sigillaria* was the only tree that to any extent could be said to have been discovered *in situ*.

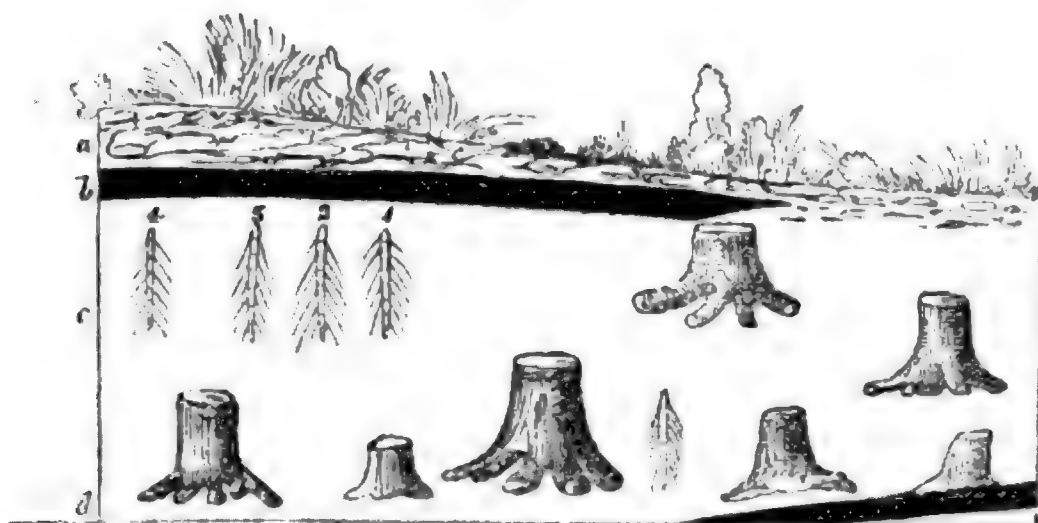
In the present communication, it is intended to show that *Calamites* have been found standing erect on the places in which they grew by the side of *Sigillariæ*, and that the rootlets of the former very much resemble, if they are not identical with, those of the latter plant.

The rootlets of *Calamites* have been very correctly figured and described by Messrs. Lindley and Hutton in vol. i. pp. 78 and 79 of their *Fossil Flora of Great Britain*; but it is believed by the writer that although numbers of erect *Calamites* have been observed in the coal-measures, still none of them to his knowledge have been described with their roots standing in the position in which they had grown.

During an examination of the deep excavations through the coal-measures made in forming the Bury and Liverpool Railway in the vicinity of Wigan, I was so fortunate as to discover on the 21st day of April last, in the Pemberton Hill cutting about two miles west of Wigan, not only a whole forest of *Sigillariæ* standing erect with their roots just as they had grown, but also many *Calamites* in a similar state of perfection.

The accompanying woodcut, fig. 1, representing a view of the south side of the railway cutting, will show the position in which the fossils occurred, although it is on an exaggerated scale, and the characters of the trees are not given.

Fig. 1.



The excavation in which the fossils were met with is about twenty-five feet deep, and consists chiefly of a light gray-coloured silty clay known by the provincial name of "Warren," containing nodules of ironstone. This deposit is very similar in composition to the strata in which the fossil trees at St. Helens and Dukinfield before described were found. It lies between two beds of coal each about two feet in thickness, and occupies a position in the higher part of the middle division of the Lancashire coal-field. The upper seam of coal is covered, and in some places partly removed, by a deposit of one or two yards in thickness of till. Near the bridge is seen a flexure in the strata, as shown in the woodcut.

In Mr. Haliburton's section at Haigh (vol. vi. New Series, of Manchester Memoirs, p. 437, Remarks on the Coal District of South Lancashire, by James Heywood, Esq., F.R.S.), occur the following strata:—



	yards.	ft.	in.
Depth from the surface . . . . .	10	0	0
Coal which burns to a white ash . . . . .	1	0	6
Interval . . . . .	8	0	0
Coal (Wigan yard coal) . . . . .	0	2	6
Interval . . . . .	16	0	0
Coal . . . . .	0	2	6
Interval . . . . .	24	0	0
Coal (Wigan four-feet coal) . . . . .	1	1	0
Interval . . . . .	32	0	0
Coal (Wigan seven-feet coal) . . . . .	2	1	0

The interval of eight yards is in my opinion the deposit in which the fossil trees were met with.

In a distance of about fifty yards of the cutting, on my first visit to the place, I observed full thirty upright stems of *Sigillariæ*, besides several flattened ones lying in a horizontal position. These trees exhibited no evidence of their former structure, being mere casts, having their insides filled with a similar material to the matrix in which they were found imbedded. Their outsides consisted of a coating of bright coal of about a quarter of an inch in thickness, and were ribbed and formed as *Sigillariæ* usually are. In diameter they varied from one to three feet: their heights ranged from two to twelve feet; but, with one exception of a stem with another lying directly across it, none of them could be traced to their termination upwards. Some of them rested with their stems on the top of the lower seam of coal; others had their roots midway between the two seams; and others again were found just under and in the floor of the upper seam. Most of the trees, which on exposure retained their coaly envelope, presented the irregularly ribbed and furrowed appearance which the Dixon Fold and St. Helen's trees exhibited, and which some geologists contend are not sufficient to identify them with *Sigillariæ*; but six specimens were decorticated, and showed well-defined scars and all the other characters of *Sigillaria reniformis*, *alternans* and *organum*. All the upright trees had roots of *Stigmaria* with their rootlets traversing the silty clay in all directions.

Many stems of *Calamites* were found standing erect amongst the last-described trees, some of which were traced four and five feet in height without reaching their tops. These stems varied in diameter from one to five inches: they showed no structure internally, being mere casts filled with silty clay and having a coaly envelope of about one-sixth of an inch in thickness, which on being removed exposed the ribbed character

and usual joints of this genus of plants. All those which could be traced downwards exhibited rootlets proceeding from the lower joints, less in size, but resembling those of *Stigmaria*.

One of the erect *Calamites* was traced for about two feet upwards, and then at first sight appeared to terminate; but on more careful inspection it could be traced running in a horizontal direction, but so much compressed as to remain unseen without very close observation.

The erect stems both of undoubted *Sigillaria* and trees which did not exhibit all the characters of *Sigillaria* as well as those of *Calamites*, occurred in all parts of the deposit of silty clay, from the top of the lower seam to the floor of the upper one.

In the deposit where the trees occurred were found plants of the genera *Neuropteris*, *Pecopteris*, *Sphenopteris*, *Cyclopteris*, *Odontopteris*, *Asterophyllites*, *Pinnularia*, *Lepidodendron*, *Lepidophyllum*, *Lepidostrobus*, *Lycopodites*, *Sphenophyllum*, &c.

Having thus given a hasty sketch of the locality where the fossils occurred, and the fossils themselves as they appeared to me on my first visit to the place, I shall proceed to describe some erect stems of *Calamites*, which are intended to form the chief subject of this communication. These trees were not only seen by myself, but by Dr. J. Hooker and M. Jobert, two well-known geologists; and it is to the latter gentleman that I am indebted for the drawings which accompany this paper.

On the 22nd of May last, in company with the above-named gentlemen, I again visited the Pemberton Hill cutting. Many erect specimens of *Calamites*, both with and without roots, had been seen on my previous visits to the place; but the three which it is now my intention to describe exhibited the lower terminations, and more distinctly showed the rootlets than the other specimens.

The three fossils marked Nos. 1, 2 and 3, in the rough sketch before given, and No. 4, an individual examined by me on a previous visit, occurred in the excavation on the south side of the railway. They were all found standing in an erect position about two yards distant from each other, having their tops, as far as bared, two yards under the upper seam of coal. They were each exposed from twenty inches to two feet, and all presented the same external characters with regard to their stems, joints, and rootlets, and most resembled the *Calamites approximatus*.

The description of No. 1 will serve for the other two.

This specimen appeared standing in the silty clay in a nearly

erect position, with the exception of a slight bend in its upper part, as shown in the drawing. It was almost cylindrical, and measured twenty-one inches from the base to its highest part, which was exposed. Its greatest diameter, which occurred near the top, was one and a half inches; it then tapered slightly towards the bottom, and terminated in a club-shaped end. The exterior was covered with a coating of fine coal of about one-eighth of an inch in thickness, which on being removed exposed the usual ribs, furrows and joints, characteristic of *Calamites*. The interior showed no trace of structure, being composed of the same kind of silty clay as the matrix in which the fossil was found.

The following is a sketch of No. 1 as it appeared in the cutting, one-eighth the natural size of the fossil. The upper part had been removed before we saw the specimen.

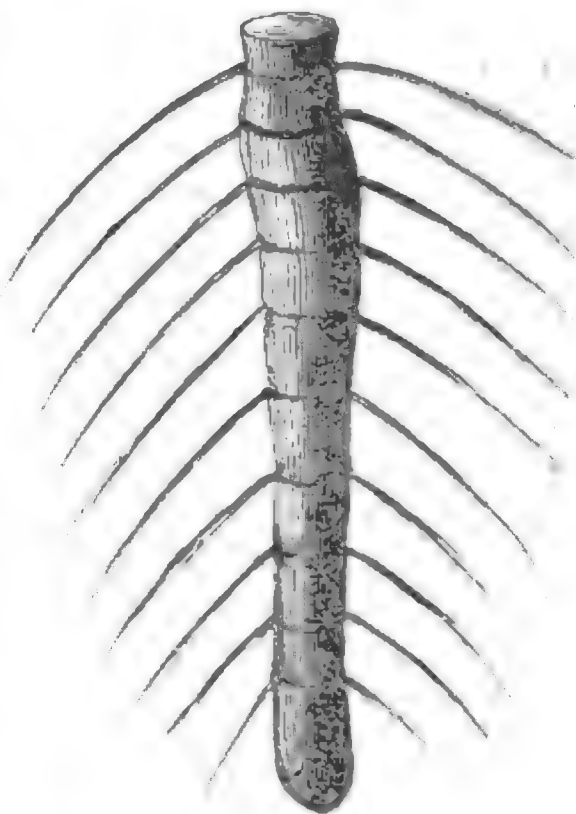
The joints or nodi were ten in number, and occurred at irregular distances, but nearer together at the upper and lower extremities than in the middle of the fossil.

At the joints small circular depressions were seen, from which proceeded rootlets. These could be traced from eight to eleven inches in length without reaching their terminations. They went down into the silty clay, the higher ones making an angle of about  $15^{\circ}$  with the horizon; but the angle gradually increased as they went lower, until they at last described an angle of about  $45^{\circ}$ .

The rootlets appeared to have been originally cylindrical and about one-eighth of an inch in diameter; but they were now compressed, and their outsides covered with a thin coating of carbonaceous matter. On a careful removal of the outside a delicate longitudinal stria could be perceived on the rootlets; there also appeared something like a pith in their middle.

Altogether the rootlets could not be well distinguished from those of *Stigmaria*. They also appeared to come from the

Fig. 2.



stems in something like quincuncial order, like the rootlets of the last-named plant; but of this circumstance I cannot speak with absolute certainty.

The specimen No. 4 differed from the other three only in its base, which did not terminate in the same club-shaped extremity which they did; but after the joints had gradually approximated it turned inwards, and it could not then be seen whether it ended or was inserted into some other stem.

In addition to the above there were many *Calamites*, both in erect, inclined, and horizontal positions, but no leaves or branches were satisfactorily traced to them.

In the course of his examination of upright stems of *Sigillariæ* in the coal-measures, the writer has nearly always found *Calamites* associated with them. At St. Helens they were abundant, and their bases were found in contact with the main roots of *Sigillariæ*. One of the authors of the *Fossil Flora*, Mr. Hutton, in describing the Burdiehouse fossils at page 24, vol. iii. of that work, states as follows: "Amongst vegetables, the characteristic fossils of this deposit are *Lepidostrophi*, *Lepidophyllites*, *Lepidodendra*, and *Filicites*; the rarity of *Calamites*, which occur but seldom, and of a diminutive size, and the almost entire absence of *Stigmaria*, are very striking to those who are accustomed to view the fossil groups usually presented by the beds of the carboniferous formation; whilst the profusion of *Lepidostrophi* and *Lepidophyllites* of various sizes and in various stages of growth associated with the stems of *Lepidodendra* and those of no other plant, is an additional argument for the opinion which has always appeared highly probable, that they were the fruit, leaves and stem of the same tribe of plants. Of *Sigillaria*, a plant which in the flora of the carboniferous group generally is of so much importance, we could not observe a trace."

In the course of his own observations, the writer has never yet been able to meet with a stem of *Sigillaria* of so small a size as six inches in diameter, or a *Calamites* of so large a size as that. Doubtless there must have been young *Sigillariæ* whether or not there were large *Calamites*. Now what are young *Sigillariæ*? This is a question which yet remains to be answered.

It is now admitted that little is known about the true nature of the genera *Sigillaria* and *Calamites*, except that they were not the hollow succulent stems which they were once supposed to be.

The rootlets of *Calamites*, as previously shown, if not actually identical with, at least very much resemble those of *Sigillaria*. In some specimens of this latter genus, especially those



of the species *approximatus*, figured and described in plate 216, vol. iii. of the Fossil Flora, and the *cruciatus*, figured in plate 19 of Brongniart's *Histoire des Végétaux Fossiles*, their rootlets are arranged in regular quincuncial order. In the largest Calamites that to my knowledge has been figured, namely, that called *Gigas*, plate 27 in Brongniart's work before alluded to, the ribs and furrows begin to appear very like those of *Sigillaria*, and the joints show indistinctly. The termination of the root of a Calamites is exactly of the same form as the terminal point of a *Stigmara*, both being club-shaped.

I am not aware that up to the present time much, if anything, is known of the structure of Calamites; but if it should resemble that of *Sigillaria*, it may tend to prove that Calamites are but young *Sigillaria*.

In our observations it must not however be lost sight of, that no central axis or pith has to my knowledge yet been discovered in the stem of the Calamites like that found in *Sigillaria*. Both plants are proved to have had similar *habitats*, and therefore it is very probable that they might have had rootlets resembling each other without being the same plant. Still, however, as *Sigillaria* was so long considered a separate plant from *Stigmara*, it is unphilosophical to take no notice of the analogies of what are now considered distinct genera. Although it will not by any means be safe to affirm that *Sigillaria* and Calamites are the same plant from their analogies, still it is conceived that sufficient evidence has been adduced in this paper to prove that the latter as well as the former plants have generally grown on the places where they are now found, and that the reason why one is so much more frequently found in an erect position than the other, arises from the circumstance of the stem of the one being much stronger than that of the other. A deposit of mud on the branches and leaves of the slender stem of a Calamites might weigh it down and prostrate it, whilst the stout trunk of the *Sigillaria* would resist such action and continue erect.

XLIV. *Upon the Chemical Constitution of Metacetic Acid, and some other Bodies related to it.* By E. FRANKLAND, Esq. and H. KOLBE, Ph.D.\*

THE researches into the constitution of organic compounds certainly belong to the most interesting in chemistry. But they are always attended with more or less danger, and those who, leaving the safer road of experiment, plunge into the depths of hypothesis, and build up theories apparently

\* Communicated by the Chemical Society; having been read April 19, 1847.

ingenious, though often untenable, frequently stumble and fall amongst a host of contradictions. It is a common error, as experience teaches, into which young chemists are very apt to fall, that, persuaded of the infallibility of their own views, and blind to well-founded objections, they endeavour to convince by quick and ready argument rather than by solid reasoning, and consequently they either offend others or feel themselves offended when contradicted.

When in the face of this danger we endeavour to advance views concerning the rational composition of some organic acids which do not accord with those generally received, we do it with a certain degree of timidity, and with the most strenuous endeavour to avoid those causes of error which have been pointed out. It is far from our intention to give a decided preference to the mode of viewing the subject here proposed, or indeed in any way to force our own opinion, nor is it unknown to us that even these views leave many facts unexplained; but we feel convinced that no detriment can accrue to the progress of science by looking at subjects of such importance with an impartial eye from all possible sides.

The starting-point of our experiments was the idea recently expressed by Berzelius, that acetic acid might be considered as a conjugate oxalic acid, as methyl-oxalic acid,  $C_2H_3C_2O_5$ . If this view of the subject, which explains so readily the conversion of acetic acid into chloracetic acid, and the remarkable reconversion of the latter into the former, and which has been further confirmed by the analogous relations of chlorocarbohyposulphuric acid and methyl-hyposulphuric acid, is correct, then the question arises whether it might not be extended to those other acids, so nearly related to acetic acid, namely, formic acid, metacetic acid, butyric acid, benzoic acid, &c. We are of opinion that this question cannot *à priori* be answered in the negative; on the contrary, it appeared to us, after pursuing the subject further from that point of view, that the manifold metamorphoses which the above combinations undergo might be explained in a very simple manner, and we consequently have submitted the question to careful experimental examination; and we believe that we have gained a fact in support of the theory of conjugate compounds in its application to the acids in question, by the action which cyanide of ethyle exhibits with alkalies and acids.

When benzoic acid is supposed to consist of oxalic acid with the carburetted hydrogen,  $C_{12}H_5$  (phenyle), as a conjunct, then it is evident that benzoe-nitrile obtained by Fehling\* in distilling benzoate of ammonia, must be a cyanogen

\* Liebig's *Annalen*, xlix. p. 91.

compound of the same carbo-hydrogen =  $C_{12}H_5Cy$ . This mode of decomposition of phenyle-oxalic acid becomes thus completely analogous to the well-known formation of cyanogen by heating oxalate of ammonia, and to the formation of hydrocyanic acid from the formiate of ammonia.

Benzoe-nitrile, viewed as cyanide of phenyle, would then, together with the analogous body recently discovered by Schlieper\*, valero-nitrile ( $C_8H_9Cy$ , cyanide of valyle), become allied to cyanide of ethyle; and as these bodies in contact with alkalies are so easily transformed into benzoic acid and valerianic acid, it is to be presumed that cyanide of ethyle under similar circumstances would become transformed into ammonia and metacetic acid.

We prepared for this purpose pure cyanide of ethyle, according to the process of Pelouze, by the distillation of sulphovinate of potash with cyanide of potassium. The yellow-coloured liquid which passes over† was mixed with water, and separated again by chloride of sodium, dried over chloride of calcium, and lastly distilled in a bent tube freed from air and hermetically sealed. Purified in this manner cyanide of ethyle is a limpid colourless liquid, having an odour much resembling that of the terrible cacodyle. The analysis of the substance gave the following numbers:—

0.219 grm. gave 0.523 grm. of carbonic acid and 0.186 grm. of water.

			Theory.
Carbon . .	65.19	6	65.45
Hydrogen . .	9.46	5	9.09
Nitrogen . .	25.35	1	25.46

To settle the question started above, this cyanide of ethyle was added drop by drop to a tolerably concentrated boiling solution of caustic potash, and the product of distillation returned to the retort as long as it retained any smell. During this operation a considerable portion of ammonia was given off. The alkaline residue distilled with sulphuric acid produced an acid liquid, which, neutralized with carbonate of silver, baryta, or lead, gave the corresponding salts of those bases. We had previously satisfied ourselves, by a carefully conducted experiment, that no formic acid was present in the acid solution.

The silver salt crystallizes from its aqueous solution in small acicular prisms. It is sparingly soluble in water, and

\* *Annalen der Chemie*, lix. p. 15.

† We found, in contradiction to Pelouze's statement, that cyanide of ethyle is tolerably soluble in water; but when the solution is saturated with common salt, it again separates unchanged and comes to the surface.

the solution becomes blackened on boiling. The crystals dried over sulphuric acid *in vacuo* had the composition of metacetate of silver.

I. 0.211 grm. gave, when burnt with oxide of copper, 0.153 grm. carbonic acid and 0.055 grm. water.

II. 0.167 grm. gave on careful ignition 0.100 grm. of metallic silver.

Carbon . . .	19.77	6	19.90
Hydrogen . . .	2.89	5	2.76
Oxygen . . .	13.06	3	13.27
Oxide of silver .	64.28	1	64.07
	<hr/> 100.00	<hr/> 15	<hr/> 100.00

The salt of barytes is very soluble in water; the solution of the salt evaporated to dryness and dried for a long time at 212°, gave the following numbers:—

I. 0.339 grm. gave 0.311 grm. carbonic acid and 0.116 grm. water.

II. 0.258 grm. gave 0.211 grm. sulphate of baryta.

			Theory.
Carbon . . .	24.98	6	25.46
Hydrogen . . .	3.79	5	3.53
Oxygen . . .	17.58	3	16.99
Barytes . . .	53.65	1	54.02
	<hr/> 100.00		<hr/> 100.00

The lead compound has the sweet taste of acetate of lead: it does not appear to crystallize, but dries up to a tough amorphous saline mass. A portion dried at 212° and *in vacuo* gave exactly the quantity of oxide of lead, corresponding to the formula  $PbO, C_6 H_5 O_3$ , namely,—

0.446 grm. gave 0.282 grm. oxide of lead and 0.021 grm. metallic lead, equivalent to 63.40 per cent. instead of 63.19.

From the above analyses, there can be no doubt that the acid product of the action of caustic potash upon cyanide of ethyle is metacetic acid. The same result is produced by weak sulphuric acid (1 part acid to 2 parts water). The silver salt prepared from the acid product of distillation in this case exhibited the properties described above, and an estimation of the oxide of silver gave the following result:—

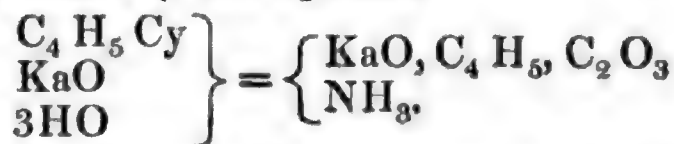
I. 0.150 grm. when ignited gave 0.090 grm. metallic silver, corresponding to 64.30 per cent. oxide of silver; the theoretical proportion being 64.07 per cent.

The mode of decomposition of cyanide of ethyle described is therefore quite analogous to that of benzoe-nitrile and valero-nitrile; and if metacetic acid is considered as ethyl-



270 *On the Chemical Composition of Metacetic Acid.*

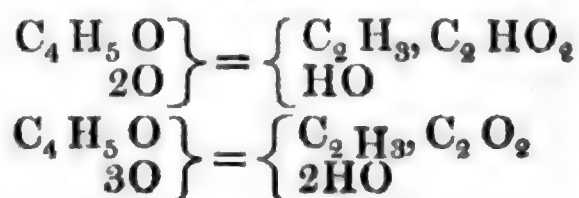
oxalic acid, with the formula  $C_4 H_5 C_2 O_3$ , the decomposition may be expressed by the equation



The assumption will be rendered still more palpable, if, as there is little doubt, at a future period we are able again to produce cyanide of ethyle from metacetate of ammonia.

If, on the contrary, metacetic acid is considered as an oxide of the radical  $C_6 H_5$ , then it must be assumed, that during the process of oxidation the two atoms of carbon, in the cyanogen, amalgamate themselves, as it were, with ethyle, in order to form the new radical metacetyl. Setting aside the great improbability attached to such an assumption, to which there would be at present no analogous case, we feel justified in giving preference to the former explanation, because it is the more simple, and because it completely agrees with the known transformation of cyanogen and water into oxalic acid and ammonia; and if, as one of us has found, valerianic acid, placed in the circuit of the voltaic current, by the assumption of 1 atom of oxygen, becomes converted into carbonic acid and the carbo-hydrogen  $C_8 H_9$ , we consider that our view has gained by that fact an additional support, and that the acid in question contains that carbo-hydrogen in conjugate combination with oxalic acid.

The foregoing observations lead to a great simplification of our hypothesis in regard to organic radicals, inasmuch as they do away with the necessity of supposing a specific radical for each acid belonging to an alcohol. The series of radicals which are produced by the addition of one or more equivalents of the carbo-hydrogen  $C_2 H_2$  to 1 equivalent of hydrogen, namely  $C_2 H_3$ ,  $C_4 H_5$ , &c., the hydrated oxides of which form the alcohols, are again found in accordance with our view in the acids derived from them, conjoined with oxalic acid. It may be conceived that the oxygen, in converting alcohol or oxide of ethyle into acetic acid, first acts upon the equivalent of carbo-hydrogen  $C_2 H_2$ , which alone distinguishes ethyle from methyle, and that the successive products of oxidation of this body take up the remaining radical methyle into conjugate combination; thus in the formation of aldehyde and acetic acid,



Moreover, it cannot be denied that our ideas concerning the functions performed by compound radicals are very much enlarged by these considerations. For when we find methyle and ethyle combining like the electro-positive inorganic elements with the electro-negative non-metallic substances, the property which they also exhibit of uniting with oxalic acid, hyposulphuric acid, and with other, perhaps even neutral, bodies to form conjugate compounds, evinces such an extensive range of properties as is nowhere to be met with amongst the more narrowly defined powers of combination of inorganic substances; and it is probable that nature, when she brings forth the innumerable and manifold products of the organic kingdom by a wonderful combination of those few elements which are at her disposal, may likewise make use of these supposed extensive combining powers of the organic radicals, as the simplest means of accomplishing her greatest works.

We beg to express our warmest thanks to Dr. Lyon Playfair for the use of his laboratory and apparatus in carrying out the above investigation, and for the uniform kindness which we as his assistants have experienced at his hands.

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**XLV. *Analysis of the Ashes of the Orange-Tree* (Citrus aurantium). By Messrs. THOMAS H. ROWNEY and HENRY How\*.**

**F**OR the materials used in the following analyses we are indebted to the kindness of Mr. Da Cumara, who had sent it over for investigation from his plantations on the island of St. Michel, being desirous to become acquainted with the mineral constituents of the orange-tree, which forms the principal wealth of his country. The analyses were performed under the direction of Dr. Hofmann in the laboratory of the Royal College of Chemistry.

To prepare the ashes in a fit state for analysis, the different parts of the plant were heated in an inclined, open Hessian crucible, until the carbon was consumed†. The ashes thus obtained were mixed with a small quantity of oxide of mercury and ignited a second time in a platinum capsule over a spirit-lamp, in order to reproduce the sulphates, which in the former process had been reduced to sulphides.

\* Communicated by the Chemical Society; having been read April 5, 1847.

† To obtain the ash of the fruit, the oranges were cut into slices, and after separation of the seed dried on the sand-bath in a covered porcelain dish, and then burnt in a crucible.

The same quantity of ash served to determine the potash and soda, sulphuric and phosphoric acids, perphosphate of iron, lime and magnesia, silicic acid and accidental sand and charcoal. For this purpose, the hydrochloric acid solution was evaporated to dryness, gently ignited and extracted with hydrochloric acid. The solution thus obtained was divided into different parts. The first portion served for the determination of the potash and soda.

For this purpose the acids, lime, magnesia, &c. were removed by baryta, the excess of baryta by carbonate of ammonia, and the ammoniacal salts by gentle ignition. The residue, potash and soda, were estimated partly by separating them by means of bichloride of platinum (analyses of the ashes of the root and seed) and partly by the indirect method, namely, by converting the mixed chlorides into sulphates, weighing these and ascertaining the amount of sulphuric acid by means of chloride of barium (analyses of the stem, leaves and fruit).

In the second portion, sulphuric and phosphoric acids were determined, the former as sulphate of baryta, the latter by neutralizing the filtrate from the former with ammonia and precipitating the phosphoric acid by means of sesquichloride of iron and acetate of potash. This precipitate was dissolved in hydrochloric acid, a sufficient quantity of tartaric acid was added, and the phosphoric acid estimated in the form of pyrophosphate of magnesia, by precipitating with ammonia, chloride of ammonium and sulphate of magnesia. The latter precipitate, frequently containing a small quantity of iron, was redissolved in hydrochloric acid, and after the addition of some tartaric acid reprecipitated by ammonia. A third portion served for the estimation of perphosphate of iron, lime and magnesia. For this purpose the liquid was neutralized with ammonia, some acetate of potash was added, and the solution strongly acidulated with acetic acid, in order to keep the phosphate of lime, which might be precipitated, in solution; on heating perphosphate of iron subsides, from which the sesquioxide of iron was calculated according to the formula  $2\text{Fe}_2\text{O}_3 + 3\text{PO}_5$ . From the filtrate the lime was precipitated by means of oxalate of ammonia, and after the separation of the lime, the magnesia by means of phosphate of soda. Chlorine and carbonic acid were determined in separate portions of the ash. In this manner the following experimental numbers were obtained:—

	Roots.		Stem.		Leaves.		Fruit.		Seed.	
	I. grms.	II. grms.	I. grms.	II. grms.	I. grms.	II. grms.	I. grms.	II. grms.	I. grms.	II. grms.
Quantity of ash employed for the general analysis .....	2·9360	3·07	{ 3·5800 3·2350 4·6265 }	3·277	4·3040	4·653	3·7490	3·5415	1·3620	2·344
Whole amount of the hydrochloric acid solution .....	164·634	154·059	{ 229·956 222·683 }	139·376	202·696	171·503	207·520	169·273	171·100	181·45
Hydrochloric acid solution for the alkalis .....	21·320	23·05	23·226	22·94	28·913	20·422	20·716	23·868	41·100	39·948
Hydrochloric acid solution for sulphuric and phosphoric acids .....	24·989	35·442	{ 39·393 27·456 }	{ 25·99 16·18 }	23·616	22·574	33·013	18·22	25·630	34·288
Hydrochloric acid solution for lime, magnesia, &c. ...	31·959 25·686	39·155	{ 38·506 36·693 }	26·331 25·827	22·830 21·550	24·324	{ 27·356 21·716 }	22·568	23·945	27·391
Quantity of ash employed for the estimation of chlorine	1·4955	1·2235	1·1060	1·291	2·1230	1·927	1·9825	1·4595	·8270	0·6255
Quantity of ash employed for the estimation of carbonic acid .....	·8400	.....	0·884	.....	1·5330	1·619	1·7660	1·5275	0·585	
Quantity of the plant dried at 100° C. for the estimation of the amount of ash	1·7175	.....	1·1280	.....	1·9892	.....	3·5450	.....	3·7825	



	Roots.		Stem.		Leaves.		Fruit.		Seed.	
	I. grms.	II. grms.	I. grms.	II. grms.	I. grms.	II. grms.	I. grms.	II. grms.	I. grms.	II. grms.
Silicic acid .....	·0405	0·0445	·0330	0·04375	·1580	0·176	·0115	0·0135	·0140	0·0225
Sand and charcoal .....	·0125	0·0195	·0120	0·007 <sub>9</sub>	·0105	0·010	·0635	0·0575	·0785	0·1325
Mixed chlorides of potassium and sodium ..	·8185	0·846	.....	.....	.....	.....	.....	.....	·7913	.....
Potassio-chloride of platinum .....	1·9089	.....	.....	.....	.....	.....	.....	.....	2·4861	4·294
Chloride of sodium .....	·2355	0·242	.....	.....	.....	.....	.....	.....	·0321	0·052
Mixed sulphates of the alkalis .....	.....	.....	·7772	0·784	1·4091	1·511	2·8349	2·7127	.....	.....
Sulphuric acid contained in the sulphates of the alkalis .....	.....	.....	·3772	0·379	·6863	0·739	1·3914	1·333	.....	.....
Sulphate of baryta for sulphuric acid .....	·3946	0·42	·3677	0·362	·4205	0·471	·3143	0·3028	·1769	0·306
Pyrophosphate of magnesia for phosphoric acid .....	·5007	0·53	·7244	0·73	·1673	0·19	·5060	0·483	·4372	0·7546
Perphosphate of iron .....	·0566	0·588	·0358	0·0375	·0355	0·0493	·0303	0·03	·0214	0·0397
Carbonate of lime .....	2·1059	2·21	3·7565	2·69	3·3296	3·61	1·2858	1·191	·4037	0·697
Pyrophosphate of magnesia for magnesia .....	·4454	0·47	·6675	0·469	·5282	0·547	·6545	0·59	·2929	0·4809
Chloride of silver .....	·0370	0·0275	·0055	0·008	·2640	0·245	·1425	0·111	·0160	0·0105
Carbonic acid .....	0·1600	.....	0·146	.....	·3560	0·372	·3690	·3090	0·04	.....
Amount of ash left on incineration .....	·0770	.....	·0310	.....	·2732	.....	·1400	.....	·1250	.....

These numbers correspond to the following per-centage :—

*Analysis of the Ashes of the Root. Composition directly found.*

Amount of ashes left by 100 parts of the root . . . 4.48

	I.	II.	Mean.
Potash . . . . .	12.54	12.40	12.47
Soda . . . . .	3.72	3.57	3.64
Lime . . . . .	40.16	40.31	40.23
Magnesia . . . . .	5.55	5.60	5.57
Sesquioxide of iron	0.83	0.82	0.83
Chloride of sodium	1.01	0.91	0.95
Phosphoric acid .	10.80	10.93	10.86
Sulphuric acid . .	4.61	4.76	4.68
Silicic acid . . .	1.38	1.45	1.42
Carbonic acid . .	19.04	19.04	19.04
Sand and charcoal	0.42	0.63	0.53
	<u>100.06</u>	<u>100.37</u>	<u>100.22</u>

Per-centage of the ash after deduction of the unessential constituents, carbonic acid, sand and charcoal:—

Potash . . . . .	15.43
Soda . . . . .	4.52
Lime . . . . .	49.89
Magnesia . . . . .	6.91
Sesquioxide of iron	1.02
Chloride of sodium	1.18
Phosphoric acid .	13.47
Sulphuric acid . .	5.78
Silicic acid . . .	1.75
	<u>100.00</u>

*Analysis of the Ashes of the Stem.*

Amount of ashes left by 100 parts of the stem . . . 2.74

	I.	II.	Mean.
Potash . . . . .	9.66	9.73	9.69
Soda . . . . .	2.61	2.47	2.54
Lime . . . . .	45.46	45.96	45.71
Magnesia . . . . .	5.28	5.24	5.26
Sesquioxide of iron	0.48	0.48	0.48
Chloride of sodium	0.19	0.24	0.21
Phosphoric acid .	14.18	14.17	14.17
Sulphuric acid . .	3.90	3.79	3.84
Silicic acid . . .	0.92	1.14	1.03
Carbonic acid . .	16.51	16.50	16.50
Sand and charcoal	0.33	0.21	0.27
	<u>99.52</u>	<u>99.93</u>	<u>99.70</u>

T 2

Potash . . . .	11.69
Soda . . . .	3.07
Lime . . . .	55.13
Magnesia . . . .	6.34
Sesquioxide of iron	0.57
Chloride of sodium	0.25
Phosphoric acid .	17.09
Sulphuric acid . .	4.64
Silicic acid . . .	1.22
	<hr/> 100.00

*Analysis of the Ashes of the Leaves.*

Amount of ashes left by 100 parts of the leaves . . 13.73

	I.	II.	Mean.
Potash . . . .	12.87	12.48	12.67
Soda . . . .	1.22	1.38	1.30
Lime . . . .	43.32	43.44	43.38
Magnesia . . . .	4.49	4.30	4.39
Sesquioxide of iron	0.36	0.44	0.40
Chloride of sodium	5.08	5.17	5.12
Phosphoric acid .	2.46	2.58	2.52
Sulphuric acid . .	3.35	3.47	3.41
Silicic acid . . .	3.67	3.78	3.72
Carbonic acid . .	23.22	22.97	23.09
Sand and charcoal	0.24	0.21	0.23
	<hr/> 100.28	<hr/> 100.22	<hr/> 100.23

Potash . . . .	16.51
Soda . . . .	1.68
Lime . . . .	56.38
Magnesia . . . .	5.72
Sesquioxide of iron	0.52
Chloride of sodium	6.66
Phosphoric acid .	3.27
Sulphuric acid . .	4.43
Silicic acid . . .	4.83
	<hr/> 100.00

*Analysis of the Ashes of the Fruit.*

Amount of ashes left by 100 parts of the fruit . . 3.94

	I.	II.	Mean.
Potash . . . .	28.21	28.32	28.26
Soda . . . .	8.73	8.99	8.86
Lime . . . .	19.20	18.83	19.02
Magnesia . . .	6.39	6.14	6.26
Sesquioxide of iron	0.35	0.36	0.35
Chloride of sodium	2.93	3.09	3.02
Phosphoric acid .	8.55	8.64	8.59
Sulphuric acid . .	2.88	2.93	2.90
Silicic acid . . .	0.31	0.38	0.34
Carbonic acid . .	20.38	20.22	20.30
Sand and charcoal	1.69	1.62	1.65
	<u>99.62</u>	<u>99.52</u>	<u>99.55</u>

Potash . . . .	36.42
Soda . . . .	11.42
Lime . . . .	24.52
Magnesia . . . .	8.06
Sesquioxide of iron	0.46
Chloride of sodium	3.87
Phosphoric acid .	11.07
Sulphuric acid . .	3.74
Silicic acid . . .	0.44
	<u>100.00</u>

*Analysis of the Ashes of the Seed.*

Amount of ashes left by 100 parts of the seed . . 3.30

	I.	II.	Mean.
Potash . . . .	35.22	35.29	35.26
Soda . . . .	0.77	0.84	0.81
Lime . . . .	16.59	16.65	16.62
Magnesia . . .	7.87	7.51	7.69
Sesquioxide of iron	0.68	0.72	0.70
Chloride of sodium	0.77	0.67	0.72
Phosphoric acid .	20.33	20.39	20.36
Sulphuric acid . .	4.46	4.48	4.47
Silicic acid . . .	1.02	0.96	0.99
Carbonic acid . .	6.83	6.83	6.83
Sand and charcoal	5.76	5.78	5.77
	<u>100.30</u>	<u>100.12</u>	<u>100.22</u>



Potash . . . . .	40·28
Soda . . . . .	0·92
Lime . . . . .	18·97
Magnesia . . . . .	8·74
Sesquioxide of iron . . . . .	0·80
Chloride of sodium . . . . .	0·82
Phosphoric acid . . . . .	23·24
Sulphuric acid . . . . .	5·10
Silicic acid . . . . .	1·13—100·00

The preceding analyses furnish a new confirmation of the fact first observed by De Saussure, namely, that the largest amount of mineral constituents is deposited in those parts of the plant in which the process of assimilation appears to be most active. While the ash left by the root, stem, fruit and seed did not exceed from 3 to 4 per cent., the leaves left not less than 13 per cent. of fixed residue on incineration.

Regarding the composition of the different ashes, the great amount of carbonic acid found in the ashes of the root, the stem, and the fruit is at once obvious; proving that not only the fruit, but also the roots and stem, contain a large quantity of organic acids.

From the composition of the ashes of the root, the stem, and the leaves, the orange-tree belongs decidedly to the lime plants. In these three ashes the joint amount of lime and magnesia exceeds the quantity of the rest of the mineral constituents. In the ashes of the fruit and seed, however, the alkalies are as prevalent as they have been found in analogous cases. The amount of phosphoric acid (23·24) in the ash of the seed is considerable, as might be expected, still it is inferior to the quantities (34·81 and 42·02) which Mr. Souchay found on analysing the seeds of the citron (*Citrus Medica*) and quince-trees (*Pyrus Cydonia*). Nevertheless the ash of the orange-seed is very analogous in composition to the ashes of the last-mentioned seeds, as may be easily seen on comparing their analysis\*.

XLVI. *On Quaternions; or on a New System of Imaginaries in Algebra.* By Sir WILLIAM ROWAN HAMILTON, LL.D., V.P.R.I.A., F.R.A.S., Corresponding Member of the Institute of France, &c., Andrews' Professor of Astronomy in the University of Dublin, and Royal Astronomer of Ireland.

[Continued from p. 219.]

37. **R**ESUMING now the quaternion form of the equation of the ellipsoid,

$$(\alpha\rho + \rho\alpha)^2 - (\beta\rho - \rho\beta)^2 = 1, \quad . \quad . \quad . \quad . \quad (1.)$$

\* Liebig's Annals, liv. p. 343.

and making

$$\alpha + \beta = \frac{i}{i^2 - x^2}, \quad \alpha - \beta = \frac{x}{i^2 - x^2}, \quad . . . \quad (2.)$$

and

$$\frac{i\rho + \rho x}{i^2 - x^2} = Q, \quad \frac{\rho i + x\rho}{i^2 - x^2} = Q', \quad . . . \quad (3.)$$

the two linear factors of the first member of the equation (1.) become the two conjugate quaternions  $Q$  and  $Q'$ , so that the equation itself becomes

$$QQ' = 1. . . . . (4.)$$

But by articles 19 and 20 (Phil. Mag. for July 1846), the product of any two conjugate quaternions is equal to the square of their common tensor; this common tensor of the two quaternions  $Q$  and  $Q'$  is therefore equal to unity. Using, therefore, as in those articles, the letter  $T$  as the characteristic of the operation of *taking the tensor* of a quaternion, the equation of the ellipsoid reduces itself to the form

$$TQ = 1; . . . . . (5.)$$

or, substituting for  $Q$  its expression (3.),

$$T \left( \frac{i\rho + \rho x}{i^2 - x^2} \right) = 1; . . . . . (6.)$$

which latter form might also have been obtained, by the substitutions (2.), from the equation (3.) of the 30th article (Phil. Mag., June 1847), namely from the following\*:

$$T(\alpha\rho + \rho\alpha + \beta\rho - \rho\beta) = 1. . . . . (7.)$$

38. In the geometrical construction or generation of the ellipsoid, which was assigned in the preceding articles of this paper (see the Numbers of the Philosophical Magazine for June and September 1847), the significations of some of the recent symbols are the following. The two constant vectors  $i$  and  $x$  may be regarded as denoting, respectively, (in lengths and in directions,) the two sides of the generating triangle  $ABC$ , which are drawn from the centre  $c$  of the auxiliary and diacentric sphere, to the fixed superficial point  $B$  of the ellipsoid, and to the centre  $A$  of the same ellipsoid; the third side of the triangle, or the vector from  $A$  to  $B$ , being therefore denoted (in length and in direction) by  $i - x$ : while  $\rho$  is the radius vector of the ellipsoid, drawn from the centre  $A$  to a

\* See equation (35.) of the Abstract in the Proceedings of the Royal Irish Academy for July 1846. The equation of the ellipsoid marked (1.) in article 37 of the present paper, was communicated to the Academy in December 1845, and is numbered (21.) in the Proceedings of that date.

variable point  $E$  of the surface; so that the constant vector  $1-x$  is, by the construction, a particular value of this variable vector  $\rho$ . The vector from  $A$  to  $C$ , being the opposite of that from  $C$  to  $A$ , is denoted by  $-x$ ; and if  $D$  be still the same auxiliary point on the surface of the auxiliary sphere, which was denoted by the same letter in the account already printed of the construction, then the vector from  $C$  to  $D$ , which may be regarded as being (in a sense to be hereafter more fully considered) the *reflexion* of  $-x$  with respect to  $\rho$ , is  $= -\rho x \rho^{-1}$ ; and consequently the vector from  $D$  to  $B$  is  $= 1 + \rho x \rho^{-1}$ . The lengths of the two straight lines  $BD$ , and  $AE$ , are therefore respectively denoted by the two tensors,  $T(1 + \rho x \rho^{-1})$  and  $T\rho$ ; and the rectangle under those two lines is represented by the product of these two tensors, that is by the tensor of the product, or by  $T(\rho + \rho x)$ . But by the fundamental equality of the lengths of the diagonals,  $AE$ ,  $BD'$ , of the plane quadrilateral  $ABED'$  in the construction, this rectangle under  $BD$  and  $AE$  is equal to the constant rectangle under  $BD$  and  $BD'$ , that is under the whole secant and its external part, or to the square on the tangent from  $B$ , if the point  $B$  be supposed external to the auxiliary sphere, which has its centre at  $C$ , and passes through  $D$ ,  $D'$ , and  $A$ . Thus  $T(\rho + \rho x)$  is equal to  $(T1)^2 - (Tx)^2$ , or to  $x^2 - 1^2$ , which difference is here a positive scalar, because it is supposed that  $CB$  is longer than  $CA$ , or that

$$T1 > Tx; \quad . \quad . \quad . \quad . \quad . \quad . \quad (8.)$$

and the quaternion equation (6.) of the ellipsoid reproduces itself, as a result of the geometrical construction, under the slightly simplified form \*

$$T(\rho + \rho x) = x^2 - 1^2. \quad . \quad . \quad . \quad . \quad . \quad (9.)$$

And to verify that this equation relative to  $\rho$  is satisfied (as we have seen that it ought to be) by the particular value

$$\rho = 1 - x, \quad . \quad . \quad . \quad . \quad . \quad (10.)$$

which corresponds to the particular position  $B$  of the variable point  $E$  on the surface of the ellipsoid, we have only to observe that, identically,

$$\begin{aligned} 1(1-x) + (1-x)x &= 1^2 - 1x + 1x - x^2 \\ &= 1^2 - x^2 = -(x^2 - 1^2); \end{aligned}$$

and that (by article 19) the tensor of a negative scalar is equal to the positive opposite thereof.

39. The foregoing article contains a sufficiently simple

\* See the Proceedings of the Royal Irish Academy for July 1846, equation (44.).

process for the *retranslation* of the geometrical construction\* of the ellipsoid described in article 31, into the language of the calculus of quaternions, from which the construction itself had been originally derived, in the manner stated in the 30th article of this paper. Yet it may not seem obvious to readers unfamiliar with this calculus, why the expression  $-\rho x \rho^{-1}$  was taken, in that foregoing article 38, as one denoting, in length and in direction, that radius of the auxiliary sphere which was drawn from  $c$  to  $D$ ; nor in what sense, and for what reason, this expression  $-\rho x \rho^{-1}$  has been said to represent the reflexion of the vector  $-x$  with respect to  $\rho$ . As a perfectly clear answer to each of these questions, or a distinct justification of each of the assumptions or assertions thus referred to, may not only be useful in connection with the present mode of considering the ellipsoid, but also may throw light on other applications of quaternions to the treatment of geometrical and physical problems, we shall not think it an irrelevant digression to enter here into some details respecting this expression  $-\rho x \rho^{-1}$ , and respecting the ways in which it may present itself in calculations such as the foregoing. Let us therefore now denote by  $\sigma$  the vector, whatever it may be, from  $c$  to  $D$  in the construction ( $c$  being still the centre of the sphere); and let us propose to find an expression for this sought vector  $\sigma$ , as a function of  $\rho$  and of  $x$ , by the principles of the calculus of quaternions.

40. For this purpose we have first the equation between tensors,

$$T\sigma = Tx; \quad . \quad . \quad . \quad . \quad . \quad . \quad (11.)$$

which expresses that the two vectors  $\sigma$  and  $x$  are equally long, as being both radii of one common auxiliary sphere, namely those drawn from the centre  $c$  to the points  $D$  and  $A$ . And secondly, we have the equation

$$V.(\sigma - x)\rho = 0, \quad . \quad . \quad . \quad . \quad . \quad . \quad (12.)$$

where  $V$  is the characteristic of the operation of *taking the vector* of a quaternion; which equation expresses immediately that the product of the two vectors  $\sigma - x$  and  $\rho$  is scalar, and

\* The brevity and novelty of this rule for constructing that important surface may perhaps justify the reprinting it here. It was as follows: From a fixed point  $A$  on the surface of a sphere, draw a variable chord  $AD$ ; let  $D'$  be the second point of intersection of the spheric surface with the secant  $BD$ , drawn to the variable extremity  $D$  of this chord  $AD$  from a fixed external point  $B$ ; take the radius vector  $AE$  equal in length to  $BD'$ , and in direction either coincident with, or opposite to, the chord  $AD$ ; the locus of the point  $E$ , thus constructed, will be an ellipsoid, which will pass through the point  $B$  (and will have its centre at  $A$ ). See Proceedings of the Royal Irish Academy for July 1846.



therefore that these two vector-factors are either exactly similar or exactly opposite in direction; since otherwise their product would be a quaternion, having always a vector part, although the scalar part of this quaternion-product  $(\sigma - \kappa)\rho$  might vanish, namely by the factors becoming perpendicular to each other. Such being the immediate and general signification of the equation (12.), the justification of our establishing it in the present question is derived from the consideration that the radius vector  $\rho$ , drawn from the centre  $\Lambda$  to the surface  $\Xi$  of the ellipsoid, has, by the construction, a direction either exactly similar or exactly opposite to the direction of that *guide-chord* of the auxiliary sphere which is drawn from  $\Lambda$  to  $D$ , that is, from the end of the radius denoted by  $\kappa$  to the end of the radius denoted by  $\sigma$ . For, that the chord so drawn is properly denoted, in length and in direction, by the symbol  $\sigma - \kappa$ , follows from principles respecting *addition and subtraction of directed lines*, which are indeed *essential*, but are *not peculiar*, to the geometrical applications of quaternions; had occurred, in various ways, to several independent inquirers, before quaternions (as *products or quotients of directed lines in space*) were thought of; and are now extensively received.

41. The two equations (11.) and (12.) are evidently both satisfied when we suppose  $\sigma = \kappa$ ; but because the point  $D$  is in general different from  $\Lambda$ , we must endeavour to find another value of the vector  $\sigma$ , distinct from  $\kappa$ , which shall satisfy the same two equations. Such a value, or expression, for this sought vector  $\sigma$  may be found at once, so far as the equation (12.) is concerned, by observing that, in virtue of this latter equation,  $\sigma - \kappa$  must bear some scalar ratio to  $\rho$ , or must be equal to this vector  $\rho$  multiplied by some scalar coefficient  $x$ , so that we may write

$$\sigma = \kappa + x\rho; \quad . \quad . \quad . \quad . \quad . \quad (13.)$$

and then, on substituting this expression for  $\sigma$  in the former equation (11.), we find that  $x$  must satisfy the condition

$$T(\kappa + x\rho) = T\kappa, \quad . \quad . \quad . \quad . \quad . \quad (14.)$$

in which this sought coefficient  $x$  is supposed to be some scalar different from zero, that is, in other words, some positive or negative number. Squaring both members of this last condition, and observing that by article 19 the square of the tensor of a vector is equal to the negative of the square of that vector, we find the new equation

$$-(\kappa + x\rho)^2 = -\kappa^2, \quad . \quad . \quad . \quad . \quad . \quad (15.)$$

But also, generally, if  $\kappa$  and  $\rho$  be vectors and  $x$  a scalar,

$$(\kappa + x\rho)^2 = \kappa^2 + x(\kappa\rho + \rho\kappa) + x^2\rho^2;$$

adding therefore  $x^2$  to both members of (15.), dividing by  $-x$ , and then eliminating  $x$  by (13.), which is done by merely changing  $x\rho + x\rho^2$  to  $\sigma\rho$ , we find the equation

$$\sigma\rho + \rho x = 0; \quad . \quad . \quad . \quad . \quad . \quad . \quad (16.)$$

and finally

$$\sigma = -\rho x \rho^{-1}; \quad . \quad . \quad . \quad . \quad . \quad : \quad (17.)$$

so that the expression already assigned for the vector from  $c$  to  $d$ , presents itself as the result of this analysis. And in fact the tensor of this expression (17.) is equal to  $Tx$ , by the general rule for the tensor of a product, or because  $(-\rho x \rho^{-1})^2 = \rho x \rho^{-1} \rho x \rho^{-1} = \rho x^2 \rho^{-1} = x^2$ , since  $x^2$  is a (negative) scalar; while the product  $(\sigma - x)\rho$ , being  $-(x\rho + \rho x)$ , is equal, by article 20, to an expression of scalar form.

42. Conversely if, in any investigation conducted on the present principles, we meet with the expression  $-\rho x \rho^{-1}$ , we may perceive in the way just now mentioned, that it denotes a vector of which the square is equal to that of  $x$ ; and that, if  $x$  be subtracted from it, the remainder gives a scalar product when it is multiplied into  $\rho$ : so that, if we denote this expression by  $\sigma$ , or establish the equation (17.), the equations (11.) and (12.) will then be satisfied, and the vector  $\sigma$  will have the same length as  $x$ , while the directions of  $\sigma - x$  and  $\rho$  will be either exactly similar or exactly opposite to each other. We may therefore be thus led to regard, subject to this condition (17.) or (16.), the two vector-symbols  $\sigma$  and  $x$  as denoting, in length and in direction, two radii of one common sphere, such that the chord-line  $\sigma - x$  connecting their extremities has the direction of the line  $\rho$ , or of that line reversed. Hence also, by the elementary property of a plane isosceles triangle, we may see that, under the same condition, the inclination of  $\sigma$  to  $\rho$  is equal to the inclination of  $x$  to  $-\rho$ , or of  $-x$  to  $\rho$ ; in such a manner that the bisector of the external vertical angle of the isosceles triangle, or the bisector of the angle at the centre of the sphere between the two radii  $\sigma$  and  $-x$ , is a new radius parallel to  $\rho$ , because it is parallel to the base of the triangle ( $ACD$ ), or to the chord ( $AD$ ) just now mentioned. And by conceiving a diameter of the sphere parallel to this chord, or to  $\rho$ , and supposing  $-x$  to denote that reversed radius which coincides in situation with the radius  $x$ , but is drawn from the surface to the centre (that is, in the recent construction, from  $\lambda$  to  $c$ ), while  $\sigma$  is still drawn from centre to surface (from  $c$  to  $d$ ), we may be led to regard  $\sigma$ , or  $-\rho x \rho^{-1}$ , as the *reflexion* of  $-x$  with respect to the diameter parallel to  $\rho$ , or simply with respect to  $\rho$  itself, as was remarked in the 38th article; since the vector-symbols  $\rho$ ,  $\sigma$ , &c. are supposed,

in these calculations, to indicate indeed the *lengths and directions, but not the situations*, of the straight lines which they are employed to denote.

43. The same geometrical interpretation of the symbol  $-\rho x \rho^{-1}$  may be obtained in several other ways, among which we shall specify the following. Whatever the lengths and directions of the two straight lines denoted by  $\rho$  and  $x$  may be, we may always conceive that the latter line, regarded as a vector, is or may be decomposed, by two different projections, into two partial or component vectors,  $x'$  and  $x''$ , of which one is parallel and the other is perpendicular to  $\rho$ ; so that they satisfy respectively the equations of parallelism and perpendicularity (see article 21), and that we have consequently,

$$x = x' + x''; \quad V \cdot x' \rho = 0; \quad S \cdot x'' \rho = 0; \quad . \quad . \quad (18.)$$

where  $S$  is the characteristic of the operation of *taking the scalar* of a quaternion. The equation of parallelism gives  $\rho x' = x' \rho$ , and the equation of perpendicularity gives  $\rho x'' = -x'' \rho$ ; hence the proposed expression  $-\rho x \rho^{-1}$  resolves itself into the two parts,

$$\left. \begin{aligned} -\rho x' \rho^{-1} &= -x' \rho \rho^{-1} = -x'; \\ -\rho x'' \rho^{-1} &= +x'' \rho \rho^{-1} = +x''; \end{aligned} \right\} . \quad . \quad . \quad (19.)$$

so that we have, upon the whole,

$$-\rho x \rho^{-1} = -\rho (x' + x'') \rho^{-1} = -x' + x''. \quad . \quad . \quad (20.)$$

The part  $-x'$  of this last expression, which is parallel to  $\rho$ , is the same as the corresponding part of  $-x$ ; but the part  $+x''$ , perpendicular to  $\rho$ , is the same with the corresponding part of  $+x$ , or is opposite to the corresponding part of  $-x$ ; we may therefore be led by this process also to regard the expression (17.) as denoting the reflexion of the vector  $-x$ , with respect to the vector  $\rho$ , regarded as a reflecting line; and we see that the direction of  $\rho$ , or that of  $-\rho$ , is exactly intermediate between the two directions of  $-x$  and  $-\rho x \rho^{-1}$ , or between those of  $x$  and of  $\rho x \rho^{-1}$ .

44. The equation (9.) of the ellipsoid, in article 38, or the equation (4.) in article 37, may be more fully written thus:

$$(\rho + x)(\rho + x\rho) = (x^2 - 1)^2. \quad . \quad . \quad . \quad (21.)$$

And to express that we propose to cut this surface by any diametral plane, we may write the equation

$$\varpi \rho + \rho \varpi = 0, \quad . \quad . \quad . \quad . \quad (22.)$$

where  $\varpi$  denotes a vector to which that cutting plane is perpendicular: thus, if in particular, we change  $\varpi$  to  $x$ , we find, for the corresponding plane through the centre, the equation

$$xp + \rho x = 0, \quad . \quad . \quad . \quad . \quad . \quad (23.)$$

which, when combined with (21.), gives

$$(x^2 - i^2)^2 = (1 - x)\rho \cdot \rho(1 - x) = (1 - x)\rho^2(1 - x) = (1 - x)^2\rho^2,$$

that is,

$$\rho^2 = \left( \frac{x^2 - i^2}{1 - x} \right)^2; \quad . \quad . \quad . \quad . \quad . \quad (24.)$$

but this is the equation of a sphere concentric with the ellipsoid; therefore the diametral plane (23.) cuts the ellipsoid in a *circle*, or the plane itself is a *cyclic plane*. We see also that the vector  $x$ , as being perpendicular to this plane (23.), is one of the *cyclic normals*, or normals to planes of circular section; which agrees with the construction, since we saw, in article 36, that the auxiliary or diacentric sphere, with centre  $c$ , touches one cyclic plane at the centre  $a$  of the ellipsoid. The same construction shows that the other cyclic plane ought to be perpendicular to the vector  $i$ ; and accordingly the equation

$$ip + \rho i = 0 \quad . \quad . \quad . \quad . \quad . \quad (25.)$$

represents this second cyclic plane; for, when combined with the equation (21.) of the ellipsoid, it gives

$$(x^2 - i^2)^2 = \rho(x - i) \cdot (x - i)\rho = \rho(x - i)^2\rho = (x - i)^2\rho^2,$$

and therefore conducts to the same equation (24.) of a concentric sphere as before; which sphere (24.) is thus seen to contain the intersection of the ellipsoid (21.) with the plane (25.), as well as that with the plane (23.). If we use the form (9.), we have only to observe that whether we change  $\rho x$  to  $-x\rho$ , or  $ip$  to  $-\rho i$ , we are conducted in each case to the following expression for the length of the radius vector of the ellipsoid, which agrees with the equation (24.):

$$T\rho = \frac{x^2 - i^2}{T(1 - x)}. \quad . \quad . \quad . \quad . \quad . \quad (26.)$$

And because  $x^2 - i^2$  denotes the square upon the tangent drawn to the auxiliary sphere from the external point  $b$ , while  $T(1 - x)$  denotes the length of the side  $BA$  of the generating triangle, we see by this easy calculation with quaternions, as well as by the more purely geometrical reasoning which was alluded to, and partly stated, in the 36th article, that the common radius of the two diametral and circular sections of the ellipsoid is equal to the straight line which was there called  $BG$ , and which had the direction of  $BA$ , while terminating, like it, on the surface of the auxiliary sphere; so that the two last lines  $BA$ , and  $BG$ , were connected with that sphere and with each other, in this or in the opposite order, as the whole se-



cant and the external part. In fact, as the point  $D$ , in the construction approaches, in any direction, on the surface of the auxiliary sphere, to  $A$ , the point  $D'$  approaches to  $G$ ; and  $BD'$ , and therefore also  $AE$ , tends to become equal in length to  $BG$ ; while the direction of  $AE$ , being the same with that of  $AD$ , or opposite thereto, tends to become tangential to the sphere, or perpendicular to  $AC$ : the line  $BG$  is therefore equal to the radius of that diametral and circular section of the ellipsoid which is made by the plane that touches the auxiliary sphere at  $A$ . And again, if we conceive the point  $D'$  to revolve on the surface of the sphere from  $G$  to  $G$  again, in a plane perpendicular to  $BC$ , then the lines  $AD$  and  $AE$  will revolve together in another plane parallel to that last mentioned, and perpendicular likewise to  $BC$ ; while the length of  $AE$  will be still equal to the same constant line  $BG$  as before: which line is therefore found to be equal to the common radius of both the diametral and circular sections of the ellipsoid, whether as determined by the geometrical construction which the calculus of quaternions suggested, or immediately by that calculus itself.

45. We may write the equation (21.) of the ellipsoid as follows:

$$f(\rho) = 1, \quad . \quad . \quad . \quad . \quad . \quad (27.)$$

if we introduce a scalar function  $f$  of the variable vector  $\rho$ , defined as follows:

$$(\kappa^2 - \iota^2)^2 f(\rho) = (\iota\rho + \rho\kappa)(\rho\iota + \kappa\rho) = \iota\rho^2\iota + \iota\rho\kappa\rho + \rho\kappa\rho\iota + \rho\kappa^2\rho;$$

or thus, in virtue of article 20,

$$(\kappa^2 - \iota^2)^2 f(\rho) = (\iota^2 + \kappa^2)\rho^2 + 2S.\iota\rho\kappa\rho. \quad . \quad . \quad (28.)$$

Let  $\rho + \tau$  denote another vector from the centre to the surface of the same ellipsoid; we shall have, in like manner,

$$f(\rho + \tau) = 1, \quad . \quad . \quad . \quad . \quad . \quad (29.)$$

where

$$f(\rho + \tau) = f(\rho) + 2S.v\tau + f(\tau), \quad . \quad . \quad . \quad (30.)$$

if we introduce a new vector symbol  $v$ , defined by the equation

$$(\kappa^2 - \iota^2)^2 v = (\iota^2 + \kappa^2)\rho + \iota\rho\kappa + \kappa\rho\iota; \quad . \quad . \quad . \quad (31.)$$

because generally, for any two vectors  $\rho$  and  $\tau$ ,

$$(\rho + \tau)^2 = \rho^2 + 2S.\rho\tau + \tau^2, \quad . \quad . \quad . \quad (32.)$$

and, for any four vectors,  $\iota, \kappa, \rho, \tau$ ,

$$S.\iota\tau\kappa\rho = S.\tau\kappa\rho\iota = S.\kappa\rho\iota\tau = S.\rho\iota\tau\kappa; \quad . \quad . \quad (33.)$$

which last principle, respecting certain transpositions of vector symbols, as factors of a product under the sign  $S.$ , shows,

when combined with the equations (27.), (28.), and (31.), that we have also this simple relation :

$$S.\nu\rho=1. \quad . \quad . \quad . \quad . \quad . \quad (34.)$$

Subtracting (27.) from (29.), attending to (30.), changing  $\tau$  to  $T\tau$ .  $U\tau$ , where  $U$  is, as in article 19, the characteristic of the operation of *taking the versor* of a quaternion (or of a vector), and dividing by  $T\tau$ , we find :

$$0=\frac{f(\rho+\tau)-f(\rho)}{T\tau}=2S.\nu U\tau+T\tau.f(U\tau). \quad (35.)$$

This is a rigorous equation, connecting the *length* or the *tensor*  $T\tau$ , of any chord  $\tau$  of the ellipsoid, drawn from the extremity of the semidiameter  $\rho$ , with the *direction* of that chord  $\tau$ , or with the *versor*  $U\tau$ ; it is therefore only a new form of the equation of the ellipsoid itself, with the origin of vectors removed from the centre to a point upon the surface. If we now conceive the chord  $\tau$  to diminish in length, the term  $T\tau.f(U\tau)$  of the right-hand member of this equation (35.) tends to become  $=0$ , on account of the factor  $T\tau$ ; and therefore the other term  $2S.\nu U\tau$  of the same member must tend to the same limit zero. In this way we arrive easily at an equation expressing the *ultimate law of the directions of the evanescent chords* of the ellipsoid, at the extremity of any given or assumed semidiameter  $\rho$ ; which equation is  $0=2S.\nu U\tau$ , or simply,

$$0=S.\nu\tau, \quad . \quad . \quad . \quad . \quad . \quad (36.)$$

if  $\tau$  be a tangential vector. The vector  $\nu$  is therefore perpendicular to all such tangents, or infinitesimal chords of the ellipsoid, at the extremity of the semidiameter  $\rho$ ; and consequently it has the direction of the *normal* to that surface, at the extremity of that semidiameter. The *tangent plane* to the same surface at the same point is represented by the equation (34.), if we treat, therein, the normal vector  $\nu$  as constant, and if we regard the symbol  $\rho$  as denoting, in the same equation (34.), a variable vector, drawn from the centre of the ellipsoid to any point upon that tangent plane. This equation (34.) of the tangent plane may be written as follows:

$$S.\nu(\rho-\nu^{-1})=0; \quad . \quad . \quad . \quad . \quad . \quad (37.)$$

and under this form it shows easily that the symbol  $\nu^{-1}$  represents, in length and in direction, the perpendicular let fall from the origin of the vectors  $\rho$ , that is from the centre of the ellipsoid, upon the plane which is thus represented by the equation (34.) or (37.); so that the vector  $\nu$  itself, as determined by the equation (31.), may be called the *vector of proxi-*

mity\* of the tangent plane of the ellipsoid, or of an element of that surface, to the centre, at the end of that semidiameter  $\rho$  from which  $\nu$  is deduced by that equation.

46. Conceive now that at the extremity of an infinitesimal chord  $d\rho$  or  $\tau$ , we draw another normal to the ellipsoid; the expression for any arbitrary point on the former normal, that is the symbol for the vector of this point, drawn from the centre of the ellipsoid, or from the origin of the vectors  $\rho$ , is of the form  $\rho + n\nu$ , where  $n$  is an arbitrary scalar; and in like manner the corresponding expression for an arbitrary point on the latter and infinitely near normal, or for its vector from the same centre of the ellipsoid, is  $\rho + d\rho + (n + dn)(\nu + d\nu)$ , where  $dn$  is an arbitrary but infinitesimal scalar, and  $d\nu$  is the differential of the vector of proximity  $\nu$ , which may be found as a function of the differential  $d\rho$  by differentiating the equation (31.), which connects the two vectors  $\nu$  and  $\rho$  themselves. In this manner we find, from (31.),

$$(\kappa^2 - \iota^2)^2 d\nu = (\iota^2 + \kappa^2) d\rho + \iota d\rho \kappa + \kappa d\rho \iota; \quad . \quad . \quad (38.)$$

and the condition required for the intersection of the two near normals, or for the existence of a point common to both, is expressed by the formula

$$\rho + d\rho + (n + dn)(\nu + d\nu) = \rho + n\nu; \quad . \quad . \quad . \quad (39.)$$

which may be more concisely written as follows:

$$d\rho + d \cdot n\nu = 0; \quad . \quad . \quad . \quad . \quad (40.)$$

or thus:

$$d\rho + nd\nu + dn\nu = 0. \quad . \quad . \quad . \quad . \quad (41.)$$

We can eliminate the two scalar coefficients,  $n$  and  $dn$ , from this last equation, according to the rules of the calculus of quaternions, by the method exemplified in the 24th article of this paper (Phil. Mag., August 1846), or by operating with the characteristic  $S \cdot \nu d\nu$ , because generally

$$S \cdot \nu \mu^2 = 0, \quad S \cdot \nu \mu \nu = 0,$$

whatever vectors  $\mu$  and  $\nu$  may be; so that here,

$$S \cdot \nu d\nu nd\nu = 0, \quad S \cdot \nu d\nu dn\nu = 0.$$

\* This name, "vector of proximity," was suggested to the writer by a phraseology of Sir John Herschel's; and the equation (31.), of article 45, which determines this vector for the ellipsoid, was one of a few equations which were designed to have been exhibited to the British Association at its meeting in 1846: but were accidentally forwarded at the last moment to Collingwood, instead of Southampton, and did not come to the hands of the eminent philosopher just mentioned, until it was too late for him to do more than return the paper, with some of those encouraging expressions by which he delights to cheer, as opportunities present themselves, all persons whom he conceives to be labouring usefully for science.

In this manner we find from (41.) the following very simple formula:

$$S. \nu d\nu d\rho = 0; \quad . \quad . \quad . \quad . \quad . \quad (42.)$$

which is easily seen, on the same principles, to hold good, as the *quaternion form of the differential equation of the lines of curvature on a curved surface generally*, if  $\nu$  be still the *vector of proximity of the superficial element* of the curved surface to the origin of the vectors  $\rho$ , which vector  $\nu$  is determined by the general condition  $S. \nu d\rho = 0$ ,  $. \quad . \quad . \quad . \quad . \quad (43.)$

combined with the equation already written,

$$S. \nu \rho = 1 \quad (34.);$$

or simply if  $\nu$  be a *normal vector*, satisfying the condition (43.) alone. Substituting, therefore, in the case of the ellipsoid, the expression for  $d\nu$  given by (38.), and observing that  $S. \nu d\rho^2 = 0$ , we find that we may write the equation of the lines of curvature for this particular surface as follows:

$$S. \nu (i d\rho x + x d\rho i) d\rho = 0; \quad . \quad . \quad . \quad . \quad (44.)$$

which equation, when treated by the rules of the present calculus, admits of being in many ways symbolically transformed, and may also, with little difficulty, be translated into geometrical enunciations.

47. Thus if we observe that, by article 20,  $i\tau x - x\tau i$  is a *scalar form*, whatever three vectors may be denoted by  $i, x, \tau$ ; and if we attend to the equation (43.), which expresses that the normal  $\nu$  is perpendicular to the linear element, or infinitesimal chord,  $d\rho$ ; we shall perceive that, for *every* direction of that element, the following equation holds good:

$$S. \nu (i d\rho x - x d\rho i) d\rho = 0. \quad . \quad . \quad . \quad . \quad (45.)$$

We have therefore, from (44.), for those *particular* directions which belong to the lines of curvature, this simplified equation;

$$S. \nu i d\rho x d\rho = 0; \quad . \quad . \quad . \quad . \quad (46.)$$

which may be still a little abridged, by writing instead of  $d\rho$  the symbol  $\tau$  of a tangential vector, already used in (36.); for thus we obtain the formula:

$$S. \nu i \tau x \tau = 0. \quad . \quad . \quad . \quad . \quad . \quad (47.)$$

We might also have observed that by the same article 20 (Phil. Mag., July 1846),  $i\tau x + x\tau i$  and therefore  $i d\rho x + x d\rho i$  is a *vector form*, and that by article 26 (Phil. Mag., August 1846), three vector-factors under the characteristic  $S$  may be in any manner transposed, with only a change (at most) in the positive or negative sign of the resulting scalar; from which it would have followed, by a process exactly similar to the foregoing, that the equation (44.) of the lines of curvature on an ellipsoid may be thus written,

$$S. \nu d\rho i d\rho x = 0; \quad . \quad . \quad . \quad . \quad . \quad (48.)$$



or, substituting for the linear element  $d\rho$  the tangential vector  $\tau$ ,

$$S.\nu\tau\iota x=0; \quad . \quad . \quad . \quad . \quad . \quad (49.)$$

or finally, by the principles of the same 20th article,

$$\nu\tau\iota x - x\tau\iota\nu = 0. \quad . \quad . \quad . \quad . \quad . \quad (50.)$$

48. Under this last form, it was one of a few equations selected in September 1846, for the purpose of being exhibited to the Mathematical Section of the British Association at Southampton; although it happened\* that the paper containing those equations did not reach its destination in time to be so exhibited. The equations here marked (49.) and (50.) were however published before the close of the year in which that meeting was held, as part of the abstract of a communication which had been made to the Royal Irish Academy in the summer of that year. (See the Proceedings of the Academy for July 1846, equations (46.) and (47.).) From the somewhat discursive character of the present series of communications on Quaternions, and from the desire which the author feels to render them, to some extent, complete within themselves, or at least intelligible to those mathematical readers of the Philosophical Magazine who may be disposed to favour him with their attention, to the degree which the novelty of the conceptions and method may require, without its being *necessary* for such readers to refer to other publications of his own, he is induced, and believes himself to be authorized, to copy here a few other equations from that short and hitherto unpublished Southampton paper, and to annex to them another formula which may be found in the Proceedings, already cited, of the Royal Irish Academy: together with a more extensive formula, which he believes to be new.

49. Besides the equation of the ellipsoid,

$$(1\rho + \rho x)(\rho 1 + x\rho) = (x^2 - 1^2)^2 \text{ (21.), art. 44;}$$

with the expression derived from it, for the vector of proximity of that surface to its centre,

$$(x^2 - 1^2)^2\nu = (1^2 + x^2)\rho + 1\rho x + x\rho 1 \text{ (31.), art. 45;}$$

the equation for the lines of curvature on the ellipsoid,

$$\nu\tau\iota x - x\tau\iota\nu = 0 \text{ (50.), art. 47;}$$

and the equation  $\nu\tau + \tau\nu = 0$ ,  $. \quad . \quad . \quad . \quad . \quad (51.)$

which is a form of the relation  $S.\nu\tau = 0$ , that is of the equation (36.), article 45, of the present series of communications; the author gave, in the paper which has been above referred to, the following symbolic transformation, for the well-known characteristic of operation,

$$\left(\frac{d}{dx}\right)^2 + \left(\frac{d}{dy}\right)^2 + \left(\frac{d}{dz}\right)^2,$$

\* See the note to article 45.

which seems to him to open a wide and new field of analytical research, connected with many important and difficult departments of the mathematical study of nature.

A QUATERNION, *symbolically considered*, being (according to the views originally proposed by the author in 1843) an algebraical quadrinomial of the form  $w + ix + jy + kz$ , where  $wxyz$  are any four real numbers (positive or negative or zero), while  $ijk$  are three co-ordinate imaginary units, subject to the fundamental laws of combination (see Phil. Mag. for July 1844):

$$\left. \begin{aligned} i^2 = j^2 = k^2 &= -1; \\ ij &= k; jk = i; ki = j; \\ ji &= -k; kj = -i; ik = -j; \end{aligned} \right\} \dots \dots \dots (a.)$$

it results at once from these definitions, or laws of symbolic combination, (a.), that if we introduce a new characteristic of operation,  $\nabla$ , defined with relation to these three symbols  $ijk$ , and to the known operation of partial differentiation, performed with respect to three independent but real variables  $xyz$ , as follows:

$$\nabla = \frac{id}{dx} + \frac{j d}{dy} + \frac{k d}{dz}; \dots \dots \dots (b.)$$

this new characteristic  $\nabla$  will have the negative of its symbolic square expressed by the following formula:

$$-\nabla^2 = \left(\frac{d}{dx}\right)^2 + \left(\frac{d}{dy}\right)^2 + \left(\frac{d}{dz}\right)^2; \dots \dots (c.)$$

of which it is clear that the applications to analytical physics must be extensive in a high degree. In the paper\* designed for Southampton it was remarked, as an illustration, that this result enables us to put the known thermological equation,

$$\frac{d^2 v}{dx^2} + \frac{d^2 v}{dy^2} + \frac{d^2 v}{dz^2} + a \frac{dv}{dt} = 0,$$

under the new and more symbolic form,

$$\left(\nabla^2 - \frac{ad}{dt}\right)v = 0; \dots \dots \dots (d.)$$

while  $\nabla v$  denotes, in quantity and in direction, the *flux* of heat, at the time  $t$  and at the point  $xyz$ .

50. In the Proceedings of the Royal Irish Academy for July 1846, it will be found to have been noticed that the same new characteristic  $\nabla$  gives also this other general transformation, perhaps not less remarkable, nor having less extensive

\* In that paper itself, the characteristic was written  $\nabla$ ; but this more common sign has been so often used with other meanings, that it seems desirable to abstain from appropriating it to the new signification here proposed.

consequences, and which presents itself under the form of a quaternion :

$$\left. \begin{aligned} \Delta(it + ju + kv) &= - \left( \frac{dt}{dx} + \frac{du}{dy} + \frac{dv}{dz} \right) \\ &+ i \left( \frac{dv}{dy} - \frac{du}{dz} \right) + j \left( \frac{dt}{dz} - \frac{dv}{dx} \right) + k \left( \frac{du}{dx} - \frac{dt}{dy} \right) \end{aligned} \right\} \dots (e.)$$

In fact the equations (a.) give generally (see art. 21 of the present series),

$$\left. \begin{aligned} (ix + jy + kz)(it + ju + kv) &= -(xt + yu + zv) \\ &+ i(yv - zu) + j(zt - xv) + k(xu - yt), \end{aligned} \right\} \dots (f.)$$

if  $xyztuv$  denote any six real numbers; and the calculations by which this is proved, show, still more generally, that the same transformation must hold good, if each of the three symbols  $i, j, k$ , subject still to the equations (a.), be commutative in arrangement, as a symbolic factor, with each of the three other symbols  $x, y, z$ ; even though the latter symbols, like the former, should not be commutative in that way among themselves; and even if they should denote symbolical instead of numerical multipliers, possessing still the distributive character. We may therefore change the three symbols  $x, y, z$ , respectively, to the three characteristics of partial differentiation,  $\frac{d}{dx}, \frac{d}{dy}, \frac{d}{dz}$ ; and thus the formula (e.) is seen to be included in the formula (f.). And if we then, in like manner, change the three symbols  $t, u, v$ , regarded as factors, to  $\frac{d}{dx'}, \frac{d}{dy'}, \frac{d}{dz'}$ , that is, to the characteristics of three partial differentiations performed with respect to three new and independent variables  $x', y', z'$ , we shall thereby change  $\frac{dt}{dx}$  to  $\frac{d}{dx} \frac{d}{dx'}$ , and so obtain the formula :

$$\left. \begin{aligned} &\left( i \frac{d}{dx} + j \frac{d}{dy} + k \frac{d}{dz} \right) \left( i \frac{d}{dx'} + j \frac{d}{dy'} + k \frac{d}{dz'} \right) \\ &= - \left( \frac{d}{dx} \frac{d}{dx'} + \frac{d}{dy} \frac{d}{dy'} + \frac{d}{dz} \frac{d}{dz'} \right) \\ &+ i \left( \frac{d}{dy} \frac{d}{dz'} - \frac{d}{dz} \frac{d}{dy'} \right) + j \left( \frac{d}{dz} \frac{d}{dx'} - \frac{d}{dx} \frac{d}{dz'} \right) \\ &+ k \left( \frac{d}{dx} \frac{d}{dy'} - \frac{d}{dy} \frac{d}{dx'} \right); \end{aligned} \right\} \dots (g.)$$

which includes the formula (c.), and is now for the first time published.

This formula (g.) is, however, seen to be a very easy and immediate consequence from the author's fundamental equations of 1843, or from the relations (a.) of the foregoing article, which admit of being concisely summed up in the following continued equation:

$$i^2 = j^2 = k^2 = ijk = -1. \quad . \quad . \quad . \quad . \quad . \quad (h.)$$

The geometrical interpretation of the equation  $S. \nabla \tau i x \tau = 0$  of the lines of curvature on the ellipsoid, with some other applications of quaternions to that important surface, must be reserved for future articles of the present series, of which some will probably appear in an early number of this Magazine.

[To be continued.]

**XLVII. On the Equation in Numbers  $Ax^3 + By^3 + Cz^3 = Dxyz$ , and its associate system of Equations. By J. J. SYLVESTER, Esq., M.A., F.R.S.\***

[Continued from p. 191.]

**I**N the last Number of this Magazine I gave an account of a remarkable transformation to which the equation

$$Ax^3 + By^3 + Cz^3 = Dxyz$$

is subject when certain conditions between the coefficients A, B, C, D are satisfied; which conditions I shall begin by expressing with more generality and precision than I was enabled to do in my former communication.

1. Two of the quantities A, B, C are to be to one another in the ratio of two cubes.

2.  $27ABC - D^3$  must contain no positive prime factor whatever of the form  $6n + 1$ . I erred in my former communication in not excluding cubic factors of this form.

3. If  $2^m$  is the highest power of 2 which enters into ABC, and  $2^n$  the highest power of 2 which enters into D, then either  $m$  must be of the form  $3n \pm 1$ , or if not, then  $m$  must be greater than  $3n$ .

These three conditions being satisfied, the given equation can always be transformed into another,

where 
$$A'u^3 + B'v^3 + C'w^3 = D'uvw,$$
  

$$A'B'C' = ABC \quad D' = D \quad uvw = \text{a factor of } z.$$

The consequence of this is, as stated in my former paper, that wherever A, B, C, D, besides satisfying the conditions above stated, are taken so as likewise to satisfy the condition,—1°, of ABC being equal to  $2^{3m \pm 1}$ , or 2°, of ABC being equal to  $2^{3m \pm 1} \cdot p^{3n \pm 1}$ , provided in the first case that ABC is also of the

\* Communicated by the Author.



form  $9n \pm 1$ , and in the second case  $ABC$  again of the same form  $9m \pm 1$ , but likewise  $D$  divisible by 9,  $p$  being in both cases a prime, then the given equation will be *generally* insoluble. And I am now enabled to add that the *only* solution of which it will in any case admit, is the solitary one found by making two of the terms  $Ax^3$ ,  $By^3$ ,  $Cz^3$  equal to one another; so that, for instance, if the given equation should be of the form

$$x^3 + y^3 + ABC \cdot z^3 = Dxyz,$$

then the above conditions being satisfied, the one solitary solution of which the equation can possibly admit, is  $x=1$   $y=1$ ,

$$Az^3 - Dz + 2 = 0,$$

which may or may not have possible roots. I call this a *solitary* or singular solution, because it exists alone and no other solution can be deduced from it; whereas in general I shall show that any one solution of the equation

$$Ax^3 + By^3 + Cz^3 = Dxyz$$

can be made to furnish an infinity of other solutions independent of the one supposed given, *i. e.* not reducible thereto by expelling a common factor from the new system of values of  $x, y, z$  deduced from the given system.

The following is the Theorem of Derivation in question:

Let

$$A\alpha^3 + B\beta^3 + C\gamma^3 = D\alpha\beta\gamma.$$

Then if we write

$$F = A\alpha^3 \quad G = B\beta^3 \quad H = C\gamma^3,$$

and make

$$x = F^2G + G^2H + H^2F - 3FGH$$

$$y = FG^2 + GH^2 + HF^2 - 3FGH$$

$$z = \frac{1}{D} \{F^3 + G^3 + H^3 - 3FGH\},$$

or

$$= \alpha\beta\gamma \{F^2 + G^2 + H^2 - FG - FH - GH\},$$

we shall have

$$x^3 + y^3 + ABCz^3 = Dxyz.$$

I am hence enabled to show that whenever  $x^3 + y^3 + Az^3 = Dxyz$  is insoluble, there will be a whole family of allied equations equally insoluble. For instance, because  $x^3 + y^3 + z^3 = 0$  is insoluble in integer numbers. I know likewise that

$$x^6 + y^6 + z^6 = x^3y^3 + x^3z^3 + y^3z^3$$

$$x^6 + y^6 + z^6 = x^3y^3 + x^3z^3 - 2y^3z^3$$

are each equally insoluble.

In fact

$$\begin{aligned} & (x^3 + y^3 + z^3) \times (x^6 + y^6 + z^6 - x^3y^3 - x^3z^3 - y^3z^3) \\ & \times (x^6 + y^6 + z^6 - x^3y^3 - x^3z^3 + 2y^3z^3) \\ & \times (x^6 + y^6 + z^6 - y^3z^3 - y^3x^3 + 2x^3z^3) \\ & \times (x^6 + y^6 + z^6 - x^3z^3 - z^3y^3 + 2y^3x^3) \\ & = u^3 + v^3 + w^3, \end{aligned}$$

where  $u, v, w$  are rational integral functions of  $x, y, z$ .

Hence each of the factors must be incapable of becoming zero\*.

As a particular instance of my general theory of transformation and elevation, take the equation

$$x^3 + y^3 + 2z^3 = Mxyz.$$

Then, with the exception of the singular or solitary solution  $x=1, y=1$ , of which I take no account, I am able to affirm that for all values of  $M$  between 7 and  $-6$ , both inclusive, with the exception of  $M=-2$ , the equation is insoluble in integer numbers.

Take now the equation where  $M=-2$ , viz.

$$x^3 + y^3 + 2z^3 + 2xyz = 0.$$

One particular solution of this is

$$x=1 \quad y=-1 \quad z=1.$$

Another, which I shall call the second †, is

$$x=1 \quad y=3 \quad z=-2.$$

From the first solution I can deduce in succession the following:

$$\begin{array}{lll} x=11 & y=5 & z=-7 \\ x=-793269121 & y=117949000 & z=-1189735855 \\ & \&c. & \&c. & \&c. \end{array}$$

From the second,

$$\begin{array}{lll} x=-10085 & y=8921 & z=-8442 \\ x=\&c. & y=\&c. & z=\&c. \end{array}$$

As another example, take the equation

$$x^3 + y^3 + 6z^3 = 6xyz.$$

One solution of the transformed equation

$$u^3 + 2v^3 + 3w^3 = 6uvw$$

is evidently

$$u=1 \quad v=1 \quad w=1.$$

\* It is however sufficiently evident from their intrinsic form, which may be reduced to  $\frac{1}{4}(M^2 + 3N^2)$ , that this impossibility exists for all the factors except the first.

† See Postscript.

Hence I can deduce an infinite series of solutions of the given equation, of which the first in order of ascent will be

$$x=5 \quad y=7 \quad z=3.$$

Again, the lowest possible solution in integers of the equation

$$x^3 + y^3 + 6z^3 = 0$$

will be

$$x=17 \quad y=37 \quad z=-21.$$

The equation

$$x^3 + y^3 + 9z^3 = 0$$

admits of the solutions

$$x=1 \quad y=2 \quad z=-1$$

$$x=-271 \quad y=919 \quad z=-438.$$

I trust that my readers will do me the justice to believe that I am in possession of a strict demonstration of all that has been here advanced without proof. Certain of the writer's friends on the continent have, in their comments upon one of his former papers which appeared in this Magazine, complimented his powers of divination at the expense of his judgement, in rather gratuitously assuming that the author of the *Theory of Elimination* was unprovided with the demonstrations, which he was too inert or too beset with worldly cares and distractions to present to the public in a sufficiently digested form. The proof of whatever has been here advanced exists not merely as a conception of the author's mind, but fairly drawn out in writing, and in a form fit for publication.

P.S. It must not be supposed that the two primary or basic solutions above given of the equation

$$x^3 + y^3 + 2z^3 + 2xyz = 0,$$

viz.

$$x=1 \quad y=-1 \quad z=1$$

$$x=1 \quad y=3 \quad z=-2$$

are independent of one another. The second may be derived from the first, as I shall show in a future communication. In fact there exist *three* independent processes, by combining which together, one particular solution may be made to give rise to an infinite series of infinite series of infinite series of correlated solutions, which it may possibly be discovered contain between them the *general* complete solution of the equation

$$x^3 + y^3 + Az^3 = Dxyz.$$

J. J. S.

26 Lincoln's Inn Fields,  
Sept. 20, 1847.

[To be continued.]

XLVIII. *On the Invention and First Introduction of Mr. Kœnig's Printing Machine.* By RICHARD TAYLOR, F.S.A.&c.

"As a step in the progress of civilization the Steam Press can only be compared to the original discovery of Printing itself."—*Times Newspaper*, July 29, 1847, on the death of Mr. J. Walter.

**M**ORE than a century after its introduction the first invention of the Art of Printing became a subject of long-continued controversy, remarkable for the insufficiency and fallacy of the most confident assertions resting upon pretended traditions and unsupported conjectures. And, as Hadrian Junius in 1575 first disputed the claims of Gutemberg after so long a period had elapsed, so did Atkyns as late as 1664 first deny the title of Caxton to the honour of having introduced the art into our own country. Hence one of the writers in this controversy remarks that "the Art of Printing, which has given light to most other things, hides its own head in darkness."

It will be our own fault if we allow any unfounded assertions and pretensions to obtain currency with regard to an improvement in the art, of which *The Times* newspaper has said that "from the days of Faust and Gutemberg to the present hour there has been only one great revolution in the art of printing, and it occurred in the year 1814. Of that revolution Mr. Walter was the prominent and leading agent."

Now though I would on no account detract from the general merits of the late Mr. Walter, as set forth in the Obituary and extended Memoir which appeared in *The Times* of the 29th of July and 16th of September, yet I cannot allow the representations which are made in these articles, as to any share which he is alleged to have had in this important invention, to pass without the most unqualified contradiction.

In the Obituary we read as follows :—

"But one achievement alone is sufficient to place Mr. Walter high in that list which the world, as it grows older and wiser, will more and more appreciate—

'Inventas aut qui vitam excoluere per artes,  
Quique sui memores alios fecere merendo.'

He first brought the steam-engine to the assistance of the public press. Familiar as the discovery is now, there was a time when it seemed fraught with difficulties as great as those which Fulton has overcome on one element and Stephenson on another. To take off 5000 impressions in an hour was once as ridiculous a conception as to paddle a ship fifteen miles against wind and tide, or to drag in that time a train of carriages weighing 100 tons fifty miles. Mr. Walter, who, without being a visionary, may be said to have thought nothing impossible that was useful and good, was early resolved that there should be no impossibility in printing by steam. It took a long time in those days to strike off the 3000 or 4000 copies of *The*



Times. Mr. Walter could not brook the tedium of the manual process. As early as the year 1804 an ingenious compositor, named Thomas Martyn, had invented a self-acting machine for working the press, and had produced a model which satisfied Mr. Walter of the feasibility of the scheme. Being assisted by Mr. Walter with the necessary funds, he made considerable progress towards the completion of his work."

"On the very eve of success he was doomed to bitter disappointment. He had exhausted his own funds in the attempt, and his father, who had hitherto assisted him, became disheartened, and refused him any further aid. The project was therefore for the time abandoned." [Why abandoned, we may ask, if so feasible, and on the very eve of success?]

"Mr. Walter, however, was not the man to be deterred from what he had once resolved to do. He gave his mind incessantly to the subject, and courted aid from all quarters, with his usual munificence. In the year 1814 he was induced by a clerical friend, in whose judgment he confided, to make a fresh experiment; and accordingly the machinery of the amiable and ingenious Koenig, assisted by his young friend Bauer, was introduced—not, indeed, at first, into *The Times* office, but into the adjoining premises, such caution being thought necessary from the threatened violence of the pressmen. Here the work advanced, under the frequent inspection and advice of the friend alluded to. At one period these two able mechanics suspended their anxious toil, and left the premises in disgust. After the lapse, however, of about three days, the same gentleman discovered their retreat\*, induced them to return, showed them to their surprise their difficulty conquered, and the work still in progress."

Who would not infer from the above, that Mr. Walter, having determined "to make a fresh experiment," in pursuance of those which he had long before abandoned (notwithstanding his early resolution that there should be no impossibility in it), and "courting aid from all quarters with his usual munificence," had been actually the person that enabled Mr. Koenig to pursue his labours on Mr. Walter's premises, "under the inspection and advice of Mr. Walter's clerical friend," and thus to produce his invention? Whereas, in truth, Mr. Walter knew nothing of Mr. Koenig till after his invention had been completed. He was merely the first newspaper proprietor who purchased from the Patentees the Printing Machines long before invented by Mr. Koenig. Of these patentees I was one, and as I am now the sole survivor, it devolves upon me to contradict any erroneous statements and unfounded pretensions. I feel this to be the more necessary, as already the misstatements of *The Times* are circulated, with additions and exaggerations, in other journals.

\* To me this story appears not a little extraordinary:—the "discovery of the retreat" of Messrs. K. and B.! who were every day to be found superintending our factory in Whitecross Street.—R. T.

Thus, in an article in the *Mechanics' Magazine* for Sept. 18, copied into the newspapers, I find the following passage :—

" No sooner were presses made of iron, than the idea occurred of working them by steam ; and the first to welcome the new and happy thought was the proprietor of a journal which stood in instant need of some such powerful auxiliary to enable him to keep pace with a circulation unexampled in the history of the press, and who, without it, would most assuredly never have been able to attain to that prodigious influence which for many years past has at once astonished and awed the world. Kœnig, the ingenious inventor of the steam-press\*, found in the proprietor of *The Times* his natural and best possible patron. *With the liberal aid of the late Mr. Walter, he produced a machine* of somewhat gigantic size, but nevertheless possessing a completeness of design and purpose which cast all other surface printing-presses into the shade."

And again—

" The steam-press has given occupation to many thousands, who, but for its introduction, would have been standing idle, and who ought, one and all, to bless the memory of Mr. Walter for *enabling the inventor to work out his ideas*, and perfect his great and glorious undertaking."

Now the whole of this is a fable. Mr. Walter was no "natural and best possible patron" of Mr. Kœnig's,—gave him no "liberal aid in producing his machine," nor did anything whatever to "enable him to work out his ideas." These had all been worked out long before ; patents had been taken out, a machine had been made, and was in operation on the premises of the Patentees, before ever Mr. Walter, or any other newspaper proprietor, was applied to and invited to adopt it. Mr. Perry of the *Morning Chronicle* declined, alleging that he did not consider a newspaper worth so many years' purchase as would equal the cost of machines. Mr. Walter, "being a cautious man of the world," but enterprising, "it being," as his biographer says, "his habit in the game of life never to throw away a chance," when he had fully satisfied himself by seeing that the invention was accomplished, and in effective operation, consented to give an order for two machines, for the cost of which he paid us a certain sum, and a rental according to the number of copies printed ; and this rent we received, until it was commuted for a sum agreed upon.

I do not mean to charge the writer in the *Mechanics' Magazine* with any intentional misrepresentation. He has evidently been misled by the articles in *The Times*, which though they do not *directly* assert all that he has inferred from them, yet they imply as much. Thus a story gains in the telling,

\* Mr. Kœnig's invention is very inappropriately designated by the terms "steam-press," and "the working of iron presses by *steam*." Its construction is wholly independent of the motive power employed.

till the most vague and unfounded suggestions, if uncontradicted, are assumed as indisputable facts; and it would be recorded that if Koenig was the Gutemberg of the new discovery, Walter was at least the Faust or Schœffer of the affair, or rather, both in one.

I am convinced that Mr. Walter, were he living, would disclaim the pretensions that have been made in his name: and indeed he has done so in the announcement which appeared in *The Times*, Nov. 20, 1814, the day on which that journal was first printed by the machines, and which contains the following passage:—

“That the completion of an invention of this kind, not the effect of chance, but the result of mechanical combinations methodically arranged in the mind of the artist, should be attended with many obstructions and much delay may be readily admitted. Our share in the event has indeed only been the application of the discovery, under an agreement with the patentees, to our own particular business.”

“The time for effecting the great revolution in the art of printing,” says Mr. Walter’s biographer, “did not arrive till the year 1814.” Now it was in 1809 that, together with the late Mr. George Woodfall, I joined Mr. Koenig and Mr. Bensley in taking out patents\*, the machine being even then so far advanced as to satisfy us as to the prospect of success, and to enable us to have the specifications drawn up. Koenig had gone on with Bensley, to whom I had recommended him some few years before, up to the year 1809, when the taking of premises and the purchase of lathes, tools, &c., and the employing of workmen, with the salaries of Mr. Koenig and his able and excellent assistant Mr. Bauer, led Bensley to invite us to a partnership in the undertaking. For several years it occupied much of our time and attention, and cost us much money (from which we had no return†) and much anxiety. Each experiment suggested some improvement, and one improvement led to others, so that additional patents had to be taken out. But with Mr. Walter we had none of us any communication, until, as I have before stated, the machine had been completed and was at work on our own premises.

I have thought it right, under the circumstances, to put on

\* One of the four patents bears date March 29, 1810 (See *Phil. Mag.* vol. xxxv. 1st Series, p. 319). It was taken out in the name of Frederick Koenig, and was assigned by articles of partnership to the firm of Bensley, Koenig, Woodfall and Taylor.

† Mr. Koenig left England, suddenly, in disgust at the treacherous conduct of Bensley, always shabby and overreaching, and whom he found to be laying a scheme for defrauding his partners in the patents of all the advantages to arise from them. Bensley, however, while he destroyed the prospects of his partners, outwitted himself, and grasping at all, lost all, becoming bankrupt in fortune as well as in character.

record my own recollections as to the progress and introduction of this invention: and though they relate to transactions which took place from thirty to forty years ago, I believe they are in the main correct, and can be confirmed by documentary evidence.

# XLIX. *Proceedings of Learned Societies.*

## CAMBRIDGE PHILOSOPHICAL SOCIETY.

[Continued from p. 143.]

**O**N the Partitions of Numbers, on Combinations, and on Permutations. By Henry Warburton, M.P., F.R.S., F.G.S., Member of the Senate of the University of London; formerly of Trinity College, A.M.

The use made by Waring of the Partitions of numbers in developing the power of a polynome, induced the author to seek for some general and ready method of determining in how many different ways a given number can be resolved into a given number of parts. On his communicating the method described in article 5 of Section I. of this abstract, to Professor De Morgan, in the autumn of 1846, that gentleman intimated a wish that the author would turn his attention also to Combinations; and such was the origin of the researches which form the subject of the 2nd and 3rd sections.

### I. *On the Partitions of Numbers.*

1. Let  $[N, p_\eta]$  denote how many different ways there are of resolving the integer  $N$  into  $p$  integral parts, none less than  $\eta$ . Then

$$[N, p_\eta] = [N \pm p\theta, p_{\eta \pm \theta}]. \quad \dots \quad (I.)$$

2. Such of the  $p$ -partitions of  $N$  as contain  $\eta$  as a part, and no part less than  $\eta$ , are obtained by resolving  $N - \eta$  into  $p - 1$  parts not less than  $\eta$ , and by adding  $\eta$ , as a  $p$ th part, to every such  $(p - 1)$ -partition. That is,

$$[N, p_\eta] - [N, p_{\eta+1}] = [N - \eta, p - 1]_\eta. \quad \dots \quad (II.)$$

3. In (II.), substitute  $\eta + 1$ ,  $\eta + 2$ , &c. successively for  $\eta$ . The sum of the results is

$$[N, p_\eta] - [N, p_{\eta+1}] = S'_0 [N - \eta - zp, p - 1]_\eta. \quad \dots \quad (III.)$$

In this expression, when  $\theta = I^* \left( \frac{N}{p} \right) - \eta$ , the term  $[N, p_{\eta+1}]$  vanishes, and the formula then becomes analogous to one published anonymously by Professor De Morgan in a paper printed in the fourth volume, p. 87, of the Cambridge Mathematical Journal.

4. In (II.), for  $[N, p_{\eta+1}]$  substitute  $[N - p\eta, p_1]$ , and transpose

\*  $I \left( \frac{N}{p} \right)$  is employed to avoid the long phrase, "the integer nearest to and not exceeding  $\frac{N}{p}$ ."



the terms. Then

$$[N, p_\eta] - [N - \eta, p - 1] = [N - p\eta, p_1]; \quad . \quad . \quad (IV.)$$

and this leads to

$$[N - \eta, p - 1] - [N - 2\eta, p - 2] = [N - p\eta, p - 1];$$

and that leads to the summation

$$[N, p_\eta] = S_z^p [N - p\eta, z_1]. \quad . \quad . \quad . \quad (V.)$$

The lower limit of  $z$  in (V.) is made 0, in order that the formula may comprehend the extreme case  $[0, 0_1] = 1$ , analogous to the extreme case in Combinations.

5. After substituting 1 for  $\eta$ , the author applies formula (IV.) to determining in how many different ways  $N$  can be resolved into  $p$  parts not less than 1. Let  $[N, p_1]$  be the term in a table of double entry corresponding to column  $N$ , line  $p$ , in the table. From the head, in line 0, of each of the columns 0, 1, 2, 3, &c., draw a diagonal, advancing one column and one line at a time. Take these diagonals one after another, and in each of them compute by formula (IV.) the terms situate on lines 0, 1, 2, 3, &c., one by one in succession. If  $N$  be the number at the head of the column from which any diagonal takes its departure, there will be only  $N$  terms to compute on that diagonal, the further terms being only repetitions of the term on the line  $N$ . For the diagonal in question intersects line  $N$  in column  $2N$ ; and, by formula V,

$$[2N, N_1] = S_z^N [N, z_1]$$

= the sum of all the terms in column  $N$ . But, moreover,

$$[2N + y, N_1 + y] = S_z^N [N, z_1]$$

= the same constant. The leading property of the table, indicated by the formula

$$[N, p_1] = S_z^p [N - p, z_1],$$

is, that the term  $[N, p_1]$  = the sum of all the terms in column  $N - p$ , from line 0 to line  $p$  inclusive. After the publication of the anonymous paper before referred to, Professor De Morgan discovered this theorem also, but he did not announce it\*.

## II. On Combinations.

1. In ordinary Combinations, the combining elements are of different kinds, and there is but one element of a kind: in the case here considered, there are different kinds of elements, and there may be many elements of a kind; and more than one element of a kind may enter into the same combination.

2. If  $u$  elements enter at a time into each combination, and the

\* The author has recently discovered an equivalent formula in p. 264 of Euler's *Int. in An. Infinitorum*; but investigated by a totally different method, and not applied as the author has applied it.

kinds are determinate in number, and their number is  $s$ , let  $\left\{ \begin{smallmatrix} u \\ s \end{smallmatrix} \right\}$  denote how many different combinations can then be formed: if the *elements* are determinate in number, and their number is  $\sigma$ , let the number of the combinations which can then be constructed, be denoted by  $\{u, \sigma\}$ . If  $\phi(x)$  be any function of  $x$ , let  $D^u \phi(x)$  denote the coefficient of  $x^u$  in that function developed according to the powers of  $x$ .

3. The same things as before being assumed, let a given set of elements consist of  $\alpha$  elements of the kind A,  $+\beta$  elements of the kind B,  $+\&c.$  Take the product, K, of the  $s$  geometrical progressions,

$[1 + Ax + A^2x^2 + \dots + A^\alpha x^\alpha]$ ,  $[1 + Bx + B^2x^2 + \dots + B^\beta x^\beta]$ ,  $\&c.$   
Then K will be of the form,

$1 + S[A]x + S[A^2 + AB]x^2 + S[A^3 + A^2B + ABC]x^3 + \&c.,$   
and  $D^u[K]$  will be of the form

$$S[A^p B^q C^r \&c.],$$

the last expression being an aggregate of terms of the form  $A^p B^q C^r \dots$ , each containing a different combination of  $u$  of the given elements, and their sum comprehending all the possible combinations of those elements taken  $u$  at a time. Now, if A, B, C,  $\&c.$  be each made equal to 1, K will become

$$k = [1 + x + x^2 + \dots + x^\alpha][1 + x + x^2 + \dots + x^\beta] \dots \&c.;$$

each of the terms  $A^p B^q C^r \dots$  will become 1, and the *number* of all the terms of the form  $A^p B^q C^r \dots$  which  $D^u[K]$  or  $S[A^p B^q C^r \dots]$  contains, that is to say,  $\{u, \sigma\}$  will be represented by  $D^u[k]$ ; which latter coefficient the author next proceeds to determine.

Now

$$k = \frac{1 - x^{\alpha+1}}{1 - x} \cdot \frac{1 - x^{\beta+1}}{1 - x} \cdot \&c. = [1 - x^{\alpha+1}][1 - x^{\beta+1}] \dots [1 - x]^{-s} \left. \begin{aligned} &= [1 - x^{\alpha+1}][1 - x^{\beta+1}] \dots S_0^\infty \left[ \frac{s^u \cdot 1^u}{1^u | 1} x^u \right] \end{aligned} \right\} \text{(VI.)}$$

$$= [1 - x^{\alpha+1}][1 - x^{\beta+1}] \dots \frac{1}{1^{s-1} | 1} S_0^\infty \left[ [u+1]^{s-1} | 1 x^u \right] \text{(VII.)}$$

For brevity, write  $u_1, \alpha_1, \beta_1, \&c.$  respectively, for  $u+1, \alpha+1, \beta+1, \&c.$ ; and also write  $[1]$  for  $[1-x]^{-s}$ ;  $[2]$  for  $[1-x^{\alpha_1}][1-x]^{-s}$ ; that is, for  $[1-x^{\alpha_1}] \cdot [1]$ ;  $[3]$  for  $[1-x^{\alpha_1}][1-x^{\beta_1}][1-x]^{-s}$ ; that is, for  $[1-x^{\beta_1}] \cdot [2]$ , and so on. Then

$$D^u[2] = D^u[1] - D^{u-\alpha_1}[1];$$

and

$$D^u[3] = D^u[2] - D^{u-\beta_1}[2];$$

\* According to the factorial notation, here used by the author,  $s^u \cdot 1^u$  represents  $s[s+1][s+2] \dots [s+(u-1)]$ .

and

$$D^u[4] = D^u[3] - D^{u-\gamma_1}[3]; \text{ and so on; (VIII.)}$$

and the developed product of the binomes,

$$[1-x^{\alpha_1}], [1-x^{\beta_1}], [1-x^{\gamma_1}], \&c.;$$

that is to say,

$$\begin{aligned} &1 - x^{\alpha_1} + x^{\alpha_1+\beta_1} - x^{\alpha_1+\beta_1+\gamma_1} + \&c. \\ &\quad - x^{\beta_1} + x^{\alpha_1+\gamma_1} - \&c. \\ &\quad - x^{\gamma_1} + x^{\beta_1+\gamma_1} \\ &\quad - \&c. + \&c. \end{aligned}$$

when multiplied into the development of  $[1-x]^{-s}$ , manifestly leads to the following formula :

$$D^u[m] = D^u[1] - S [D^{u-\alpha_1}[1]] + S [D^{u-\alpha_1-\beta_1}[1]] \left. \begin{aligned} &- S [D^{u-\alpha_1-\beta_1-\gamma_1}[1]] + \&c. \end{aligned} \right\} \text{(VIII*.)}$$

where, since the powers of  $x$ , in (VI.) or (VII.) developed, are to be all positive, no expression of the form

$$(u-\alpha_1), (u-\alpha_1-\beta_1), (u-\alpha_1-\beta_1-\gamma_1), \&c.$$

is to be negative. Then by giving to

$$D^u[1], D^{u-\alpha_1}[1], D^{u-\alpha_1-\beta_1}[1], \&c. \quad \text{(IX.)}$$

their respective values, we obtain the series of expressions :

$$D^u[1] = \frac{1}{1^{s-1|1}} u_1^{s-1|1} = \frac{s^{u|1}}{1^{u|1}} = \left[ \begin{matrix} u \\ s \end{matrix} \right]$$

where in all the kinds the elements are plural without limit ; a formula given by Hirsch :

$$D^u[2] = \frac{1}{1^{s-1|1}} \left[ u_1^{s-1|1} - [u_1 - \alpha_1]^{s-1|1} \right] = \left[ \begin{matrix} u \\ s \end{matrix} \right]$$

where the elements A are limited in number to  $\alpha$ , but those of the other  $(s-1)$  kinds are plural without limit :

$$D^u[3] = \frac{1}{1^{s-1|1}} \left[ \begin{aligned} &u_1^{s-1|1} - [u_1 - \alpha_1]^{s-1|1} + [u_1 - \alpha_1 - \beta_1]^{s-1|1} \\ &\quad - [u_1 - \beta_1]^{s-1|1} \end{aligned} \right] = \left[ \begin{matrix} u \\ s \end{matrix} \right]$$

where, moreover, the elements B are limited in number to  $\beta$ , but those of the other  $(s-2)$  kinds are plural without limit : and so for the rest. The law of the terms being evident, they need not be continued further.

Example of (IX.). Given one element of 1 kind, two elements of a 2nd kind, three of a 3rd, and four of a 4th ; and let  $u=5$ . Then

$$\{u, \sigma\} = \frac{1}{1.2.3} \left[ \begin{array}{r} -4.5.6 \\ 6.7.8 - 3.4.5 + 1.2.3 \\ -2.3.4 \\ -1.2.3 \end{array} \right] = 22.$$

4. If  $\alpha=\beta=\gamma=\&c.$ , formula (IX.) becomes

$$\{u, \sigma\} = \frac{1}{1^{\sigma-1}|1|} S^I_{\theta} \left( \frac{u_1}{\alpha_1} \right) \left[ (-1)^{\theta} \frac{s^{\theta-1}}{1^{\theta}|1|} [u_1 - \theta \alpha_1]^{\sigma-1|1|} \right]. \quad (X.)$$

Example of formula (X.) Given seven kinds of elements, and three of each kind ; and let  $u=4$ . Then

$$\{u, \sigma\} = \frac{1}{1.2.3.4.5.6} [5.6.7.8.9.10 - 7.1.2.3.4.5.6] = 203.$$

5. If it is required to determine many, or all, of the terms of the series  $\{0, \sigma\}, \{1, \sigma\}, \{2, \sigma\}, \dots, \{\sigma, \sigma\}$ , formulas (VIII.) suggest the following process for the determination of those terms. An example will best explain the process.

Given 1 element of one kind, 2 elements of a second kind, and 3 of a third kind. How many combinations can be formed from these elements, when taken 0, 1, 2, 3, 4, 5, 6 at a time, respectively ?

Values of $u$ .....	0	1	2	3	4	5	6
Coefficients of $x^u$ in $[1-x]^{-3}$ .....	1	3	6	10	15	21	28
Multiply by $[1-x^2]$ ; that is, subtract .....	...	...	1	3	6	10	15
Coefficients of $x^u$ in $[1-x^2][1-x]^{-3}$ .....	1	3	5	7	9	11	13
Multiply by $[1-x^3]$ ; that is, subtract .....	...	...	...	1	3	5	7
Coefficients of $x^u$ in $[1-x^2][1-x^3][1-x]^{-3}$ .....	1	3	5	6	6	6	6
Multiply by $[1-x^4]$ ; that is, subtract .....	...	...	...	...	1	3	5
Coefficients of $x^u$ in $[1-x^2][1-x^3][1-x^4] \{ [1-x]^{-3} \}$ .....	1	3	5	6	5	3	1
That is .....	$\{0, \sigma\}$	$\{1, \sigma\}$	$\{2, \sigma\}$	$\{3, \sigma\}$	$\{4, \sigma\}$	$\{5, \sigma\}$	$\{6, \sigma\}$

6. Let a set of elements, S, such as we have been previously considering, consist of two similar sets, T and T', which do not contain in common any elements of the same kind. If S consists of  $\sigma$  elements combining  $u$  at a time, and T consists of  $\tau$  elements combining  $v$  at a time, T' will consist of  $(\sigma-\tau)$  elements combining  $(u-v)$  at a time. Consider  $u$  as constant, for the moment, and  $v$  as variable. The author then shows that if by the process described in art. 5, the whole series of terms  $\{v, \tau\}$  and the whole series of terms  $\{u-v, \sigma-\tau\}$ , have been determined, we can thence determine the whole series of terms  $\{u, \sigma\}$  by means of the formula

$$\{u, \sigma\} = S^{\tau}_0 [\{v, \tau\} . \{u-v, \sigma-\tau\}]; \quad . \quad . \quad (XI.)$$

and of this he gives examples.

7. In formula (XI.) substitute  $(\sigma-u)$  for  $u$ ; and develope  $\{u, \sigma\}$  and  $\{\sigma-u, \sigma\}$  in the manner indicated by that formula. By comparing the 1st, 2nd, 3rd, &c. terms respectively of  $\{u, \sigma\}$  with the last, last but one, last but two, &c. terms of  $\{\sigma-u, \sigma\}$ , and vice



versd, the author shows that  $\{u, \sigma\}$  will be identical with  $\{\sigma - u, \sigma\}$ , provided  $\{v, \tau\}$  is identical with  $\{\tau - v, \tau\}$ , and provided also  $\{u - v, \sigma - \tau\}$  is identical with  $\{\sigma - \tau - (u - v), \sigma - \tau\}$ . But this identity actually exists when  $T$  consists of elements of one kind only, and when  $T'$  also consists of elements of one kind only. For, in that case, every term of the series  $\{v, \tau\}$  and every term of the series  $\{u - v, \sigma - \tau\}$  is equal to 1. Let the elements of the single kind which  $T$  contains, be different from those of the single kind which  $T'$  contains. Then the identity in question will exist, when  $S$  consists of elements, finite in number, of two different kinds: consequently, it exists also when  $T$  consists of elements, finite in number, of two different kinds, and  $T'$  consists of elements, finite in number, of one or two other kind or kinds; that is, when  $S$  consists of elements, finite in number, of three or four different kinds. And therefore universally, in the case as well of finitely plural, as of singular elements, the following law obtains:

$$\{u, \sigma\} = \{\sigma - u, \sigma\}. \quad \dots \quad (XII.)$$

Hence it follows that in applying formulas (IX.) and (X.) to particular cases, the labour of computation will be shortened by substituting for the variable the lesser of the two numbers  $u$  and  $\sigma - u$ .

8. The author next considers how many different combinations can be formed from a given set of elements, when every combination is to be constructed in conformity with a given type; in which type there are  $m$  different kinds containing  $v$  elements each,  $m'$  other different kinds containing  $v'$  elements each,  $m''$  other different kinds containing  $v''$  elements each, and so on; and where, consequently, in each combination,  $z$ , the number of kinds, is  $m + m' + m'' + \&c.$ ; and  $u$ , the number of elements, is  $mv + m'v' + m''v'' + \&c.$  The type remaining constant, any combination conformable thereto may be altered, either by changing the particular  $z$  kinds which are selected out of the  $s$  given kinds; or, the kinds remaining the same, by altering the distribution of the parts  $v, v, v, \dots (m)v', v', v', \dots (m')v'', v'', v'', \dots (m'') \&c.$ , among those kinds. When all the elements are plural without limit, the changes of the former description will be represented by

$$\frac{s^z - 1}{1^{z|1}};$$

and those of the latter description by

$$\frac{1^{z|1}}{1^{m|1} \cdot 1^{m'|1} \cdot 1^{m''|1} \dots}$$

and their joint effect by the product

$$\frac{s^z - 1}{1^{z|1}} \times \frac{1^{z|1}}{1^{m|1} \cdot 1^{m'|1} \cdot 1^{m''|1} \dots} \quad \dots \quad (XIII.)$$

But when the elements of all the given kinds are finite in number, class these kinds, so that each kind in class 1 contains not fewer

than  $v$  elements; each kind in class 2 contains fewer than  $v$ , but not fewer than  $v'$  elements; each kind in class 3 contains fewer than  $v'$ , but not fewer than  $v''$  elements; and so on; and so that the given kinds may in this way be reduced, say, to  $t$  kinds containing  $v$  elements each +  $T'$  kinds containing  $v'$  elements each +  $T''$  kinds containing  $v''$  elements each, &c. Then let  $t - m + T' = t'$ ;  $t' - m' + T'' = t''$ ; and so on. The given kinds being thus ordered, since we are required to select, 1st,  $m$  out of  $t$  kinds; then, 2nd,  $m'$  out of  $t'$  kinds; then, 3rd,  $m''$  out of  $t''$  kinds; and so on; the number of the different combinations which can be constructed from those kinds in conformity with the type, will be

$$\frac{t| - 1}{1^{m|1}} \cdot \frac{t'| - 1}{1^{m'|1}} \cdot \frac{t''| - 1}{1^{m''|1}}, \text{ \&c. . . . . (XIV.)}$$

If  $tv + T'v' + T''v'' \dots$  &c. is reduced to a single term,  $t.v$ ; then formula (XIV.) becomes

$$\frac{t| - 1}{1^{m|1} \cdot 1^{m'|1} \cdot 1^{m''|1}}, \text{ \&c. . . . . (XV.)}$$

Example of (XIV.). Given eight elements of 1 kind, seven of a 2nd kind, six of a 3rd, five of a 4th, four of a 5th, three of a 6th, two of a 7th, and one element of an 8th kind, out of which it is required to construct combinations, each consisting of three kinds with five elements each + two kinds with three elements each + one kind with two elements. Of such combinations there can be formed

$$\frac{4^3| - 1}{1^3|1} \cdot \frac{3^2| - 1}{1^2|1} \cdot \frac{2^1| - 1}{1^1|1} = 24.$$

9. If it be required to determine how many different combinations can be constructed, each containing  $u$  elements of  $z$  kinds, and the given elements are all finite in number; we must form all the different  $z$ -partitions of  $u$ ; and each of these partitions being regarded as a type, we must determine, by formula (XIV.) or (XV.), how many combinations correspond to each of these types; and the total number required will be the sum of all these particular determinations. But if the given elements may all be repeated without limit, it follows from formula (XIII.), that the sum of all the particular determinations may be represented by

$$\frac{z^z| - 1}{1^{z|1}} \times S. \left( \frac{1^{z|1}}{1^{m|1} \cdot 1^{m'|1} \cdot 1^{m''|1}, \text{ \&c.}} \right).$$

Now

$$S \left( \frac{1^{z|1}}{1^{m|1} \cdot 1^{m'|1} \cdot 1^{m''|1}} \right)$$

denotes how many different permutations can be formed, when, in each different  $z$ -partition of  $u$ , the parts are permuted  $z$  together at a time; and the number of such permutations is

$$\Sigma^{z-1} [1] = \Sigma^{z-2} [u-1] = \frac{[u-1]^{z-1| - 1}}{1^{z-1|1}}.$$

Consequently the required sum is

$$\frac{s^{z|-1}}{1^{z|1}} \times \frac{[u-1]^{z-1|-1}}{1^{z-1|1}} \dots \dots \dots \text{(XVI.)}$$

If in (XVI.)  $z$  varies from 0 to  $u-1$ ,

$$S_{z=0}^{u-1} \left[ \frac{s^{z|-1}}{1^{z|1}} \times \frac{[u-1]^{z-1|-1}}{1^{z-1|1}} \right] = \frac{s^{u|1}}{1^{u|1}},$$

this summation being a particular case of formula (XI.). The result agrees with  $D^u[1]$  formula (IX.), art. 3.

10. When the given elements are all finite in number, we may determine  $\{u, \sigma\}$ , by taking the sum of all the particular determinations that may be obtained pursuant to art. 9, by giving to  $z$  the successive values 0, 1, 2, 3, &c. If  $u < s$ , the upper limit of  $z$  is  $u$ , and the number of types to be formed is  $[2u, u_1]$ ; which becomes  $[2s, s_1]$ , if  $u = s$ . If  $u > s$ , the upper limit of  $z$  is  $s$ ; and the number of types to be formed is  $[u+s, s_1]$ . (See articles 4 and 5, Section I.) But, if the repetition is finite, some of these partitions may fail to yield combinations.

11. If the elements A, B, C, &c. represent different prime numbers, all the methods and theorems contained in this section will apply, *mutatis mutandis*, to the composite numbers of which those primes, or the powers of those primes, are divisors.

### III. On Permutations.

1. Let the given elements be of  $s$  different kinds. We can determine in two known cases, by an explicit function of  $u$ , when the elements are taken  $u$  at a time, in how many different ways they can be permuted. The number of the permutations is denoted, when there is but one element of a kind, by  $s^{u|-1}$ ; and when in all the kinds the elements are plural without limit, by  $s^u$ . When the plurality is finite, it is only in the particular case of all the elements being permuted at a time, that there is a known formula to express the number of their permutations.

2. Every combination constructed on a given type,  $u = mv + m'v' + m''v'' + \&c.$ , will generate the same number of permutations,

$$\frac{1^{u|1}}{[1^{v|1}]^m [1^{v'|1}]^{m'} [1^{v''|1}]^{m''} \&c.} = P.$$

Therefore, if the number of the different combinations which can be constructed out of the given elements in conformity with that type, is represented by  $Q$ ,  $Q \times P$  will be the number of the permutations corresponding to the type and to those elements. If the plurality be without limit,

$$\frac{s^{z|-1}}{1^{m|1} \cdot 1^{m'|1} \cdot 1^{m''|1} \&c.} \times P$$

will be the number of the permutations. If the given elements be finite in number, as in formulas (XIV.) and (XV.), the number of

the permutations corresponding to those elements and to the type, will be

$$\frac{t^{m|}-1}{1^{m|1}} \cdot \frac{t^{m'|}-1}{1^{m'|1}} \cdot \frac{t^{m''|}-1}{1^{m''|1}} \&c. \times P.$$

Every different partition of  $u$  that may be formed within the limits pointed out in art. 10, Section II., will give rise to a similar product,  $Q \times P$ ; and the sum of all these particular products,  $S[Q \times P]$ , will show how many different permutations can be formed from the given elements, taken  $u$  at a time. The author illustrates this method of computing the number of permutations, by examples.

3. Let  $P \left\{ \begin{smallmatrix} u \\ s \end{smallmatrix} \right\}$  denote how many different permutations can be formed when  $u$  elements are taken at a time out of  $s$  kinds; and  $P \{u, \sigma\}$  denote how many different permutations can be formed when  $u$  elements are taken at a time out of  $\sigma$ , a finite number of elements. If all the elements may be repeated without limit,

$$\begin{aligned} \left\{ \begin{smallmatrix} u \\ s \end{smallmatrix} \right\} &= D^u [1^{u|1} \cdot \epsilon^{sx}] = s^u \\ &= D^u \left[ 1^{u|1} \left[ 1 + x + \frac{x^2}{1 \cdot 2} + \dots + \frac{x^u}{1^{u|1}} + \dots \right]^s \right]. \end{aligned}$$

Hence the author infers that, if the elements  $A$  are limited in number to  $\alpha$ , while those of the other  $(s-1)$  kinds are plural without limit,

$$P \left\{ \begin{smallmatrix} u \\ s \end{smallmatrix} \right\} = D^u \left[ 1^{u|1} \epsilon^{(s-1)x} \left[ 1 + x + \frac{x^2}{1 \cdot 2} + \dots + \frac{x^\alpha}{1^{\alpha|1}} \right] \right];$$

that if, moreover, the elements  $B$  are limited in number to  $\beta$ , while the other  $(s-2)$  kinds are plural without limit,

$$\begin{aligned} P \left\{ \begin{smallmatrix} u \\ s \end{smallmatrix} \right\} &= D^u \left[ 1^{u|1} \epsilon^{(s-2)x} \left[ 1 + x + \frac{x^2}{1 \cdot 2} + \dots + \frac{x^\alpha}{1^{\alpha|1}} \right] \right. \\ &\quad \left. \left[ 1 + x + \frac{x^2}{1 \cdot 2} + \dots + \frac{x^\beta}{1^{\beta|1}} \right] \right]; \end{aligned}$$

and so on, until finally, if all the elements are finite in number, and the elements  $A, B, C, \&c.$  are respectively limited, in point of number, to  $\alpha, \beta, \gamma, \&c.$ ,

$$P \{u, \sigma\} = D^u \left[ 1^{u|1} \left[ 1 + x + \dots + \frac{x^\alpha}{1^{\alpha|1}} \right] \left[ 1 + x + \dots + \frac{x^\beta}{1^{\beta|1}} \right] \left[ 1 + x + \dots + \frac{x^\gamma}{1^{\gamma|1}} \right] \right] \dots \quad (\text{XVII.})$$

4. Hence, if in all the  $s$  kinds the elements are dual, (XVII.) becomes



$$P\{u, \sigma\} = D^u \left[ 1^{u|1} \left[ 1 + x + \frac{x}{2} \right]^s \right] = \left\{ S^1_{\sigma} \left( \frac{u}{2} \right) \left[ \frac{u^{2\sigma-1} \times s^{u-\sigma-1}}{2^{\sigma/2}} \right] \right\}. \quad (\text{XVIII.})$$

This is the only addition which the author has been able to make to the cases wherein  $P \left[ \begin{smallmatrix} u \\ s \end{smallmatrix} \right]$ , or  $P\{u, \sigma\}$  is expressed by an explicit function of  $u$ , symmetrical in form.

Example. Let there be five kinds of elements, and two of each kind. Let  $u=3$ .

$$P\{u, \sigma\} = \frac{1.5.4.3}{1} + \frac{3.2 \times 5.4}{2} = 120.$$

5. The author gives the following theorem, which is precisely analogous to that of art. 6, Sect. II., formula (XI.), in Combinations; viz.

$$P\{u, \sigma\} = S^{\sigma}_0 \left[ \frac{u^{\sigma-1}}{1^{\sigma-1}} P\{v, \tau\} \cdot P\{u-v, \sigma-\tau\} \right]. \quad (\text{XIX.})$$

6. By a mode of proof precisely analogous to that employed in art. 7, Sect. II., he shows that  $P\{\sigma-1, \sigma\} = P\{\sigma, \sigma\}$ ; that is to say, that

$$\frac{1^{\sigma|1}}{1^{\alpha|1} \cdot 1^{\beta|1} \cdot 1^{\gamma|1} \cdot \&c.}$$

denotes the number of permutations that can be formed with  $\alpha$  elements A,  $\beta$  elements B, &c. (where  $[\alpha + \beta + \gamma + \&c.] = \sigma$ ), as well when  $\sigma-1$  elements, as when  $\sigma$  elements, are taken at a time.

Since correcting his paper for publication, the author has had his attention called to the work of Bézout on Elimination (4to. Paris, 1779, p. 469), as containing a formula similar in structure to that numbered VIII\*. in the present abstract.

Bézout investigates the composition of a polynome function of  $s$  quantities, A, B, C, &c., consisting of terms which are of the form  $A^p B^q C^r$ , and of every dimension from 0 to  $u$  inclusive. Let  $[s]^u$  denote such a polynome, complete in all its terms, and  $N[s]^u$  the number of its terms. Then, 1st,

$$N[s]^u = \frac{[u+1]^{\sigma|1}}{1^{\sigma|1}};$$

and 2nd, the number of the terms in  $[s]^u$  which are not divisible by either  $A^{\alpha}$ , or  $B^{\beta}$ , or  $C^{\gamma}$ , &c., he expresses by

$$\begin{aligned} N[s]^u - N[s]^{u-\alpha} + N[s]^{u-\alpha-\beta} - \&c. \\ - N[s]^{u-\beta} + \&c. \\ - \&c. \end{aligned}$$

He also observes (p. 39) that when  $A^\alpha$ ,  $B^\beta$ ,  $C^\gamma$ , &c. are the highest powers of  $A$ ,  $B$ ,  $C$ , &c. which a polynome, agreeing in other respects with  $[s]^u$ , contains, the terms of such incomplete polynome will agree in point of number with those terms in  $[s]^u$  which are not divisible by either  $A^{\alpha+1}$ , or  $B^{\beta+1}$ , or  $C^{\gamma+1}$ , &c. The polynomes from which Bézout proposes to eliminate certain terms, contain terms of all dimensions from 0 to  $u$  inclusive. The terms which are to remain after the others have been eliminated, and which are enumerated by means of the condition, that they are not divisible by certain powers of  $A$ , or  $B$ , or  $C$ , &c., may be of all dimensions indiscriminately from 0 to  $u$  inclusive. Bézout's object is exclusively Elimination, and he makes no allusion to any other application of his formulæ.

The polynomes considered by the author, taken in their entirety, agree in their general structure with those considered by Bézout; but the nature of the author's inquiries led him to confine his attention to the composition of those particular terms in a polynome which were of the same dimension; and to seek to express the number of the terms, not of all dimensions indiscriminately, but of each particular dimension separately. To show how it has happened that researches, very different at their point of departure, have, as regards one point of investigation, ended in nearly similar formulæ, the author proceeds to deduce his formula (VIII\*) from the investigations of Bézout. Such a deduction, he conceives, might readily have been made by any one to whom it had occurred to make it; and the application of such a deduction, when once made, to problems in Combinations, would have been much too obvious to have remained long unnoticed.

Expressions of the form above considered are regarded by Bézout as of the nature of Differences; and the truth of this view of the subject may be shown in the following brief manner.

If  $\phi(x)$  generates  $\psi(u)$ ,  $[1-x^\alpha]\phi(x)$  will generate  $\psi(u)-\psi(u-\alpha)$ , which we may denote by  $\Delta_\alpha\psi(u)$ . Consequently  $[1-x^\beta][1-x^\alpha]\phi(x)$ , that is to say,

$$\left\{ \begin{array}{l} 1-x^\alpha+x^{\alpha+\beta} \\ -x \end{array} \right\} \phi(x),$$

will generate  $\Delta_{\alpha,\beta}^2\psi(u)$ ; and so on; the independent variable,  $u$ , undergoing, not uniform, but variable decrements,  $\alpha$ ,  $\beta$ ,  $\gamma$ , &c.

### *L. Intelligence and Miscellaneous Articles.*

#### ON THE ARTIFICIAL PRODUCTION OF MINERALS, AND ESPECIALLY OF PRECIOUS STONES.

**M.** EBELMEN states that the first results which he obtained related to minerals of the family of Spinelles.

The method adopted by the author to effect the crystallization of these compounds, is based on the property which boracic acid pos-

sesses of dissolving metallic oxides in the dry way, and the volatility of this acid at a high temperature. It occurred to him that by dissolving alumina and magnesia, mixed in the proportions which constitute spinelle, in fused boracic acid, and exposing the mixture in open vessels to the high temperature of a porcelain furnace, that the affinity of the alumina for the magnesia might cause the separation of a crystallized aluminate and the expulsion of the boracic acid. The proportions employed were about one part of fused boracic acid, and two parts of a mixture of alumina and magnesia, composed so as to constitute the compound  $\text{Al}^2\text{O}^3\text{MgO}$ ; and from  $\frac{1}{100}$  to  $\frac{1}{200}$  of bichromate of potash were added to it. The ingredients, well-mixed, were placed on platina foil, in a cup of porcelain, and exposed to the highest temperature of the porcelain furnace of Sèvres. A product was obtained the surface of which was covered with crystalline facets, and the interior contained cavities sprinkled with crystals, the form of which was readily distinguishable with a glass. These crystals were rose-red, transparent, scratched quartz readily, and had the form of the regular octohedron without any modification. They were completely infusible by the blowpipe. These characters, combined with the composition of the crystals as deduced from synthesis, appear to M. Ebelmen sufficiently conclusive as to their identity with spinelle.

By substituting the equivalent of protoxide of manganese for magnesia, a crystalline product was obtained in large laminæ, exhibiting the form of equilateral triangles or regular hexagons. The author considers these as constituting the manganesian spinelle  $\text{Al}^2\text{O}^3\text{MnO}$ , which has not hitherto been met with in the mineral kingdom.

Oxide of cobalt substituted for magnesia, equivalent for equivalent, yielded crystals of a black-blue colour, in regular octohedrons. They also scratched quartz, but not so readily as the two preceding.

In employing alumina and glucina in the proportions which constitute cymophane  $\text{Al}^2\text{O}^3\text{GlO}$ , a mass covered with crystalline asperities of great splendour was obtained. This product scratched quartz and even topaz distinctly; it therefore possessed hardness comparable to that of natural crystallized cymophane.

Certain silicates, which are infusible by the heat of our furnaces, appear also to be produced by the same process. Thus, on fusing the elements of emerald with half their weight of boracic acid at the same temperature as in the preceding experiments, a substance is obtained which easily scratches quartz, and its surface presents a great number of facets, the form of which is the regular hexagon.

The author proposes to continue these experiments, but at present only states in addition, that it is possible to produce at temperatures lower than those obtainable in our furnaces, diaphanous crystals, the hardness and external characters of which are analogous to those of precious stones; and he also concludes that many mineral species may be formed at a lower temperature than that required for their fusion.—*Comptes Rendus*, August 16, 1847.

## ANALYSIS OF THE GRAY COPPER FROM MOURAÏA IN ALGERIA.

M. Ebelmen states that a copper mine, apparently of great importance, has been for some time worked at the foot of the defile of Mouraïa in Algeria. The veins are composed principally of carbonate of iron and gray copper; the latter sometimes occurring in compact masses and sometimes in crystals, the prevailing form of which appears to be a rhombic dodecahedron, but with numerous modifications on the edges and angles.

The specimens received by M. Ebelmen for analysis contained a great number of very brilliant small crystals of gray copper, on a gangue composed of carbonate of iron and sulphate of barytes. These specimens were digested for some time in warm dilute hydrochloric acid, which dissolved the carbonate of iron without altering the gray copper, the crystals of which were then readily detached.

Qualitative experiments, conducted in the usual manner, showed that the ore contained sulphur, arsenic, antimony, copper, iron and zinc: lead, bismuth, and mercury were tried for, but not the smallest quantity was found. No notable quantity of silver could be detected; and the fact that M. Berthier found 0·0008 in 1 part of the ore, shows that the silver is very irregularly interspersed through the veins.

For the quantitative analysis of this ore, M. Ebelmen employed, with a slight modification, the method proposed by M. H. Rose; and taking the mean of several experiments, he obtained the following as the composition of this ore:—

Sulphur .....	27·25
Antimony .....	14·77
Arsenic .....	9·12
Copper .....	41·57
Iron .....	4·66
Zinc .....	2·24
	<hr/>
	99·61

If the analysis of this ore be compared with that of gray copper from various localities, the greatest similarity will be found between it and that from Sainte-Marie-aux Mines, which gave M. H. Rose—

Sulphur .....	26·83
Antimony .....	12·46
Arsenic .....	10·19
Copper .....	40·60
Iron .....	4·66
Zinc .....	3·69
Silver .....	0·60
	<hr/>
	99·03

*Annales des Mines*, tome xi., p. 47.



## ANALYSIS OF KUPFERNICKEL.

M. Ebelmen states that this mineral comes from Ayer, in the valley of Annivier (H<sup>t</sup> Valais). It possesses the usual characters of Kupfernickel. It forms compact masses which are perfectly homogeneous, but exhibit no traces of crystals; the ore is mixed with laminar carbonate of lime, which is easily separated by dilute hydrochloric acid. Its density is 7.39.

The analysis was effected by treating the purified mineral with aqua regia. The sulphuric acid was precipitated by chloride of barium and the excess of barium by sulphuric acid. The arsenic acid was converted into arsenious acid by means of ebullition with sulphurous acid, and the arsenious acid was precipitated by sulphuretted hydrogen. The sulphuret of arsenic obtained was, after drying and weighing, analysed by aqua regia to obtain the sulphur; by heating another portion in a current of hydrogen, a minute residue of antimony was obtained. The liquor freed from sulphuret of arsenic was concentrated along with nitric acid, and precipitated by excess of ammonia; an abundant precipitate of peroxide of iron was formed, which retained a little nickel, as appeared from its colour.

It was redissolved on the filter by hydrochloric acid, and the liquor was then treated cold with carbonate of barytes. The peroxide of iron only was precipitated; the carbonate of barytes, with which it was mixed, was readily separated. The liquor containing the nickel was treated with sulphuric acid, and after filtration it was added to the ammoniacal solution of the rest of the nickel; this was precipitated by excess of potash, and after drying and calcining, it was weighed, and its quantity indicated that of the metallic nickel.

The ammoniacal liquor, afterwards treated with hydrosulphate of ammonia, yielded a slight black precipitate, which, collected, calcined and weighed, gave with borax the reaction of cobalt.

The results of the various experiments showed that the ore consisted of—

Arsenic .....	54.05
Antimony .....	0.05
Nickel .....	48.50
Cobalt .....	0.32
Iron .....	0.45
Sulphur .....	2.18
Gangue .....	0.20
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	100.75

*Annales des Mines*, tome xi. p. 56.

## ON THE DEHYDRATION OF MONOHYDRATED SULPHURIC ACID.

M. Barreswil observes that anhydrous sulphuric acid has been hitherto prepared by distilling protosulphate of iron or dry bisulphate of soda. These two processes produce an anhydrous salt and sulphuric acid. The author states that he is not aware that an

attempt has ever been made to deprive concentrated sulphuric acid of spec. grav. 1.848 of its water, without previously causing it to enter into combination and form a salt. The same may also happen when monohydrated sulphuric acid is employed in the preparation of fluoboric and fluosilicic acids, which are considered as substances having great affinity for water.

The reaction which M. Barreswil employs he considers as extremely simple. He mixes anhydrous phosphoric acid with the sulphuric acid of commerce, and leaves them in contact, and the mixture is afterwards heated: the combination of the two acids produces an increase of temperature, and some acid vapours soon appear; but this is prevented by proceeding cautiously, and keeping the acids in a freezing mixture: by distillation anhydrous sulphuric acid is disengaged, and vitreous hydrated phosphoric acid remains; the distillation is effected in the same way as the Saxon acid.

A circumstance which struck the author in this operation, is the fact of the innocuousness of the mixture of monohydrated sulphuric acid and anhydrous phosphoric acid, with respect to organic matters, such as paper and cotton, which are instantaneously destroyed by the Saxon acid. The author considers this circumstance as a proof that the sulphuric acid in the mixture is not anhydrous, but becomes so when heat is applied.

Even if the reaction above described possesses interest in a theoretical point of view, M. Barreswil admits that as a manufacturing process it is unimportant, and will hardly be regarded as a ready method of obtaining anhydrous sulphuric acid. The high price of phosphorus, and the difficulty of preparing anhydrous phosphoric acid, are obstacles to the employment of the process.—*Comptes Rendus*, Juillet 5, 1847.

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#### OBSERVATIONS ON SILICA. BY M. DOVERI.

It results from the experiments detailed by the author—

1. That the alkaline silicates, when decomposed by acids, and particularly hydrochloric acid, deposit the greater part of the silica which they contain if the acid in excess be added drop by drop; whereas the same quantity of acid added at once does not occasion the precipitation of the smallest portion of silica.

2. That silica, once precipitated, does not redissolve in acids, whatever may have been its origin, whether precipitated from an alkaline silicate by an acid, or from fluoride of silicium by water.

3. That weak acids, as the carbonic, sulphurous, boracic and the vegetable acids, decompose the alkaline silicates at common temperatures, and precipitate the silica either as a jelly or in gelatinous flocculi.

4. That very finely-divided silica, whether anhydrous or hydrated, is capable of decomposing the aqueous solutions of the alkaline carbonates, and dissolving in the solution at a boiling heat.

5. That silica precipitated at common temperatures from a solu-

tion of an alkaline silicate or from fluoride of silicium, is a hydrate of definite proportions, the composition of which may be represented by the formula  $\text{HO, Si O}^2$ . This hydrate, when heated to  $212^\circ \text{F.}$ , loses one equivalent of water, and is converted into another compound,  $\text{HO, 2Si O}^2$ .

6. That when a solution of an alkaline silicate is treated with a metallic solution, a precipitate is formed, which is a mixture of hydrate of silica and a metallic silicate; the metallic silicate being entirely dissolved by the mineral acids, while the free silica remains undissolved.

7. That a limpid and very strong solution of silica in hydrochloric acid may be obtained by dissolving in this acid silicate of copper, and precipitating the copper by sulphuretted hydrogen.

8. That a solution of silica in hydrochloric acid, slowly evaporated under the receiver of the air-pump, gives hydrate of silica ( $\text{HO, Si O}^2$ ) perfectly crystallized in very small transparent needles, grouped either in stars or tufts.—*Comptes Rendus*, Juillet 19, 1847.

#### ON NITRIC MANNITE. BY M. SOBRERO.

Since the action of nitric acid on organic bodies has been studied, a number of substances of great interest to science have been discovered; but the arts have hitherto acquired only fulminating-cotton, the fate of which is as yet uncertain. Whilst the question as to cotton is under consideration, M. Sobrero announces to the Academy another body which is fulminating in the highest degree, resulting from the action of nitric acid upon mannite—the nitric mannite, the composition of which has been already given by MM. Flores Domonte and Ménard.

Fulminating mannite possesses the property of detonating by the stroke of a hammer with as much violence as fulminate of mercury, and produces, during its decomposition, sufficient heat to inflame gunpowder. As soon as the author was acquainted with this property, he set about to apply it, and prepared capsules with it instead of detonating mercury for the discharge of fire-arms, and a fowling-piece was discharged by it.

With respect to its use, the author has arrived at the following conclusions:—

1. Fulminating mannite must always be cheaper than fulminating mercury.

2. It is more conveniently prepared, and does not expose the workmen to the great danger which attends the manufacture of fulminating mercury.

It must be cheaper than fulminating mercury, because the price of manna is not very high; because in the preparation of mannite an uncrystallizable residue is obtained, mixed with a little mannite, which may be employed in medicine and the veterinary art as a purgative; and because, according to the analyses of MM. Flores

Domonte and Ménard, the mannite, in becoming nitric mannite, must increase considerably in weight (from 100 to 225).

It is less dangerous in preparation and manipulation: in fact the preparation is merely accompanied with the disengagement of some vapour of nitric acid.

Fulminating mannite requires for detonation a violent blow between two hard bodies; heat gradually applied to it fuses and afterwards decomposes it, but without detonation. In fact it may be placed on paper and touched with a red-hot coal, and fused without detonation; the paper on which it is put may be burnt, and it is decomposed without detonation.

Lastly, fulminating mannite is decomposed by the blow of a hammer, without, as far as appears, producing nitrous vapours. It seems to be entirely decomposed into carbonic acid, water and azote; besides which it keeps indefinitely without undergoing decomposition.—*Comptes Rendus*, Juillet 19, 1847.

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#### ON THE EXTRACTION OF SILVER.

BY MM. MALAGUTI AND DUROCHER.

From the numerous researches which the authors have made on a large series of specimens from different parts of Europe, they have inferred the general fact, that all metallic compounds which accompany or are found near argentiferous minerals contain more or less silver; so that they deem it an established fact, that silver is probably one of the most widely-diffused metals in nature.

The researches of the authors have been made on sulphurets, arseniurets, arsenio-sulphurets, some metallic oxides, and even native metals. This fact being established, the mode in which the silver exists occupied their attention. As the subject appeared a difficult one, it was simplified by inquiring in what state the silver existed in galena, blende and pyrites, and they supposed it could exist only in the native state, as chloride or sulphuret. Experiments appeared to show that in these sulphurets the silver is not in the metallic state; and experiments still more numerous and decisive seemed also to prove that the silver could not be in the state of chloride; and on this occasion they remarked a circumstance which has hitherto escaped the observation of chemists:—They found that all metallic sulphurets, properly so called, and even some arseniurets, possess the property of decomposing a certain quantity of chloride or bromide of silver. This decomposition is effected more or less slowly when contact is effected merely by water; but it is produced much more rapidly, and in some cases even instantaneously, when the chloride or bromide of silver is in solution.

By comparative trials the authors succeeded in determining the decomposing power of a great number of sulphurets and several arseniurets. Thus—



100 of sulphuret of zinc decompose 3 of chloride of silver				
100	....	cadmium	.. 14	....
100	....	bismuth	.. 2	....
100	....	lead	.. 5	....
100		protosulphuret of tin	.. $\frac{1}{2}$	....
100		of bisulphuret of tin	.. 30	....
100		protosulphuret of copper	360	....
100		arseniuret of antimony	120	....
100	....	cobalt	166	....

In operating with natural sulphurets, the authors remarked very considerable differences in their decomposing power. They attribute these differences to the presence of small quantities of sulphurets or arseniurets of very high decomposing power; and they suppose they may sometimes attach to the molecular condition of the bodies. They found, for example, that a very pure and well-crystallized blende from Königsberg possessed decomposing power equal to that of artificial sulphuret of zinc; while a blende equally pure and as well crystallized, but coming from Radna, had a decomposing power which was twice as weak, and yet these two blendes were of equal density.

The authors draw the following conclusions from the results of their experiments:—

All pure metallic sulphurets possess the power of decomposing, under certain circumstances, a given quantity of chloride of silver, and even of other insoluble chlorides. This power appears to be modified in some cases by the molecular condition.

The decomposition of chloride of silver by sulphurets may be effected,—1st, by double decomposition; 2nd, by reduction; 3rd, by simultaneous reduction and double decomposition.

Natural sulphurets sometimes exhibit very high absorbent powers, on account of the presence of minute quantities of foreign sulphurets or arseniurets, acting by the reduction of the chloride of silver.

The decomposing action of sulphurets is exerted proportionally on the bromide of silver, and it is but slightly appreciable on the iodide.

In these phenomena the solvent exerts no influence; for the same results are obtained, except as to time, by simple contact aided by water.

The general fact of the decomposition of insoluble chlorides by sulphurets appears then to render it probable that, in natural sulphurets, the silver is in the state neither of chloride nor bromide.

Having then shown the improbability of the presence of metallic silver or chloride in the natural argentiferous sulphurets, the authors are of opinion that it must exist in the state of sulphuret; but if this conclusion were correct, how does it happen that blende, pyrites and galena, do not yield silver to mercury? Is not the sulphuret of silver almost as readily acted upon by mercury as metallic silver itself? The authors propose shortly to communicate the second part of this inquiry to the Academy.—*Comptes Rendus*, Juillet 26, 1847.

VANADIATE OF LEAD AND COPPER.

M. Dufrénoy presented to the Academy, in the name of M. Domeyko, Professor of Chemistry and Mineralogy in the college of San Yago, Chili, an account of this new mineral, which is composed of—

Oxide of lead .....	54·9
Oxide of copper.....	14·6
Vanadic acid.....	13·5
Arsenic acid .....	4·6
Phosphoric acid.....	0·6
Chloride of lead .....	0·3
	<hr/>
	88·5

*Comptes Rendus*, Mai 5, 1847.

METEOROLOGICAL OBSERVATIONS FOR AUG. 1847.

*Chiswick*.—August 1, 2. Very fine: sultry. 3. Very fine: clear. 4. Very fine: densely overcast. 5. Rain. 6. Overcast. 7. Very fine. 8. Very fine: cloudy. 9. Cloudy: shower: clear. 10. Rain: showery. 11. Very fine. 12. Light clouds, with bright sun at intervals: clear at night. 13. Overcast: very fine. 14. Very fine: cloudy. 15. Cloudy: clear: lightning at night. 16. Rain. 17. Overcast. 18. Heavy rain. 19. Overcast: lightning at night. 20. Uniformly overcast: slight fog. 21. Slight fog: fine. 22. Overcast: rain: cloudy. 23. Cloudy: rain. 24. Cloudy: clear at night. 25. Very fine. 26. Overcast: very fine. 27, 28. Very fine. 29. Rain: very fine. 30. Very fine: cloudy. 31. Very fine: clear at night.

Mean temperature of the month .....	62°·68
Mean temperature of Aug. 1846 .....	64 ·16
Mean temperature of Aug. for the last twenty years .....	62 ·32
Average amount of rain in Aug. ....	2·41 inches.

*Boston*.—Aug. 1. Fine: 2 o'clock p.m. thermometer 83°. 2. Fine: rain p.m. 3, 4. Fine. 5. Cloudy: rain p.m. 6. Fine. 7. Fine: rain p.m. 8. Fine. 9, 10. Cloudy. 11. Cloudy: rain early a.m. 12. Cloudy. 13, 14. Fine. 15. Cloudy. 16. Cloudy: rain a.m. and p.m. 17. Cloudy: rain p.m. 18, 19. Cloudy. 20—25. Fine. 26. Cloudy. 27. Fine. 28. Rain. 29. Cloudy: rain early a.m.: rain p.m. 30, 31. Cloudy.

*Sandwich Manse, Orkney*.—Aug. 1, 2. Bright: clear. 3. Bright: cloudy. 4. Cloudy: drops. 5. Bright: cloudy. 6. Cloudy: fine. 7. Rain: fine. 8. Cloudy: rain. 9. Cloudy: fine. 10. Cloudy: rain. 11. Clear: showers. 12. Cloudy. 13. Clear: cloudy. 14. Cloudy: fine. 15. Bright: fine. 16, 17. Clear: fine. 18. Cloudy: fine. 19, 20. Cloudy. 21. Showers: rain. 22. Cloudy: showers. 23. Clear: showers: cloudy. 24. Cloudy: rain. 25. Cloudy. 26. Cloudy: rain. 27. Cloudy: clear. 28. Bright: showers: clear. 29. Showers. 30. Rain: showers. 31. Bright: rain.

*Applegarth Manse, Dumfries-shire*.—Aug. 1. Fair, but cloudy. 2. Fair and fine: shower early a.m. 3. One slight shower. 4. Rain early a.m. 5. Rain nearly all day. 6. Frequent showers. 7. Heavy showers and sun. 8. Rain. 9. Cloudy: cool: dry. 10. Heavy rain. 11. Fine a.m.: rain p.m. 12. Rain nearly all day. 13. Fair and fine. 14. Very fine. 15, 16. Very fine: heavy dew. 17. Fine, though cloudy. 18. Very fine. 19. Still fine, but dull. 20. Heavy showers. 21. Slight showers. 22, 23. Fine: clear. 24. Rain p.m. 25, 26. Fine, though cloudy. 27. Fine, though cloudy: a few drops. 28. Fine, though cloudy: one slight shower. 29. Fair and fine. 30. Fine: one slight shower. 31. Fine harvest day.

Mean temperature of the month .....	57°·15
Mean temperature of Aug. 1846 .....	61 ·2
Mean temperature of Aug. for twenty-five years.....	57 ·14
Average rain for twenty years .....	3·16 inches.

Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at Chiswick, near London; by Mr. Veall, at Boston; by the Rev. W. Dunbar, at Applegarth Manse, Dumfries-shire; and by the Rev. C. Clouston, at Sandwick Manse, Orkney.

Days of Month.	Barometer.						Thermometer.				Wind.				Rain.				
	Chiswick.		Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Dumfries-shire.		Orkney, Sandwick.		
	Max.	Min.	8 1/2 a.m.	9 a.m.	9 p.m.	8 1/2 p.m.	Max.	Min.	8 1/2 a.m.	Max.	Min.	8 1/2 a.m.	Max.	Min.	8 1/2 a.m.	Max.	Min.	8 1/2 a.m.	
1847. Aug.																			
1.	30.018	29.944	29.50	29.82	29.81	29.71	29.74	46	91 1/2	70	59	63	55	calm	ssw.	calm	.....	.....	ws.
2.	30.044	29.827	29.36	29.80	29.85	29.81	29.86	46	89	73	52 1/2	56	55	w.	ssw.	w.	.....	.....	calm
3.	30.009	30.003	29.48	29.86	29.88	29.82	29.82	38	77	62	41 1/2	55 1/2	53	n.	w.	n.	.....	.....	w.
4.	29.945	29.741	29.47	29.70	29.50	29.70	29.56	51	80	63.5	44	57	55	calm	s.	calm	.....	.....	se.
5.	29.640	29.504	29.17	29.41	29.39	29.44	29.47	54	72	59	51 1/2	58	56	s.	ssw.	s.	.....	.....	se.
6.	29.733	29.505	29.10	29.49	29.63	29.62	29.66	47	69	63	50	59 1/2	56	nw.	ssw.	nw.	.....	.....	calm
7.	29.773	29.723	29.27	29.50	29.43	27.41	29.41	49	76	66	53	56 1/2	55	w.	s.	w.	.....	.....	calm
8.	29.731	29.700	29.20	29.25	29.44	29.53	29.68	49	76	63	51	57	49 1/2	ws.	ssw.	ws.	.....	.....	n.
9.	29.876	29.751	29.23	29.61	29.63	29.72	29.69	44	66	58	49	52 1/2	53	ss.	ssw.	ss.	.....	.....	calm
10.	29.881	29.808	29.40	29.60	29.63	29.58	29.60	48	65	55	43	55 1/2	53	ss.	ssw.	ss.	.....	.....	w.
11.	30.112	30.072	29.57	29.85	29.80	29.82	29.76	61	79	59.5	46	59	56	ss.	ssw.	ss.	.....	.....	s.
12.	30.110	30.088	29.54	29.78	29.94	29.69	29.83	58	86	68	56	59 1/2	56	ss.	ssw.	ss.	.....	.....	ws.
13.	30.253	30.189	29.68	30.14	30.28	30.11	30.27	49	81	68	44 1/2	57	54	n.	w.	n.	.....	.....	ws.
14.	30.316	30.276	29.84	30.32	30.30	30.37	30.40	52	79	65	45 1/2	57 1/2	54	ne.	s—e.	ne.	.....	.....	s.
15.	30.222	30.163	29.78	30.31	30.24	30.35	30.32	56	80	66	46 1/2	59	56	ne.	e.	ne.	.....	.....	s.
16.	30.088	30.040	29.67	30.21	30.15	30.24	30.17	61	68	63	46 1/2	59	56	n.	ne.	n.	.....	.....	w.
17.	30.044	30.040	29.58	30.09	30.18	30.27	30.31	60	71	63	46	56	52 1/2	n.	ne.	n.	.....	.....	w.
18.	30.101	30.082	29.63	30.20	30.18	30.32	30.26	62	85	65	55	58 1/2	55	nne.	n—s.	nne.	.....	.....	w.
19.	30.119	30.016	29.67	30.14	30.01	30.14	29.95	55	69	64	47 1/2	56 1/2	57	n.	ssw.	n.	.....	.....	ws.
20.	29.971	29.895	29.54	29.90	29.80	29.75	29.68	49	72	65.5	46	56 1/2	53	calm	ssw.	calm	.....	.....	w.
21.	29.911	29.749	29.43	29.78	29.40	29.72	29.31	54	82	66.5	49	55	54	e.	ssw.	e.	.....	.....	ese.
22.	29.763	29.666	29.17	29.34	29.66	29.06	29.66	43	68	66	51	55 1/2	46	calm	w.	calm	.....	.....	n.
23.	30.060	29.886	29.48	29.97	30.05	29.98	30.07	46	61	56	41	51	50	calm	ssw.	calm	.....	.....	w.
24.	30.124	30.075	29.70	30.10	30.07	30.07	29.96	49	65	59	37 1/2	56	54	n.	w.	n.	.....	.....	se.
25.	30.113	30.071	29.68	29.95	30.02	30.00	30.07	50	74	59	50	56 1/2	55	calm	ssw.	calm	.....	.....	w.
26.	30.227	30.146	29.69	30.10	30.12	30.11	30.06	43	72	62	48	60	57	calm	ssw.	calm	.....	.....	calm
27.	30.297	30.254	29.75	30.11	30.11	29.97	30.01	46	81	67	56 1/2	63 1/2	56 1/2	calm	ssw.	calm	.....	.....	ws.
28.	30.305	30.197	29.77	30.20	30.12	30.09	30.10	55	81	63	49	57 1/2	54	calm	ssw.	calm	.....	.....	w.
29.	30.145	30.097	29.64	30.10	30.09	30.07	29.98	44	75	62	50 1/2	55	54	e.	ssw.	e.	.....	.....	w.
30.	30.065	29.976	29.49	29.90	29.78	29.80	29.73	53	69	57	51	55	54	calm	nw.	calm	.....	.....	w.
31.	30.009	29.951	29.48	29.88	29.83	29.81	29.56	42	67	61.5	51	54 1/2	53	nw.	w.	nw.	.....	.....	s.
Mean.	30.064	29.949	29.54	29.844	29.881	29.875	29.869	50.32	75.04	63.1	50.1	57.00	54.00			1.50	1.46	2.23	3.20

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LI. *Researches on the Voltaic Arc, and on the influence which Magnetism exerts both on this Arc and on bodies transmitting interrupted Electric Currents.* By M. AUGUSTE DE LA RIVE, Professor in the Academy of Geneva, Foreign Member of the Royal Society, Corresponding Member of the Academy of Sciences at Paris, &c.\*

THE luminous voltaic arc occurring between two conducting bodies, each communicating with one of the poles of the pile, is not merely one of the most brilliant phænomena in physics, but, from the numerous aspects under which it may be regarded, it is also one of the most important.

As a source of light, this phænomenon, when exhibited in a vacuum, enables us to examine what influence this particular origin of the light employed may have in various optical experiments. Compared with the solar light, the light of the voltaic arc presents some curious differences and also resemblances. If, on the one hand, we find in it the seven coloured rays of the spectrum, on the other the black streaks are replaced by brilliant ones, and these are differently interspaced. In this field of inquiry, much, or rather all, yet remains to be investigated.

As a source of heat, the voltaic arc enables us to study the fusion and solidification of even the most refractory bodies *in vacuo*, and consequently under circumstances exempting them from oxidizing action and other chemical influences, which usually result from the application of a high temperature in atmospheric air. It likewise allows us to determine the effects produced upon bodies at a high temperature, by various gases or vapours, distinct from those which enter into the composition of atmospheric air, and at different degrees of density.

As an electro-chemical power, the voltaic arc may be ap-

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plied so as to submit to the electrolyzing action of the electric current gaseous media, which, from some experiments already made, appear capable of decomposition by this process.

As a mechanical power, the voltaic arc, by bringing bodies into a state of minute division, and impressing upon them, in this state, a tendency to motion, places them in a favourable condition for the study of their molecular constitution, and of the relations which connect this constitution with electricity and magnetism. The struggle that takes place between cohesion and the expansive force of the electric current, the reduction of matter to the molecular state, and the form and nature of the deposits resulting therefrom, are so many phænomena capable of throwing light on the obscure subject of molecular physics.

The few preceding remarks suffice to give some idea of the extent of an investigation embracing the whole range of experimental research on the voltaic arc under its various aspects, which I am far from pretending to have attempted. I shall confine myself at present to a few details, and especially to such as exhibit the action of magnetism on the voltaic arc, and on those bodies which transmit interrupted currents. I shall begin by describing some particular phænomena which I observed during my study of the voltaic arc under various circumstances, while employing different substances as electrodes, both in the air and in a vacuum; I shall then proceed to examine the action of a powerful electro-magnet on this voltaic arc, and I shall conclude by describing some remarkable experiments also illustrating the influence of magnetism on conducting bodies, of whatever nature, traversed by interrupted currents.

### § 1. *Some Phænomena concerning the Voltaic Arc.*

Davy was the first who produced the phænomenon of the voltaic arc with two points of charcoal. More recently, Messrs. Grove\* and Daniell† employed with success the points of different metals, and arrived at interesting results: I also published some experiments I made on the voltaic arc‡ in 1841. Subsequently, MM. Fizeau and Foucault observed some remarkable facts of the same kind on the occasion of an investigation into the intensity of the light emitted by charcoal in the experiment of Davy§. The researches made up to the present time, have already led to many results, of which I shall consider only the most important.

1. That the voltaic arc may be produced, a pile of greater tension is required than that which is necessary for the ordi-

\* *Bibl. Univ.* June 1840, i. 27. p. 387. † *Arch. de l'Elect.* tom. i. p. 462.

‡ *Arch. de l'Elect.* tom. i. p. 262.

§ *Ibid.* tom. iv. p. 311.

nary calorific and electro-chemical phænomena. The necessity of this condition proves the great resistance presented to the passage of the electric current by the minutely divided matter, whatever it may be, which connects the two poles.

2. The luminous arc cannot exist, unless contact be previously made between the electrodes, and unless these, or at least one of them, be terminated at the point of contact by points fine enough to produce in them an increase of temperature. When this increased temperature is once produced, we may, by separating the electrodes gradually and with precaution from each other, obtain the luminous arc, the length of which will depend on the intensity of the pile. Daniell discovered the important fact, which was confirmed by M. Van Breda in a very recent investigation inserted in the *Comptes Rendus de l'Académie\**, that without contact having taken place, the luminous arc may be produced between two electrodes placed very near together, by causing the discharge of a Leyden jar to pass between them: this is owing to the discharge being always attended by the transference of highly diffused matter, which closes the circuit during the instant of time necessary for the formation of the arc.

3. The enormous elevation of temperature which accompanies the production of the luminous arc, is also manifested in the electrodes, especially in the positive ones, which become much more strongly heated than the negative.

4. Matter is thus transported from the positive electrode to the negative, a fact which may be verified with electrodes of all kinds, but particularly with those of charcoal.

5. The various phænomena presented by the voltaic arc, are modified to a greater or less extent by the nature of the electrodes and by that of the surrounding medium. Thus Mr. Grove adduces facts from which it appears that the presence of oxygen is necessary in most cases to produce a very luminous and brilliant arc. It results also from his experiments, as well as those of other philosophers, that when two different substances are made use of for the electrodes, it is not a matter of indifference which of the two is placed at the positive pole.

I now proceed to my own researches. I commenced by studying the production of a luminous arc between a plate and a point of the same material in air, and *in vacuo*. By means of a micrometer screw I was able to make the point recede from the plate very gradually, and judge of their mutual distance with great precision. The limit of distance beyond which the luminous arc ceases to appear, is constant for the

[\* See also p. 538 of the December Number of this Journal for 1846.]

same plate and the same point: when, however, the plate communicates with the positive pole, it is in general double that which it is when the point communicates with the same pole. But in proportion as the strength of the pile is greater, the difference is so much the smaller.

With respect to the absolute amount of this distance, it is very variable, depending on the strength of the pile, on the nature and molecular state of the electrodes, and on the time occupied in the experiment. Thus, with a Grove battery composed of fifty pairs of plates sixteen square inches in surface, it is two or three times greater than with a pile of seventy elements of two or three square inches. With metals easily fused or oxidized, as zinc and iron, it is much greater than with platinum or silver. The duration of the phenomenon influences the result, inasmuch as the high temperature of the electrodes allows them to be drawn asunder to a greater distance without breaking the arc. The same effect may be produced by heating them artificially, by means of a spirit-lamp. It is evident from what I have said that the length of the luminous arc has a relation to the greater or less facility which the substances composing the electrodes possess of being segregated, a facility which may depend upon their temperature diminishing their cohesion, upon their tendency to oxidize (which produces the same effect), upon their molecular state, and lastly upon their peculiar nature. Carbon derives from its molecular constitution, which renders it so friable, the property of being one of the substances which produces the longest luminous arc.

The deposits of the transported matter, form upon the plate, when it is negative and the point positive, a species of very regular ring, the centre of which is the projection of the point upon the plate. This takes place equally, whether the plate be vertical or horizontal, plainly indicating a determinate direction in the transfer of the substance from the positive to the negative electrode; in the air and with metallic electrodes, the deposits always consist of the oxidized dust of the metal, of which the positive electrode is composed.

I shall here enter into some details. A plate and a point of platinum have been used as electrodes in a vacuum, in air and in hydrogen. In a vacuum with a Grove battery of fifty pairs of plates, which had previously been used, I had only a very feeble effect, and particularly when the plate served as the positive electrode. The point was hardly removed a millimetre\* from the plate when the arc broke; to re-establish it, it became necessary to renew the contact between the point

\* 1 millimetre = 0.03937 inch.—*Trans.*

and the plate, by touching another point of the plate, the first point which was touched appearing to have undergone such a modification as to prevent the re-formation of the arc. The same effect is produced when the experiments are made in the air, but it ceases when the power of the battery is increased: this is probably due to an augmentation of cohesion consequent on the increase of temperature in that part of the plate which acts as the positive electrode. Besides, when the experiment is made in air, the voltaic arc is more marked and of greater length than when it is made *in vacuo*, at least if the battery be weak; for when the battery is powerful, composed, for example, of fifty pairs of plates freshly charged, it appeared to me that the contrary obtained. I did not, however, perceive any great difference; but the vacuum in which I experimented was far from being perfect; it was that of a pneumatic pump, enclosing therefore highly rarefied air.

In the latter case, that is to say, with the pile composed of fifty pairs strongly charged, and in highly rarefied air, a bluish spot, perfectly circular and presenting the appearance of a coloured ring of Nobili, was formed on the plate of platinum when it served as the positive electrode. The same spot appeared in atmospheric air, but its diameter was one-half less, and its colours much less vivid. In hydrogen, no coloured spot was formed; its formation is therefore evidently the result of the oxidation of the platinum at a high temperature when acting as a positive electrode in the ordinary atmosphere, and still more so, perhaps, in rarefied air\*. When the same plate of platinum was made use of as a negative electrode, the point being positive, it became covered with a white circular spot, formed of a vast number of minute grains of platinum, which, having been raised to a high temperature, remained adhering to the surface. The white spot, like the blue one, was much larger in rarefied air than in a vacuum. If the experiment be prolonged for a minute or two when the plate is negative, the rod of platinum terminating in a point, which is positive, soon becomes highly incandescent; its end is fused and falls on the plate in the form of a perfectly spherical globule. When the plate is positive and the point negative, the latter is less heated, and does not become fused; but the plate, unless it be very thick, is liable to be perforated: besides, as may easily be

\* This effect may possibly have been owing to the action of the oxygen brought by the voltaic current into that particular state which Schönbein first described under the name of *ozone*. Indeed, in this state the oxygen may attack those metals which are supposed to be inoxidizable; and M. Marignac and I have shown that this may be effected by causing a succession of electric discharges to pass through the oxygen, even when very dry, with which the phenomenon of the voltaic arc has a great resemblance.



imagined, the phænomenon lasts much longer in the latter case. The light is less brilliant, but it is accompanied by a reflexion of a superb blue, which may be seen when the experiment is made in the interior of a bell, whether the air be rarefied or not. This blue reflexion is observed on the side of the bell, and is to be seen whatever may be the nature of the electrodes, or the colour of the light to which these give rise in the centre of the bell; only when this central light is very brilliant, it becomes slightly paler by the effect of contrast.

I substituted for the platinum point a point of coke, but the plate of platinum remained; this being positive and the point negative, I obtained a luminous arc more than double the length of the arc produced by the point of platinum. With respect to the arc, instead of its being a cone of light, having its base on the plate and its apex at the point, as was the case when the latter was platinum, it was composed of a multitude of luminous jets diverging from different points of the plate, and tending to various parts of the point of coke. This fact shows clearly the influence that may be exercised by the negative electrode, the function of which is very far from being a merely passive one. Let me add, that although the strength of the pile was precisely the same as when the point was of platinum, not only was the luminous arc much longer with the point of coke, but the heat developed in the plate of platinum was so much greater that it was soon melted and perforated. The coke being positive and the plate negative, the length of the arc was less than in the preceding case, and particularly so in air, where it was sensibly less than in a vacuum. The heat generated was however still very great, the point of coke becoming quickly incandescent throughout. I ought to add, that with the point of coke, the luminous arc was so brilliant that the blue light which I have mentioned almost entirely disappeared, which was not the case with any other kind of point.

Leaving the plate of platinum, I adjusted a zinc point. The effects were most brilliant, but of short duration, the point speedily melting. In common air, a deposit of white oxide was precipitated upon the platinum plate; in highly rarefied air (the vacuum of an air-pump), a black deposit was formed: in both cases it communicated with the positive pole. An iron point being substituted for that of zinc, equally produced in common air a brownish-red deposit of oxide of iron, and in rarefied air a deposit of black oxide.

I call the attention of chemists to these two facts, as well as that of the oxidation of the platinum at a high temperature in rarefied air. They appear to prove the influence which the state of greater or less density of the surrounding oxygen may

exert on the phænomenon of oxidation and on the nature of the oxide formed. A plate and a point of soft iron were used as positive and negative electrodes, both in a vacuum and in the atmosphere; the same results appeared with a plate and a point of silver, a plate and a point of copper, and a plate and point of argentane\*. The blue light was perceived in all the experiments; coloured circles were likewise seen on all the plates when they had acted as positive electrodes in rarefied air. The silver and copper plates presented in this case very decided cavities, caused by the passage of the matter from the positive to the negative pole. The points became incandescent throughout when they served as positive electrodes; whereas when negative, they were heated only at their extremities. The copper point when positive became isolating at its extremity, and it was necessary to excite it by friction in order to renew the experiment. This circumstance is probably attributable to the formation of a thin film of oxide. The point and plate of copper gave out a luminous arc of a beautiful green light, which contrasted in a remarkable manner with the blue reflexion visible in this, as in the other experiments. Mercury was likewise employed, both as a positive and negative electrode. In a vacuum as well as in atmospheric air, the luminous effect was most brilliant. The mercury was excessively agitated, rising up in the form of a cone when it was positive, and sinking considerably below the positive point when it was negative. The quantity of vapour thrown off by the mercury during this experiment filled the bell so quickly that it was not easy to observe the details.

I shall terminate this section by stating a fact which appears to me to be important; it is the influence which the nature of the metallic points forming the electrodes exercises on the temperature which they acquire in relation to the production of the voltaic arc. If the two points are of the same metal, both platinum, or both silver, the positive one alone becomes incandescent throughout its whole length. If the silver point be positive and that of the platinum negative, the latter becomes incandescent, and the silver one is much less heated. Thus, when the voltaic arc is formed, the circuit must be regarded as completed, and then it is those parts of the circuit which present the greatest resistance to the current which become the hottest; at first it is that portion forming the arc itself, and then, in the rest of the circuit, the metal which is the worst conductor. But if the conductors be of the same material on both sides of the arc, or if there be only a slight

\* An alloy of copper and nickel: also known by the names of *pachfong* and *melchior*.

difference of conductibility between them, then the development of heat, instead of being uniform, as it might appear it ought to be, is much greater on the positive side. This important fact evidently proves that this portion of the circuit has to resist a much more energetic action than that which the other side experiences; a fact which is confirmed by the molecular segregation accompanying this action at the positive electrode. This want of resemblance in the phænomena presented by the two electrodes, although placed in conditions entirely symmetrical, deserves to be taken into serious consideration, for it may throw light upon the nature of the electric current, and upon the link which unites it with the molecular state of the bodies through which it is transmitted.

### § 2. *Influence of Magnetism on the Voltaic Arc.*

Davy was the first who observed that a powerful magnet acts upon the voltaic arc as upon a moveable conductor, traversed by an electric current; it attracts and repels it, and this repulsion and attraction manifests itself by a change in the form of the arc. Even the action of the magnet may, as I have found, break the arc by too great an attraction or repulsion exerted upon it, causing the communication which the transmitted particles establish between the electrodes to cease.

The action which I have just mentioned is not the only one which magnetism exerts on the voltaic arc. I have already stated the curious fact, that if two points of soft iron acting as electrodes, be both placed within a helix formed of thick copper wire of several coils, the voltaic arc developed between the two points of iron ceases the moment a strong current is passed through the wire of the helices, and reappears if this current be arrested before the points have become cold. The arc cannot be formed between the two iron points when they are magnetized, whether by the action of the helices, or by that of a powerful magnet, unless they be brought much nearer to one another, and the appearance of the phænomenon is then entirely different. The transported particles appear to disengage themselves with difficulty from the positive electrode, sparks fly with noise in all directions, while in the former case it was a vivid light without sparks, and without noise, accompanied by the transfer of a liquid mass, and this appeared to be effected with the greatest ease. It is of little moment with respect to the result of the experiment, whether the two rods of magnetized iron present to that part of their extremities between which the luminous arc springs, the same magnetic poles or different poles.

The positive electrode of iron, when it is strongly magnet-

ized, produces, the moment that the voltaic arc is formed between it and a negative electrode of whatever nature, a very intense noise, analogous to the sharp hissing sound of steam issuing from a locomotive engine. This noise ceases simultaneously with the magnetization.

For the purpose of better analysing these different phenomena, I placed an electro-magnet of large dimensions and great power in such a manner as to enable me to place on each of its poles, or between them, different metals destined to form one of the electrodes of the pile, while one point of the same metal, or another substance, acted as the other electrode. I have alike employed as electrodes, placing them in the same circumstances, two points of the same metal or of different metals. The following are the results which I have obtained. A plate of platinum was placed on one of the poles of the electro-magnet, and a point of the same metal was placed vertically above it; the voltaic arc was produced between the plate and the point, the plate being positive and the point negative. As soon as the electro-magnet was charged, a sharp hissing was heard; it became necessary to bring the point of the plate nearer to enable the arc to continue, and the bluish circular spot which the platinum plate presented, became larger than when the experiment was made beyond the influence of the electro-magnet. The plate was made negative, and the point positive; the effect was then totally different; the luminous arc no longer maintained its vertical direction when the electro-magnet was charged, but took an oblique direction, as if it had been projected outwards towards the margin of the plate; it was broken incessantly, each time accompanied by a sharp and sudden noise, similar to the discharge of a Leyden jar. The direction in which the luminous arc is projected, depends upon the direction of the current producing it, as likewise on the position of the plate on one or other of the two poles, or between the poles of the electro-magnet. A plate and a point of silver, a plate and a point of copper, and generally a plate and a point of any other metal, provided it be not metal too easily fused, present the same phenomena.

Copper, and still more silver, present a remarkable peculiarity. Plates of these two metals retain on their surfaces the impression of the action that took place in the experiments just described. Thus, when the plate is positive, that portion of its surface lying beneath the negative point presents a spot in the form of a helix; as if the metal melted in this locality had undergone a gyratory motion around a centre, at the same time that it was uplifted in the shape of a cone towards the



point. Moreover, the curve of the helix is fringed throughout by minute ramifications, precisely similar to the tufts which mark the passage of positive electricity in a Leyden jar. When the plate is negative and the point positive, the marks are totally different, being merely a simple point, or rather a circle of a very small diameter, whence proceeds a line more or less curved, forming a kind of tail to the comet, of which the small circle might be the nucleus: the direction of this tail depends upon the direction in which the luminous arc has been projected.

When, instead of a plate and a point, two points are used for electrodes, it is evident that no visible trace of this phenomenon can be obtained; but both the sharp hissing and the detonations may be produced, which latter are sometimes so loud as to bear a resemblance to distant discharges of musketry. For this the electro-magnet must be very powerful, and the current which produces the arc very intense. I had observed that when I took for a positive electrode a point of platinum, and for a negative electrode a point of copper, and placed them between the two poles of the electro-magnet, the production of the voltaic arc between the two poles was accompanied by a sharp hissing noise; whereas in the opposite case, the copper being positive, and the platinum negative, the detonations were heard, attended by a frequent breaking of the arc. On examining this phenomenon more closely, I perceived that the fact I have just mentioned was due to the platinum becoming heated much more rapidly than the copper when they were employed as electrodes in producing the voltaic arc; and I have satisfied myself that in order to obtain the hissing sounds, it is necessary that the positive electrode should be at a sufficiently high temperature to experience a commencement of liquefaction; for without this condition, only a series of detonations are heard. The hissing would be the result of the easy and continuous transport of matter more or less liquefied from the positive electrode, whilst the detonations would be the effect of the resistance opposed by the same matter to the disintegration of its particles when it is not sufficiently heated. Numerous experiments made with metallic points, whether of the same or different natures, as silver, iron, brass, as also platinum and copper, some of which become heated sooner than others under the same circumstances, have quite confirmed me in this view of the subject. It is merely necessary to be careful, in order to produce the hissing noise, to maintain as much as possible the continuity of the arc when once the positive electrode becomes incandescent; while, on the other hand, to obtain the detonations, one of the

electrodes must be held in the hand, and then the arc frequently made and broken without waiting till the metallic points acquire too high a temperature.

It remains now to be considered why the influence of powerful magnetism, such as that exerted by the electro-magnet, is necessary for the production of these sounds, which are not heard in the ordinary experiment of the voltaic arc. This can arise only from the change which the magnet produces in the molecular constitution of the matter of the electrode, or rather in the highly diffused matter which forms the voltaic arc. This action is besides shown by the shortening of the arc, and by the remarkable difference which it presents in its appearance; it is therefore not surprising that it should also be capable of producing a phænomenon such as sound, which essentially depends on the variations in the molecular state of bodies. This view of the subject appears to me to deserve very particular attention: the results at which I have arrived, in pursuing it more closely, form the subject of the following section.

§ 3. *Influence of the permanent action of Magnetism on conducting bodies traversed by interrupted electric currents.*

Faraday's brilliant discovery of the action exerted by magnetism on a ray of polarized light, when that ray traverses a transparent body submitted to the action of a powerful electro-magnet, had no sooner been announced by its illustrious author, than the majority of philosophers saw in it a proof that magnetism, when at a high degree of intensity, has power to modify the molecular constitution of all bodies. They consequently attributed the phænomenon observed by Faraday, not to the direct action of the electro-magnet on the polarized ray, but to the modification effected by this action on the molecular constitution of the substance traversed by the ray. I was of this opinion, and communicated it to Mr. Faraday, who alludes to it in his memoir. Desirous, however, of founding this opinion on facts of a different kind, I asked myself if it were not possible to find in the electric current, an agent capable of performing the same function for opaque conducting bodies that polarized light does for transparent ones. I had stated in my paper on the sound emitted by iron wires traversed by interrupted electric currents, that the nature as well as the intensity of the sounds were singularly modified by the molecular state of the wire submitted to the experiment. I had particularly mentioned the influence of temper and annealing, of greater or less tension, and of temperature. I had shown that iron wire, when under the influence of an action which renders it magnetic, does not emit the same sound as

when it is in its natural state. Finally, by modifying, through the agency of heat, the molecular arrangement of some metals, such as platinum and brass, I had succeeded in obtaining from them, during the passage of the interrupted current, sounds, which, though feeble, were yet distinct.

The preceding reflexions tended to confirm me in my opinion, that sounds produced under the influence of magnetism in the experiments on the voltaic arc, are owing to a molecular modification effected by the action of the magnet, and the more so inasmuch as the voltaic arc may be regarded as produced by a succession of interrupted currents, following each other with extreme rapidity, rather than by a perfectly continuous current. I accordingly took bars of other metals besides iron, as of tin, zinc, lead, bismuth, &c. I placed them on the poles of the electro-magnet, and I caused an interrupted current from a Grove's battery of from five to ten pairs to traverse them. They emitted no sound as long as the electro-magnet was not magnetized; but as soon as it was, sounds were very distinctly heard, composed of a series of strokes corresponding to the interruptions of the current, and analogous to that produced by a toothed wheel. The bars were eighteen inches long, and from nine to ten lines square. Rods of copper, platinum, and silver produced a similar effect; a rod of iron did not emit a much louder sound under the influence of the magnet than it did when not exposed to this action.

What appeared to me most remarkable was, to find lead, a body so little elastic, yield a sound as powerful as those proceeding from the other metals, when placed under the same circumstances. The position of metallic bars with respect to the poles of the electro-magnet did not in any way modify the result of the experiment; they might be placed axially, that is to say, in the direction of the poles, or equatorially, that is, across the poles; the effect remained the same, being merely weakened as the distance between the bar and the poles increased. In order to hear the sound distinctly, when not very powerful, it was sufficient to establish a communication between the metallic bar and the ear by means of a wooden rod. In this manner the sound was not unfrequently heard prolonged some seconds, though growing constantly feebler, until it ceased entirely, after the source of magnetism had been withdrawn from the electro-magnet. Mr. Faraday has remarked an analogous fact in the action of the transparent medium on the polarized ray, an action which does not cease immediately with the magnetism of the electro-magnet. Is this prolongation owing to the magnetization of the electro-magnet not ceasing in a sudden manner; or to its return to its primitive

molecular state not taking place instantaneously in the substance submitted to its action? This question I am unable to decide. I incline, however, rather to the latter of these explanations, seeing that the effect is not equally perceptible in all bodies, and that it is, for example, more sensible in a bar of bismuth than in one of copper.

It is needless to remark that the calorific action of the current could not have any influence on the production of the phenomenon, since there could have been no development of heat, on account of the dimension of the bars compared with the force of the current. Besides, if the expansion arising from the heating of the body traversed by interrupted currents had caused the sound, the effect would have been produced equally, whether the bar had been under the influence of the magnet or not. This last remark applies equally to the following experiments, as to the preceding.

The intensity of the sound appears to depend much less on the nature of the substance submitted to the experiment, than on its form, its volume, and its mass. Tubes of platinum, of copper, and of zinc, emitted more marked sounds than massive cylinders of the same metals. I wound a leaden wire in the form of a helix round a cylinder of wood; I did the same with a very fine platinum wire, and also with copper, zinc, and tin wires, taking care to place the coils of the helices so far apart that each should be isolated. Placed like bars and tubes, whether in the direction of, or across the poles of the electro-magnet, these helices emitted very powerful sounds when, the electro-magnet being charged, they were traversed by the interrupted current. It was particularly surprising to hear the lead wire emit so strong a sound. A helix constructed with copper wire, covered with silk, and composed of several coils wound round each other, emitted a very intense sound; it also emitted one, but much feebler, under the action of the electro-magnet.

It is almost needless to remark, that in all these experiments an ordinary magnet produces the same effect as an electro-magnet. But what is more interesting, is to replace the action of the electro-magnet by that of a helix traversed by a strong continuous current, in the axis of which helix is placed the bar, the tube or the coiled wire, through which the interrupted current is transmitted. Experiments have shown me that in this case the results are the same; the intensity of the sounds is not very different, especially when tubes and wires coiled as helices are used.

If, between the exterior helix and the metal submitted to the action, a tube of soft iron is placed, the effect is a little



heightened: it is neither increased nor lessened when the tube is of copper, only in this case another sound is heard which seems to proceed from the copper tube. This tube, however, is not traversed by a current, but it is probably acted upon by the currents of induction, which the interrupted currents traversing the conductor placed in the axis of the helix produce in it. I constructed a double helix formed of two thick copper wires covered with silk and coiled, each forming several circumvolutions, the one exterior to the other. In making a continuous current pass through the exterior wire, and an interrupted current through the interior one, I heard a remarkably intense sound. In the reverse case, the sound existed, but was much weaker. This fact is evidently connected with the known property of helices traversed by electric currents exercising scarcely any magnetic influence exteriorly, whilst in the interior this action is very energetic.

Metals and solid bodies are not the only substances which produce the phenomena I have just described; all conducting bodies, whatever may be their state or their nature, appear to be capable of producing them. Thus, I have observed them with pieces of charcoal of all kinds and shape. Mercury also produces them in a very marked manner. I have inclosed mercury in a tube of glass an inch in diameter, and ten inches long: the tube was completely full and closed with care, so that the mercury could have no motion. As soon as it was traversed by an interrupted current, transmitted by means of two platinum wires, and the electro-magnet or the helix was made to act upon it, a sound was heard remarkable for its intensity. When the mercury was placed in an open trough, instead of being inclosed in a tube, it likewise produced a sound, and in addition there was seen on its surface an agitation or vibratory motion, very different from the gyratory motion observed by Davy, which appears under the influence of the poles of a magnet when traversed by a continuous current.

Dilute sulphuric acid, and what is even better, salt water, were successively put in a capsule of platinum placed on one of the poles of an electro-magnet. A point of platinum immersed in the liquid, served, together with the capsule, to send an interrupted current through it. A sound was again heard, but less distinct, on account of the noise produced by the disengagement of the gas: still it was so clear that no doubt could be entertained of its existence.

It may perhaps be thought that in the experiments I have just described, the sounds are produced by the mechanical action of attraction or repulsion exerted by the electro-magnet

on the substance traversed by an interrupted current, and that, consequently, magnetism has no more share in the phenomenon than a finger might be supposed to have, when pressing on a sonorous cord. The simple description of the experiments shows this interpretation to be inadmissible. In the first place, the sound is the same with the wires in a helix, whether these wires be stretched or not, or whether they be of lead, platinum, or brass. Besides, how could this account for the sound produced in large masses, especially in liquids, such as mercury, and for the fact, that the position of the conductor traversed by the interrupted current with regard to the poles of the electro-magnet does not exert any influence on the phenomenon? Further, it must be remarked that the sound in question is not a musical sound, such as would be produced by a string or mass made to vibrate by a cause acting exteriorly at its surface; it is a series of sounds corresponding exactly to the alternations of the passage of the current; like a species of collision of the particles amongst themselves. Thus, the phenomenon is molecular; and it leads to the demonstration of two important principles.

The first principle is, that the passage of the electric current modifies, even in solid bodies, the arrangement of the particles; a principle which I have already deduced from the experiments contained in my preceding memoir on this subject. The second principle is, that the action of magnetism, under whatever form it may be exerted, modifies alike the molecular constitution of all bodies, and that this modification lasts as long as the cause producing it endures, and only ceases with it. What is the nature of these two modifications? This is what we must endeavour to investigate and to ascertain. I purpose to engage in this inquiry, and indeed I have already made some attempts of which it would, however, be premature to give any account. I shall confine myself at present to a single remark, which does not appear to me to be devoid of interest: it is, that the influence of magnetism on all conducting bodies seems to impress on them, as long as it lasts, a molecular constitution similar to that which iron, and generally all bodies susceptible of magnetism possess naturally; for it develops in them the property of producing, when traversed by interrupted currents, sounds identical with those emitted also by iron and other magnetic bodies when transmitting these currents, but produced in these last without requiring the action of a magnet.

LII. *Analyses of the Ashes of Rough Brown Sugar and Molasses.* By THOMAS RICHARDSON\*.

**D**URING some inquiries which I had occasion to make in the manufacture of an artificial manure for the sugar-cane, it was necessary to know the composition of the ash of coarse brown sugar and molasses as imported into this country. The results may be interesting to some of the readers of your Journal.

I. *Rough Brown Sugar.*

206·48 grs. in its ordinary state left 2·74 grs. ash = 1·33 per cent. 143·05 grs. of ash furnished 18·16 grs. silica, and 4·75 grs. carbonic acid.

28·61 grs. of ash furnished 8·43 grs.  $\text{SO}_3$  BaO = 2·89 grs. sulphuric acid.

28·61 grs. of ash furnished 0·19 gr. oxide of copper.

28·61 grs. of ash furnished 1·95 gr. peroxide of iron.

28·61 grs. of ash furnished 7·00 grs.  $\text{CO}_2$  CaO = 3·92 grs. lime.

28·61 grs. of ash furnished 7·81 grs.  $\text{PO}_5$  2MgO = 2·86 grs. magnesia.

16·12 grs. of ash furnished 7·36 grs.  $\text{Cl}_2$  Ag = 1·96 gr. chlorine.

28·61 grs. of ash furnished 28·61 grs. chlorides of alkalies, and this yielded 33·88 grs. of the double chloride of platinum and potassium = 10·34 grs. chloride of potassium = 6·53 grs. potash, leaving 4·16 grs. chloride of sodium = 2·20 grs. soda. The ash also contained a trace of oxide of manganese.

The result of the analysis is therefore—

Potash	. . . . .	22·84
Soda	. . . . .	7·69
Lime	. . . . .	13·69
Magnesia	. . . . .	10·00
Peroxide of iron	. . . . .	6·11
Oxide of copper	. . . . .	·66
Oxide of manganese	. . . . .	trace
Sulphuric acid	. . . . .	10·12
Silica	. . . . .	12·68
Carbonic acid	. . . . .	2·32
Chlorine	. . . . .	12·20
		<hr/>
		98·31

\* Communicated by the Author.

Omitting the carbonic acid and combining the chlorine with the sodium and potassium, we obtain the following composition in 100 parts:—

Potash . . . . .	19.42
Lime . . . . .	14.67
Magnesia . . . . .	10.72
Peroxide of iron . . . . .	6.55
Oxide of copper . . . . .	.71
Oxide of manganese . . . . .	trace
Chloride of potassium . . . . .	8.03
Chloride of sodium . . . . .	15.46
Sulphuric acid . . . . .	10.85
Silica . . . . .	13.59
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	100.00

## II. Molasses.

Great difficulty was experienced in incinerating the bulky charcoal mass left by boiling down the molasses. Part of the oxide of iron and sulphuric acid were decomposed; and this accounts for the excess in the analysis, as these substances were obviously in part twice estimated. 477.77 grs. left 17.21 grs. ash = 3.60 per cent.

64.27 grs. of ash furnished 6.46 grs. carbonic acid.

64.27 grs. of ash furnished 8.65 grs. charcoal, containing .55 gr. peroxide of iron and .26 gr. lime.

64.27 grs. of ash furnished 1.02 gr. silica.

21.423 grs. of ash furnished .28 gr. peroxide of iron.

21.423 grs. of ash furnished 3.83 grs.  $\text{CO}_2$   $\text{CaO} = 2.14$  grs. lime.

21.423 grs. of ash furnished 5.34 grs.  $\text{PO}_5$   $2\text{MgO} = 1.95$  gr. magnesia.

21.423 grs. of ash furnished chlorides of alkalis 14.91 grs., and this gave chlorides of platinum and potassium 32.88 grs.  $= \text{Cl}_2 \text{K}$  10.34 grs., leaving  $\text{Cl}_2 \text{Na}$  4.57 grs., = potash 6.53 and soda 2.42 grs.

12.62 grs. of ash furnished 2.38 grs.  $\text{SO}_3$   $\text{BaO} = .816$  gr. sulphuric acid.

18.405 grs. of ash furnished 9.92 grs.  $\text{Cl}_2 \text{Ag} = 2.453$  grs. chlorine.

The ash also contained traces of oxides of copper and manganese.

Collecting these results we obtain the following composition:  
*Phil. Mag. S. 3, Vol. 31, No. 209. Nov. 1847.* Z



Potash . . . . .	30·50
Soda . . . . .	11·30
Lime . . . . .	10·42
Magnesia . . . . .	9·13
Peroxide of iron . . . . .	2·15
Oxide of copper . . . . .	trace
Oxide of manganese . . . . .	trace
Sulphuric acid . . . . .	6·48
Chlorine . . . . .	13·33
Carbonic acid . . . . .	10·04
Silica . . . . .	1·58
Charcoal . . . . .	11·78
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	106·71

Omitting the charcoal and carbonic acid, and combining the chlorine as before, we have as follows:—

Potash . . . . .	36·23
Lime . . . . .	12·72
Magnesia . . . . .	11·14
Peroxide of iron . . . . .	2·62
Oxide of copper . . . . .	trace
Oxide of manganese . . . . .	trace
Chloride of potassium . . . . .	1·58
Chloride of sodium . . . . .	25·87
Sulphuric acid . . . . .	7·91
Silica . . . . .	1·93
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	100·00

The molasses and sugar came from the same manufactory, and were made from the same sugar-cane.

LIII. *Letter from Professor LOOMIS of the New York University to Lieut.-Colonel SABINE, Foreign Secretary of the Royal Society, on the determination of differences of Longitude made in the United States by means of the Electric Telegraph, and on projected observations for investigating the Laws of the great North American Storms.*

DEAR SIR,

New York University, Aug. 2, 1847.

I HAVE been for some time engaged upon a work in which you may perhaps feel some interest,—it is the exact determination of the difference of longitude between New York, Philadelphia and Washington, by means of the magnetic

telegraph. Morse's magnetic telegraph has been in operation between these places for a considerable time, and Prof. Bache proposed to use the line for the transmission of signals for the comparison of our local times for the use of the coast survey. Accordingly I erected a temporary observatory last season as near to this city as I conveniently could, and set up a transit instrument and clock. A wire was then carried from my observatory to the telegraph-office, thus connecting me with the regular Philadelphia line. A wire was also carried from the Philadelphia telegraph-office to the High School Observatory in Philadelphia, and another wire was carried from the Washington telegraph-office to the National Observatory. Thus three observatories, at New York, Philadelphia, and Washington, were in telegraphic communication; and having determined our local times by astronomical observations, we only needed some signal which could be heard simultaneously at the three places. This signal was afforded by the click of a magnet in the usual mode of telegraphic communication. Our plan of operation is as follows:—At ten in the evening, when the usual business of the telegraph company is concluded, our three observatories are put in communication with each other. After corresponding with each other long enough to ascertain that everything is in good order, New York commences giving clock signals. At the commencement of a minute by my clock I strike the key of my register, and a click is heard simultaneously at New York, Philadelphia, and Washington. The three observers record the time each by his own clock. At the expiration of  $10^s$  I give a similar signal, and all three record; after another  $10^s$  I do the same, and so on to twenty signals. Having waited one minute, Philadelphia repeats the same series of signals, and all record the time. We then wait another minute, and Washington repeats the same signals. Thus we have obtained sixty comparisons of our clocks, which will give our difference of longitude with as great accuracy as we can determine our local times.

In our first experiments we met with a great many disappointments, as might have been anticipated from the novelty and delicacy of the undertaking; but we have triumphed over them all. On *five* different nights we have transmitted good signals back and forth, and we propose to continue the comparisons until a further degree of accuracy is not to be expected. The errors of our clocks have not yet been rigorously computed, and we have not obtained final results; but we have made sufficient comparisons to know that the results of different nights agree remarkably well with each other. I

think the extreme discrepancy of different nights' work will amount to only a small fraction of a second. It appears to me that this mode of determining differences of longitude must supersede every other method between places which are connected by a telegraphic wire. The observations can be repeated indefinitely, and I think the longitude can be determined with a precision fully equal to that of the local times. I presume the same cannot be said of any other method yet practised. I have not heard of this method being tried in any part of Europe, though the application is very obvious. Can you inform me of any such trials?

In my former correspondence with you, and in my printed papers, I have more than once alluded to the importance of a combined movement in this country for meteorological observations. I am happy to say that the prospect of such a combination is brightening. You are probably aware that the Smithsonian Institution has been organized, and Prof. Joseph Henry has been placed at the head of it. The plan of organization is to appropriate \$15,000 a-year to the promotion of *original researches*. Prof. Henry is disposed to include in this plan a grand meteorological campaign, to continue for *three years*,—to cover the entire area of the United States with the greatest possible number of observers: we want 300, and I think they could be obtained. I have been drawing up a paper for Prof. Henry which will be placed before the Smithsonian Institution, and also before Congress the coming winter. I think the plan will be carried into execution, *provided* we can obtain the co-operation of the British Government. You know from the papers which I have sent you that our great storms frequently extend far to the northward of the United States. When the centre of a storm travels along the valley of the St. Lawrence, its margin often extends to the Gulf of Mexico. Observations spread over the entire United States would frequently include only *half* the area of a violent winter storm; and this is the class of storms from which most is to be expected, because their phænomena are most strongly developed. Unless therefore we could obtain simultaneous observations from the British possessions on the north of us, we should feel that our observations were deprived of more than half their value. Will you not see if the British Government and the Hudson's Bay Company cannot be induced to co-operate with us? What I propose is, that at every government station a register should be kept for a period of one, two, or three years. I should hope 100 such stations could be procured. The first cost of the instruments would not be

great, and the expense of observing probably nothing at all. If your government will co-operate, I think the Smithsonian Institution will undertake the organization for the United States.

With much respect I remain,  
Yours truly,  
ELIAS LOOMIS.

LIV. *On the Algebraic Equation of the Fifth Degree.*  
By the Rev. BRICE BRONWIN\*.

IT appears that the resolution of equations of the fifth and higher degrees into factors, one of which is of the second degree, depends upon the solution of the proposed equation itself. This circumstance appears to me deserving of notice, as it seems to indicate the impossibility of solving such equations in finite terms. Suppose

$$\left. \begin{aligned} x^5 + Ax^3 + Bx^2 + Cx + D &= (x^3 + ax^2 + bx + c) \\ (x^2 - ax + f) &= 0. \end{aligned} \right\} \quad (1.)$$

Multiplying the two factors, and comparing the result with the first member, we find

$$b - a^2 + f = A, \quad c - ab + af = B, \quad bf - ac = C, \quad cf = D.$$

Eliminating  $b$  and  $c$  from these, we have

$$\begin{aligned} 2af^2 - (a^3 + Aa + B)f + D &= 0 \\ f^3 - (a^2 + A)f^2 + Cf + Da &= 0. \end{aligned}$$

From these we easily deduce

$$\begin{aligned} f^2 - (3a^2 + A)f + a^4 + Aa^2 + Ba + C &= 0 \\ (a^3 + Aa - B)f^2 - (2Ca - D)f - 2Da^2 &= 0. \end{aligned}$$

Eliminating  $f^2$  by  $2af^2 = (a^3 + Aa + B)f - D$ , we shall have two equations, in which  $f$  will be only of the first degree; and then, by eliminating it from these, there results an equation in  $a$  of the tenth degree; and it is obvious that  $f$ ,  $c$ , and  $b$  will be determined from  $a$  by simple equations.

Now let  $a_1, a_2, \&c.$  be the roots of the equation in  $a$ , and  $x_1, x_2, \&c.$  those of (1.); then, since  $x^2 - ax + f = 0$  must contain two of the last, we shall have

$$\left. \begin{aligned} a_1 &= x_1 + x_2, & a_2 &= x_1 + x_3, & a_3 &= x_1 + x_4, & a_4 &= x_1 + x_5, \\ a_5 &= x_2 + x_3, & a_6 &= x_2 + x_4, & a_7 &= x_2 + x_5, & a_8 &= x_3 + x_4, \\ a_9 &= x_3 + x_5, & a_{10} &= x_4 + x_5. \end{aligned} \right\} \quad (2.)$$

\* Communicated by the Author.



To which we may add,

$$0 = x_1 + x_2 + x_3 + x_4 + x_5.$$

By eliminating  $x_1, x_2$ , &c. from these, we find

$$\begin{aligned} 3a_5 &= -2a_4 - 2a_3 + a_2 + a_1, & 3a_6 &= -2a_4 + a_3 - 2a_2 + a_1, \\ 3a_7 &= a_4 - 2a_3 - 2a_2 + a_1, & 3a_8 &= -2a_4 + a_3 + a_2 - 2a_1, \\ 3a_9 &= a_4 - 2a_3 + a_2 - 2a_1, & 3a_{10} &= a_4 + a_3 - 2a_2 - 2a_1, \end{aligned}$$

which may be verified by putting for  $a_1, a_2$ , &c. their values in  $x_1, x_2$ , &c. Therefore six of the roots  $a_1, a_2$ , &c. are linear functions of the remaining four, and the equation in  $a$  of the tenth degree is reducible to one of the fourth.

We also find

$$\left. \begin{aligned} x_1 &= \frac{1}{3}(a_1 + a_2 + a_3 + a_4), & x_2 &= \frac{1}{3}(2a_1 - a_2 - a_3 - a_4), \\ x_3 &= \frac{1}{3}(-a_1 + 2a_2 - a_3 - a_4), & x_4 &= \frac{1}{3}(-a_1 - a_2 + 2a_3 - a_4), \\ x_5 &= \frac{1}{3}(-a_1 - a_2 - a_3 + 2a_4). \end{aligned} \right\} (3.)$$

Now let the reduced equation in  $a$  be

$$a^4 + ma^3 + na^2 + pa + r = 0, \quad . \quad . \quad . \quad (4.)$$

the roots of which are  $a_1, a_2, a_3, a_4$ ; and therefore

$$-m = \Sigma(a_i), \quad n = \Sigma(a_1 a_2), \quad -p = \Sigma(a_1 a_2 a_3), \quad r = a_1 a_2 a_3 a_4.$$

Consequently,  $-m = 3x_1$  by (3.),

$$\begin{aligned} n &= 6x_1^2 + 2x_1(x_2 + x_3 + x_4 + x_5) + \Sigma(x_1 x_2) = 4x_1^2 + \Sigma(x_1 x_2) \\ &= \frac{4}{9} m^2 + A, \end{aligned}$$

because  $\Sigma(x_i) = 0$ ;

$$\begin{aligned} -p &= 4x_1^3 + 2x_1^2(x_2 + x_3 + x_4 + x_5) + x_1 \Sigma(x_1 x_2) + \Sigma(x_1 x_2 x_3) \\ &= 2x_1^3 + x_1 \Sigma(x_1 x_2) + \Sigma(x_1 x_2 x_3) = -\frac{2}{27} m^3 - \frac{1}{9} m A - B; \end{aligned}$$

$$r = x_1^4 + x_1^3 \Sigma(x_1 x_2) + \Sigma(x_1 x_2 x_3 x_4) = \frac{1}{81} m^4 + \frac{1}{9} m^2 A + C.$$

Hence  $n, p$ , and  $r$  are given in terms of  $m$ , and  $m = -3x_1$  can only be found by solving (1.); or the resolution of the proposed into factors, one of which is of the second degree, depends upon the solution of the proposed itself.

We may introduce fifth roots if we please; thus, let

$$\lambda^4 + g\lambda^3 + h\lambda^2 + k\lambda + l = 0, \quad . \quad . \quad . \quad (5.)$$

the roots being

$$\lambda_1 = a_1^5, \lambda_2 = a_2^5, \lambda_3 = a_3^5, \lambda_4 = a_4^5.$$

Here we shall find, as before, putting for  $a_1, a_2, \&c.$  their values in  $x_1, x_2, \&c.$ , that

$$\begin{aligned} -g = \Sigma(\lambda_1) = \Sigma(a_1^5) &= 4x_1^5 + 5x_1^4(x_2 + x_3 + x_4 + x_5) \\ &+ 10x_1^3(x_2^2 + x_3^2 + x_4^2 + x_5^2) + 10x_1^2(x_2^3 + x_3^3 + x_4^3 + x_5^3) \\ &+ 5x_1(x_2^4 + x_3^4 + x_4^4 + x_5^4) + x_2^5 + x_3^5 + x_4^5 + x_5^5 = -27x_1^5 \\ &+ 10x_1^3\Sigma(x_1^2) + 10x_1^2\Sigma(x_1^3) + 5x_1\Sigma(x_1^4) + \Sigma(x_1^5). \end{aligned}$$

We may find  $h, k$ , and  $l$  in terms of  $g$ , as we found  $n, p$ , and  $r$  in terms of  $m$ ; and as  $\Sigma(x_1^2), \Sigma(x_1^3), \&c.$  are known functions of  $A, B, \&c.$ , we shall have  $g, h, \&c.$  functions of  $x_1$ . The determination of these therefore may be said to depend upon the solution of the given equation. If otherwise found, as they may be by finding the equation on which  $g$  depends, it must be by an equation of the fifth degree not reducible; for the five values of  $x_1, x_2, \&c.$  being distinct, there will be as many distinct values of  $g$ .

It may be observed that if we make  $\lambda$  any other integer function of  $a$ , not passing the fifth degree, we shall still have an ultimate equation to solve of the same degree.

To give two very simple examples of the equation in  $g$ , let

$$x^5 + Ax + B = 0.$$

Then

$$\Sigma(x_1^2) = 0, \Sigma(x_1^3) = 0, \Sigma(x_1^4) = -4A, \Sigma(x_1^5) = -5B;$$

and

$$g = 27x_1^5 + 20Ax_1 + 5B.$$

Eliminating  $x_1$  between this and  $x_1^5 + Ax_1 + B = 0$ , we have

$$(g + 22B)^5 + 7^4 A^5 (g + 22B) - 7^5 A^5 B = 0.$$

Again, let  $x^5 + Ax^2 + B = 0$ . In this case

$$\Sigma(x_1^2) = 0, \Sigma(x_1^3) = -3A, \Sigma(x_1^4) = 0, \Sigma(x_1^5) = -5B;$$

and

$$g = 27x_1^5 + 30Ax_1^2 + 5B.$$

Eliminate  $x_1$  from this and  $x_1^5 + Ax_1^2 + B = 0$ , and there results

$$(g + 22B)^{\frac{5}{2}} + 3^{\frac{5}{2}} A^{\frac{5}{2}} (g + 22B) - 3^{\frac{5}{2}} A^{\frac{5}{2}} B = 0.$$

By making  $g + 22B = v$  in the first of these examples, and  $g + 22B = v^2$  in the second, the equations in  $v$  are similar to those in  $x$ , and are no way in a more solvable form.

Let us now take the equation of the sixth degree,

$$\begin{aligned} x^6 + Ax^4 + Bx^3 + Cx^2 + Dx + E &= (x^4 + ax^3 + bx^2 + cx + d) \\ (x^2 - ax + f) &= 0. \end{aligned}$$

There are fifteen ways in which this may be done, and consequently the equation in  $a$  will be of the fifteenth degree. As before,

$a_1 = x_1 + x_2$ ,  $a_2 = x_1 + x_3$ ,  $a_3 = x_1 + x_4$ ,  $a_4 = x_1 + x_5$ ,  $a_5 = x_1 + x_6$ , &c., and

$$0 = x_1 + x_2 + x_3 + x_4 + x_5 + x_6$$

If we eliminate  $x_1, x_2$ , &c. from these sixteen equations, we shall have ten resulting equations between  $a_1, a_2$ , &c., which will give  $a_6, a_7$ , &c. in terms of the first five of these quantities. The equation of the fifteenth degree is therefore reducible to one of the fifth, or

$$a^5 + ma^4 + na^3 + pa^2 + qa + r = 0,$$

where

$$-m = a_1 + a_2 + a_3 + a_4 + a_5 = 4x_1.$$

The determination of  $m$  then will be the same thing as solving the given equation of the sixth degree. And it is easy to see that we shall arrive at results precisely the same in equations of a still higher degree.

If we resolve the given equation into the factors  $x^3 + ax^2 + bx + c$  and  $x^3 - ax^2 + fx + g$ , we shall have

$$a_1 = x_1 + x_2 + x_3, \quad a_2 = x_1 + x_2 + x_4, \quad \&c.,$$

and the equation in  $a$  will be of the twentieth degree. But since  $a_{11} = -a_1$ ,  $a_{12} = -a_2$ , &c., the equation in  $a^2$  will be only of the tenth degree. The reduced equation however, whether we find by it  $a$  or  $a^2$ , will be of a higher degree than the fifth.

Let us now return to (1.), or the equation of the fifth degree, in order to find Lagrange's final equation of the sixth degree.

Make

$$\begin{aligned} x_1 &= \theta^{\frac{1}{5}}_1 + \theta^{\frac{1}{5}}_2 + \theta^{\frac{1}{5}}_3 + \theta^{\frac{1}{5}}_4, & x_2 &= \alpha\theta^{\frac{1}{5}}_1 + \alpha^2\theta^{\frac{1}{5}}_2 + \alpha^3\theta^{\frac{1}{5}}_3 + \alpha^4\theta^{\frac{1}{5}}_4, \\ x_3 &= \beta\theta^{\frac{1}{5}}_1 + \beta^2\theta^{\frac{1}{5}}_2 + \beta^3\theta^{\frac{1}{5}}_3 + \beta^4\theta^{\frac{1}{5}}_4, & x_4 &= \gamma\theta^{\frac{1}{5}}_1 + \gamma^2\theta^{\frac{1}{5}}_2 + \gamma^3\theta^{\frac{1}{5}}_3 + \gamma^4\theta^{\frac{1}{5}}_4, \\ x_5 &= \delta\theta^{\frac{1}{5}}_1 + \delta^2\theta^{\frac{1}{5}}_2 + \delta^3\theta^{\frac{1}{5}}_3 + \delta^4\theta^{\frac{1}{5}}_4. \end{aligned}$$

Whence we find

$$\begin{aligned} 5\theta^{\frac{1}{5}}_1 &= x_1 + \alpha^4x_2 + \beta^4x_3 + \gamma^4x_4 + \delta^4x_5 \\ 5\theta^{\frac{1}{5}}_2 &= x_1 + \alpha^3x_2 + \beta^3x_3 + \gamma^3x_4 + \delta^3x_5 \\ 5\theta^{\frac{1}{5}}_3 &= x_1 + \alpha^2x_2 + \beta^2x_3 + \gamma^2x_4 + \delta^2x_5 \\ 5\theta^{\frac{1}{5}}_4 &= x_1 + \alpha x_2 + \beta x_3 + \gamma x_4 + \delta x_5 \end{aligned}$$

where 1,  $\alpha$ ,  $\beta$ ,  $\gamma$ ,  $\delta$  are the five roots of unity. If we make  $\beta = \alpha^2$ ,  $\gamma = \alpha^3$ ,  $\delta = \alpha^4$ , we have

$$5\theta_1^4 = x_1 + \alpha^4 x_2 + \alpha^3 x_3 + \alpha^2 x_4 + \alpha x_5$$

$$5\theta_2^4 = x_1 + \alpha^3 x_2 + \alpha x_3 + \alpha^4 x_4 + \alpha^2 x_5$$

$$5\theta_3^4 = x_1 + \alpha^2 x_2 + \alpha^4 x_3 + \alpha x_4 + \alpha^3 x_5$$

$$5\theta_4^4 = x_1 + \alpha x_2 + \alpha^2 x_3 + \alpha^3 x_4 + \alpha^4 x_5.$$

Let  $\theta_1, \theta_2, \theta_3, \theta_4$  be the roots of

$$\theta^4 + M\theta^3 + N\theta^2 + P\theta + Q = 0.$$

Then

$$-M = \theta_1 + \theta_2 + \theta_3 + \theta_4;$$

to find which I employ

$$(u + v + w + z + t)^5 = \Sigma(u^5) + 5\Sigma(u^4v) + 10\Sigma(u^3v^2) + 20\Sigma(u^3vw) \\ + 30\Sigma(u^2v^2w) + 60\Sigma(u^2vwz) + 120uvwzt,$$

collecting the terms separately, and reducing by means of

$$1 + \alpha + \alpha^2 + \alpha^3 + \alpha^4 = 0, \quad x_1 + x_2 + x_3 + x_4 + x_5 = 0,$$

$$x_2^2 + x_3^2 + x_4^2 + x_5^2 = \Sigma(x_1^2) - x_1^2, \quad x_1^3 + x_2^3 + x_3^3 + x_4^3 + x_5^3 = \Sigma(x_1^3) - x_1^3, \&c.;$$

and also

$$x_1^3 = x_1^2 x_1 = -x_1^2(x_2 + x_3 + x_4 + x_5),$$

$$x_1^3(x_2 x_3 + x_3 x_4) = -x_1^2 x_2^2 x_5 - x_1^2 x_3^2 x_4 - x_1^2 x_4^2 x_3 - x_1^2 x_5^2 x_2$$

$$-x_1^2(x_2 x_3 x_4 + x_2 x_3 x_5 + x_2 x_4 x_5 + x_3 x_4 x_5), \&c.$$

We thus find

$$\left. \begin{aligned} 5^5(\theta_1 + \theta_2 + \theta_3 + \theta_4) &= 19\Sigma(x_1^4) - 10\Sigma(x_1^3)\Sigma(x_1^2) - 20\Sigma(x_1^3 x_2 x_3) \\ &\quad - 130\Sigma(x_1^2 x_2^2 x_3) - 40\Sigma(x_1^2 x_2 x_3 x_4) + 480x_1 x_2 x_3 x_4 x_5 + 250 \\ &\quad (x_1^2 x_2^2 x_4 + x_1^2 x_3^2 x_2 + x_1^2 x_4^2 x_3 + x_1^2 x_5^2 x_2 + x_2^2 x_3^2 x_4 + x_2^2 x_4^2 x_3 \\ &\quad + x_2^2 x_5^2 x_1 + x_3^2 x_4^2 x_1 + x_3^2 x_5^2 x_4 + x_4^2 x_5^2 x_2). \end{aligned} \right\} (6.)$$

The first six terms of the second member are all given, being symmetrical functions of  $x_1, x_2, \&c.$  Let their sum be  $R$ ; then, putting  $-M$  for  $\Sigma(\theta_1)$ , the above will be

$$-5^5 M - R = 250(x_1^2 x_2^2 x_4 + x_1^2 x_3^2 x_2 + \dots).$$

Or if we make

$$\frac{5^5 M + R}{250} = \phi,$$

it will become

$$-\phi = x_1^2 x_2^2 x_4 + x_1^2 x_3^2 x_2 + \dots$$

Now if we make  $x_1, x_2$  change places in the second member of the last, then  $x_1, x_3$ , and  $x_2, x_3, \&c.$ , we shall find that it has six different values, as stated by Lagrange. Thus



$$-\phi_1 = x_1^2 x_2^2 x_4 + x_1^2 x_3^2 x_2 + \dots\dots$$

$$-\phi_2 = x_1^2 x_2^2 x_4 + x_2^2 x_3^2 x_1 + \dots\dots$$

$$-\phi_3 = x_2^2 x_3^2 x_4 + x_1^2 x_3^2 x_2 + \dots\dots$$

$$-\phi_4 = x_2^2 x_4^2 x_1 + x_3^2 x_4^2 x_2 + \dots\dots$$

$$-\phi_5 = x_2^2 x_5^2 x_4 + x_3^2 x_5^2 x_2 + \dots\dots$$

$$-\phi_6 = x_1^2 x_5^2 x_4 + x_1^2 x_3^2 x_5 + \dots\dots$$

By adding these six equations, the sum of the second members will be a symmetrical function of  $x_1$ , &c., and we easily find  $-\Sigma(\phi_1) = \Sigma(x_1^2 x_2^2)$ , a given quantity. Thus the coefficient of the second term of the equation of the sixth degree, of which the roots are  $\phi_1$ ,  $\phi_2$ , &c., is known, and the other coefficients may be found by means of it. It does not appear that there is any other relation between  $\phi_1$ ,  $\phi_2$ , &c.; and therefore it would seem that the equation of the sixth degree is not reducible. But if any one thinks that there may exist such a relation as

$$\phi_1 = f(\phi_2),$$

$f$  denoting a rational function, he may, from what precedes, make the trial. Success however seems so hopeless, that it is pity that time and talent should be wasted upon it.

Gunthwaite Hall, near Barnsley,  
October 2, 1847.

LV. *Letter from Capt. J. H. LEFROY, R.A., Director of the Magnetic Observatory of Toronto in Canada, to Lieut.-Colonel SABINE, R.A., on a great Magnetic Disturbance on the 24th of September 1847.*

Observatory, Toronto,  
September 24, 1847.

MY DEAR COLONEL,

**T**HIS day has been distinguished by a greater disturbance than any we have had yet. The observed range of Declination was  $4^\circ 2'$ ; and I have little doubt that the actual range was greater, as the non-commissioned officer on duty, when he found that the movement was beyond the scale of the Observatory declinometer, lost time in sending for me, instead of at once lighting the lamp of the transportable one, and following it up on that. The observed range of horizontal force was over 600 divisions, or 0.052 of the horizontal force! The day has been raw and cloudy, with occasional rain, so that if an aurora existed, it could not have been seen. The disturbance seems to have begun between 21<sup>h</sup> and 22<sup>h</sup> Göttingen time on the 23rd, as the observation at 22<sup>h</sup> was decidedly unusual; but extra observations did not commence until 23<sup>h</sup> 20<sup>m</sup>. The extreme disturbance began about 0<sup>h</sup> 35<sup>m</sup> on the 24th, when both the large declinometer and large bifilar went off their scales.

At this time I was called, and we began to observe the transportable declinometer and bifilar. The last also went off the scale. The lowest reading of the former was  $692.5$  at  $1^h 0^m$  Gött., and the highest  $1126.0$  at  $1^h 45^m$ : this gives a range of  $3^\circ 36'.7$ ; but at a subsequent period ( $5^h 0^m$  Gött.) a reading of  $1177.2$  was obtained, thus giving the enormous range of  $4^\circ 2'.3^*$ . I did not take a reading of your compass; but looking hastily at it, I perceived that during the great shock it was ranging more than  $3^\circ 20'$  from its usual position. As both bifilar scales were exceeded, we can only say that the range of that element between  $0^h$  and  $1^h$  Gött. exceeded 600 divisions, or  $0.052$  of its whole amount, on the testimony of two instruments; a fact which cannot, I think, but make it a most interesting question, what is the nature of a force subject to such immense variations, and how can they occur without affecting or being affected by the other physical agents in the globe? This disturbance was attended by a great degree of motion in the magnets, a peculiar mechanical agitation, which they only exhibit on rare occasions; it lasted, more or less, down to  $12^h$  Gött. As the results have not been made up, I cannot state precisely the range of inclination, but perhaps may do so before I close this.

After some little trouble, I think we have got Dr. Robinson's Anemometer into beautiful working order. If the principle on which the velocity is estimated is correct, as we must feel confident it must be, I think it has a great superiority over any other instrument of the kind yet invented. The facility and precision with which the velocity is measured, and the beautiful manner in which sudden changes are shown, together with the large scale on which directions are marked, make it a pleasure to use it, and make Osler's instrument look quite clumsy beside it; it is a most elegant instrument, and will give diurnal curves of velocity with a precision we have never attained before. I found on careful examination that Osler's anemometer, which has been up seven years, was much the worse for wear, and not in a condition to give a satisfactory comparison with the other; we have therefore, with a good deal of difficulty, taken it down. I have put it into the hands of an engineer here, and he is to refit all the essential parts, particularly the shoulder and collar of the vane, which were worn, and made the vane unsteady: we shall then be able to compare pressures and velocities.

Believe me, my dear Colonel,

Faithfully yours,

J. H. LEFROY.

\* I think our greatest range before this was only  $2^\circ 23'$ ; this occurred last April.

LVI. *On the Decomposition of Valerianic Acid by the Voltaic Current.* By H. KOLBE, Ph.D.\*

THE very remarkable changes which a series of organic compounds undergoes by means of the voltaic current, have induced me to make that mode of decomposition the subject of a thorough investigation. As however the numerous difficulties which present themselves in researches of this nature, and the immense extent of the field which opens before us, do not admit of the results being communicated in a complete and connected form, I beg to lay before the Chemical Society a short preliminary notice of the changes which valerianic acid undergoes when exposed to the oxidizing action of the voltaic current, reserving a more complete description of the products obtained till the investigation shall have been brought to a close.

When the voltaic current, excited by six pairs of Bunsen's carbo-zinc battery, is permitted to act on a concentrated neutral solution of valerianate of potash in the cold, two plates of platinum forming the electrodes, a brisk evolution of gas takes place simultaneously from both; the gases evolved consist of hydrogen, carbonic acid and a new carbo-hydrogen, but contain no traces of oxygen gas as long as the solution of valerianate of potash does not become too much exhausted. At the same time a light oily liquid separates at the surface, having an agreeable æthereal odour, and the alkaline solution ultimately consists chiefly of carbonate and bicarbonate of potash, the latter of which generally separates during the operation in a crystalline form.

The neutral æthereal oil is a mixture of two compounds; the one containing oxygen, the other perfectly free from it. By the action of an alcoholic solution of potash the former is decomposed, and the latter can then, by means of water, be separated unchanged. In the pure state it exists in the form of a light colourless æthereal oil, possessing an agreeable aromatic smell. It is insoluble in water, but soluble in alcohol and æther; it boils at  $108^{\circ}$  C. without decomposition, and has the composition  $C_8 H_9$ . Oxygen and iodine are without action upon it, but chlorine, bromine, and fuming nitric acid form with it products of substitution.

The oil containing oxygen, which in the first instance was found mixed with this substance, I have not yet been able to obtain in a pure state; but several circumstances render it more than probable that it is formed by the union of valerianic acid with the oxide of the above carbo-hydrogen. An

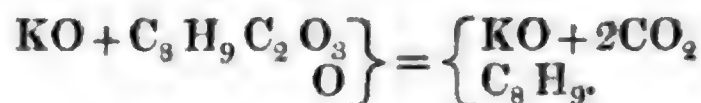
\* Communicated by the Chemical Society; having been read April 19, 1847.

alcoholic solution of potash treated with it is found to contain as a product of decomposition a considerable amount of valerianate of potash. But on account of the small quantity of material which has been at my disposal, I have not succeeded in separating the alcohol  $C_8H_{10}O_2$ , which must have been formed at the same time.

The gaseous carbo-hydrogen, which is evolved with the hydrogen, is a substance analogous to olefiant gas; it is characterized by a peculiar æthereal smell, and has a specific gravity double that of olefiant gas. It unites with chlorine even in the dark, forming a heavy oily liquid, having a marked similarity to chlorelayl, and is generally composed of a mixture of several products of substitution. Its rational composition is expressed by the formula  $C_8H_8$ . The changes which valerianic acid undergoes, in accordance with the foregoing experiments, are capable of a very simple explanation, if we consider that acid as a conjugated combination of the carburetted hydrogen, or the radical  $C_8H_9$  with oxalic acid, in a similar manner to the new view taken of the constitution of acetic acid. For whilst by the addition of one atom of oxygen oxalic acid becomes converted into carbonic acid, this radical is set free; but a portion of it unites with the excess of oxygen to form an oxide, and this enters into combination with a portion of undecomposed valerianic acid, giving rise to a new æther,  $C_8H_9O + C_8H_9C_2O_3$ .

Another portion of the radical is probably decomposed at the moment of its formation, in consequence of the concomitant evolution of heat into hydrogen and the gaseous carbo-hydrogen  $C_8H_8$ . This latter view is supported by the fact, that if the temperature of the solution of valerianate of potash exceeds a certain point during the decomposition, not a single drop more of the ætherial oil is produced.

The following formula will throw light on this decomposition:—



Both butyric and acetic acids are acted on in a similar manner to valerianic acid; the products of decomposition of acetic acid are all gaseous, and appear to contain oxide of methyle. Butyric acid gives in addition to the gaseous compounds a volatile oil composed of  $C_6H_7$ .

The minute description of this product will form the subject of a future memoir.

The foregoing investigation has been carried out during the late session in the laboratory of Dr. Lyon Playfair, as whose assistant I have been engaged during that time; and I



cannot allow this opportunity to pass by without thanking him for the kindness and liberality which he has shown in placing his laboratory at my disposal, in leaving so much of my time on my own hands, and in rendering me every assistance in his power.

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LVII. *An Account of Experiments with Galvanic Couples immersed in pure water and in oxygenated water.* By Mr. RICHARD ADIE\*.

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IN the years 1845 and 1846, I published in the Edinburgh Philosophical Journal two series of experiments, made with a view to prove that the action of the water battery was maintained by absorbing oxygen from the atmosphere. Some of these experiments† show that it is the oxygen only that is drawn from the atmosphere, and that the presence of the other component parts is unnecessary. But there was one given to show that zinc and copper elements placed in a hermetically sealed tube along with pure water did not act, there being no flocculent deposit of oxide of zinc, which is formed in abundance when a minute aperture admits the atmosphere to the contents of the tube. After a lapse of two years, I examined an arrangement of this kind which had been hermetically sealed since December 1844; there was no apparent change, the water was transparent, and the metals bright. I had scarcely put the tube down when it burst with violence; this fact immediately satisfied me that the water battery must have a true decomposition of water action when it acts on zinc associated with copper or any other metal less oxidizable than the copper, independent of the much more extensive effect due to atmospheric oxygen. It is from a desire to trace by experiment the double action of this battery that I respectfully submit for the consideration of the Society the following results:—

In fig. 1, *a a a* represents six pieces of zinc soldered at *c c* to a corresponding number of pieces of copper *b b b*, arranged alternately as in the figure, and insulated from one another by strands of thread, *d d d d*. These were placed inside a flint-glass test-tube, which was after their insertion drawn off at the blowpipe to a capillary point. The tube was now filled with pure water, and to dislodge the air from among the fibres of the thread, the water was repeatedly boiled, closing and re-opening the capillary point at each boiling. When the air was well-removed, the tube was hermetically sealed,

\* Communicated by the Chemical Society; having been read April 19, 1847.

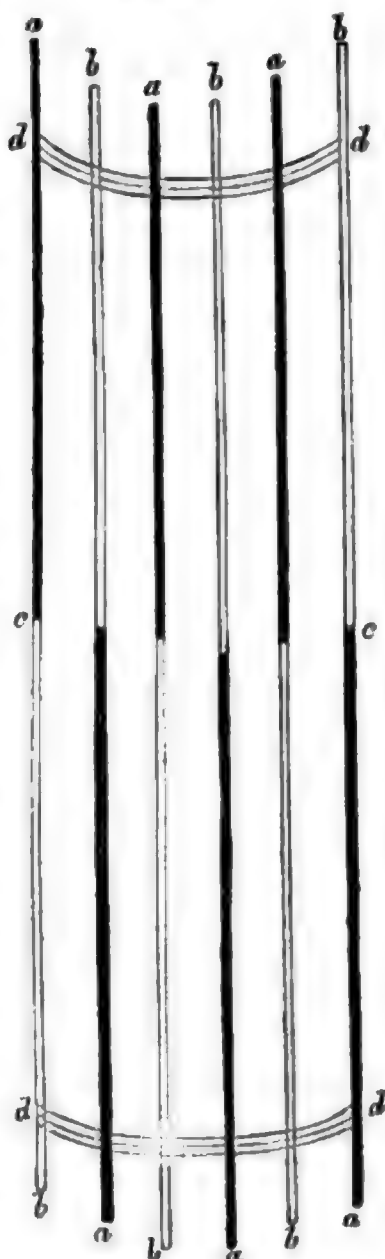
† Edinburgh New Phil. Journal, vol. xxxviii. p. 99, and vol. xl.

the water at the time of closing being near the boiling temperature. On cooling, the space left vacant by the contraction of the fluid was estimated to be  $\frac{1}{70}$ th of a cubic inch; the superficies of each plate  $\frac{1}{4}$ th of a superficial inch. From previous trials, I knew that when the above arrangement had a communication with the atmosphere, a flocculent deposit of the protoxide of zinc was soon perceived, which steadily increased. With the same hermetically sealed there was no such deposit; neither was there any perceptible change, until the bursting of the vessel after two years revealed another action of the battery. Judging from the thickness of the broken glass, I endeavoured at the time to make an approximate estimate of the volume of the gas generated, which in the vacant space of  $\frac{1}{70}$ th of a cubic inch, where it could lodge, produced pressure sufficient to burst the vessel: the result of my estimate gave less than a cubic inch of gas measured at the usual atmospheric pressure; for the development of which six zinc surfaces of  $\frac{1}{4}$ th of a superficial inch each had been two years in action. In a repetition of this experiment, with zinc filings in lieu of plates, a small quantity of gas was collected, and proved to be hydrogen.

Afterwards examining the inner surfaces of the fragments of the glass, the surface of the plates, and the fibres of the thread with a powerful lens, I found all of them covered with minute transparent crystals; the largest crop of these was on a copper surface opposite a spot on one of the zinc plates, to which nearly the whole of the corrosion of the metal appeared to have been confined. The red ground of the copper surface showed them most distinctly. On heating the copper the crystals parted with water of crystallization, and became circular white spots, very much resembling the protoxide of zinc.

My friend Mr. Waldie examined the thread; his process was, incinerating, dissolving the ash in hydrochloric acid, adding excess of potash, filtering to separate a trace of oxide

Fig. 1.



of iron, and treating the filtered liquid with hydrosulphuret of ammonia, which gave a yellowish white precipitate. This result proves that the minute transparent crystals among the fibres of the thread contained protoxide of zinc.

On a former occasion I employed either the air-pump or ebullition to deprive water used in exciting voltaic couples, of absorbed air. I gave preference to the method of boiling the water in the battery cell, as the more severe test, for showing how far a battery's action depended on oxygen from the atmosphere. The proof which appeared to me to furnish satisfactory evidence of the assistance given by absorbed oxygen, was when the indication in the galvanometer fell near to zero by prolonged boiling, and rose again when water holding dissolved air was thrown into the cell. According to this test, a zinc and platinum couple lose much of their action when excited by pure water boiled for near two hours. The galvanometer needles always indicated a slight action, however long the boiling was prolonged; but as I found when care was taken to have an atmosphere of steam resting on the surface of the boiling water the action of the couple was at its lowest, I was led to think that what remained might be due to oxygen from the atmosphere, which it was impossible to remove perfectly. The experiment given above renders this view no longer tenable; for if zinc and copper elements can at ordinary temperatures slowly generate gas, it must follow that all the elements less oxidizable than copper will at boiling temperatures possess, when associated with zinc, a voltaic action independent of oxygen from the atmosphere.

To try the effect of a zinc and copper couple excited by pure boiling water, I attached a pair of plates to a more sensitive galvanometer than I had hitherto used: the plates were placed in a Florence flask and covered to a depth of two inches with pure water previously distilled in glass vessels; there was only a small orifice in the cork at the top of the flask for a steam escape, in order to preserve the boiling surface from the atmosphere.

Previous to boiling, the galvanometer needle stood at  $50^{\circ}$

Indication the moment boiling was about to begin .  $70^{\circ}$

... after long boiling . . . . .  $20^{\circ}$

A similar experiment with iron and copper elements:—

Indication previous to boiling . . . . .  $20^{\circ}$

... at boiling . . . . .  $46^{\circ}$

... after long boiling . . . . .  $7^{\circ}$

In this experiment the indication rose on cooling to  $30^{\circ}$ , and afterwards fell back.

When silver or platinum was substituted for the copper the

results were in the same order, giving the highest action at the time the water is parting with dissolved air, and lowest when the water is thoroughly boiled. Where zinc is the positive element the action falls considerably, as the boiled water cools before it has time to re-absorb air. With a little common salt added to the water of a zinc and platinum couple, ebullition serves greatly to exalt the action, for the arrangement is no longer dependent on oxygen from the atmosphere.

These experiments, in extension of those I formerly submitted to the public through the *Edinburgh Philosophical Journal*, do not militate against the general conclusion then drawn, that the water battery supported its action by absorbing oxygen from the atmosphere; they only show that there is in addition a minute degree of action when two metallic elements are excited by pure water.

Perhaps the experiments of the most importance for determining the theory of the action of gas absorbing galvanic couples, are those where one metal only is excited by oxygenated water; to illustrate this action I made the following experiments:—

Two slips of zinc cut side by side from the same sheet were placed in a running brook, the one opposed to a rapid part of the current, the other in a still place at the edge. Connecting these in the usual manner with the galvanometer, there was a permanent deflection of  $25^{\circ}$ ; and on changing the respective places of the plates in the stream without disturbing their attachments to the galvanometer, the needles immediately passed to the opposite side of the card; in both cases the piece of zinc in the current acted as a negative or platinode plate. With both plates in still water and a tube filled with oxygen inverted over one, the effect was the same. It is the greater supply of oxygen to the plate in the current which converts it into a negative or platinode. A cell containing two small silver wires and the cyanide of silver solution used for electro-plating was attached in place of the galvanometer, when, after a lapse of two hours, metallic silver was seen precipitated in a minute quantity on the silver wire connected with the piece of zinc in still water.

Two plates of iron were placed in the stream, under like conditions to the zinc; after two hours metallic silver was distinctly seen precipitated on the silver wire connected with the iron plate in still water.

The fact here shown, of two similar pieces of iron giving rise to a galvanic current capable of precipitating metallic silver, appears to me to be important, for it proves that the electricity in passing through the water intervening between



these two plates, either decomposes it with the aid of oxygen in solution, or that oxygenated water forms a binary compound, capable of acting as an electrolyte.

The fact of iron and oxygen uniting together at ordinary temperatures when moisture is present, is well known. It is the office performed by the water during this union, wherein lies the true ground of the theory of gas-absorbing batteries. A single plate of iron exposed to water and oxygen gas, has local differences on its surface which act in the same way as if the iron had been in two halves and placed in a stream in the manner described: the oxidation of the iron is developing a voltaic current which passes through the fluid from one point of the plate to another, either by a process of decomposition and re-composition of water, or by the decomposition of the compound formed by the solution of the gas in water.

The first of these views can only be supported by holding that the solution of oxygen so changes the affinities, that iron with its aid can at ordinary temperatures decompose water. I see no evidence sufficient to give probability to this hypothesis, while, if the second supposition be admitted to meet all the facts shown by experiment, it will establish the existence of an electrolyte more easily decomposed than water, and as universal in nature; and account for the very reduced action of zinc and copper elements excited by pure water freed from absorbed air or from oxygen gas, the active principle derived from the air.

I may here take occasion to add, that a saturated solution of carbonate of potash and soda in an open cylindrical vessel has so shut out the oxygen of the atmosphere from some pieces of iron immersed in it, that now, after two years and four months immersion, there is no rust on the surface of the iron.

The experiments with two similar pieces of zinc or of iron placed in a running stream, as already described, were performed during the cold weather of winter, with the temperature varying from 32° F. to 42°. On the return of a little warmer weather I recommenced the experiments with iron plates, from a wish to try if two similar pieces of iron could be made to develop a voltaic current of the same electromotive force as that derived from a platinum and iron couple excited by still water.

A piece of iron wire was cut into two equal lengths; each of those was bent into the form of a flat spiral (fig. 2), and a copper wire well-varnished was soldered to the iron at A, for connecting the plate with a small decomposing apparatus in the usual manner.

A pair of iron plates thus formed was taken to the banks of a small stream in Cheshire, called the Grange brook ; one plate was placed in a current of moderate velocity, where the water poured through the interstices of the coils ; the other plate was dropped into still water in a convenient place at the edge. In both plates the solderings at A projected above the surface and were kept dry.

Fig. 2.



When copper wire poles were placed in a decomposing cell filled with sulphate of copper solution, and connected with the galvanic couple formed of two pieces of iron (fig. 2), distinct evidence of the precipitation of metallic copper on the wire connected with the plate in still water, was observed after an hour's action, temperature  $45^{\circ}$ . One of the iron plates was now removed to a cell filled with water, and associated with a platinum plate, the arrangements for precipitating metallic copper remaining as before. With the temperature at  $42^{\circ}$ , the depositing of the metal did not proceed so actively as it had done with an iron plate in a current of water for a platinode.

When the decomposing cell was filled with a solution of sulphate of zinc, and zinc wire poles supplied, after three hours' action, temperature  $46^{\circ}$ , the wire in connexion with the iron plate in still water showed, with the aid of a lens, a distinct deposit of metallic zinc. Repeating this experiment with an iron and platinum couple in still water, the metallic deposit of zinc was again obtained, temperature  $46^{\circ}$ , the rate of action in both experiments being, as near as I could judge, the same.

The inference from these results is, that a piece of bright iron placed in a current of water performs the office of a piece of platinum, as well as the latter metal does when excited by still water.

The quantities of metal precipitated during two or three hours' action of these oxygen absorbing batteries is in no case sufficient to give results by weight. I have tried experiments of one week each, but the changes in the level of the stream and other sources of derangement, made me prefer trials of two or three hours each, where there is no difficulty in detecting any decided change in the rate of action.

The Grange brook is supplied with water almost wholly by the drainage of a rather poor clay soil, reposing on the new red sandstone formation of the district.

The plate in the centre of the stream shows less rusting than the one at the edge in still water; but judging from the analogous case of the copper sheathing of a ship, it should waste away the fastest, the particles of peroxide of iron as they are formed being removed by the force of the stream, while the voltaic current developed during this action only circulates to some other point of the same plate, or belongs to what is called local action.

From the above results, the benefit to be obtained in a constant battery by making the negative plate rotate, should be apparent; but to prevent waste it would still be necessary to employ one of the more costly metals, which are not liable to oxidation.

In concluding these experiments, I may again notice, that a tube of oxygen suspended over a plate of iron in still water has the same effect as the current of the stream, converting the oxygenated plate into a platinode. The carbonic acid present in all surface water may by some be thought to perform an essential part in the ordinary rusting of iron. But where every care is taken to exclude this gas from a tube filled with oxygen, a small quantity of water, and a piece of iron, the oxidation of the iron proceeds with rapidity, accompanied by changes which appear to me to preclude the idea that even a trace of carbonic acid can be essential. The oxygen gas disappears; at first an abundant formation of red or peroxide of iron is seen; then, after the supply of oxygen has decreased, the green-coloured protoxide is gradually formed. These two oxides afterwards begin slowly to unite, and form the well-known black or magnetic oxide. In an experiment of this kind every trace of the red and green-coloured oxides had disappeared at the end of three months from the time of closing the tube, and there remained only an inky precipitate, which was proved to be the black oxide of iron.

LVIII. *On certain Improvements in the Construction and Supply of the Hydro-Oxygen Blowpipe, by which Platinum may be fused in the large way.* By ROBERT HARE, M.D.\*

ON my return from Europe in 1836, I was very much in want of a piece of platinum of a certain weight, while many more scraps than were adequate to form such a piece were in my possession. This induced new efforts to extend the power of my blowpipe; and after many experiments, I succeeded so as to fuse twenty-eight ounces of platinum into one mass.

\* Communicated by the Author.

Although small lumps of platinum had been fused by many operators with the hydro-oxygen blowpipe as well as myself, it had not, up to the year 1837, been found sufficiently competent to enable artists to resort to this process. I am informed by Mr. Saxton, that some efforts which were made while he was in London were so little successful, that the project was abandoned. There was an impression that the metal was rendered less malleable when fused upon charcoal, as in the experiments alluded to. This is contradicted by my experiments, agreeably to which fused platinum is as malleable as the best specimens obtained by the Wollaston process, and is less liable to flake. Dr. Ure, on seeing specimens of platinum which I had elaborated and fused in the form of wire, of leaf, ingots and plate, said that there was no one in Europe who could fuse platinum in such masses. He also informed me that it had been found so difficult to weld platinum, that no resort was had to that process. In this I concur, having had the welding tried by a skilful smith, both with a forge heat, and with a heat given by the hydro-oxygen blowpipe. An incorporation of two ingots was effected on their being hammered together, when heated nearly to fusion; but on hammering the resulting mass cold, a separation took place along the joint by which the ingots were united.

The difficulty seems to arise from the rapidity with which the platinum becomes refrigerated. It seems to have a less capacity for heat than iron; and, not burning in the air as iron does, has not the benefit of the heat acquired by iron from its own combustion with atmospheric oxygen.

Lately, by means of the instrument and process which it is my object here to describe, I have been enabled to obtain malleable platinum directly from the ore, by the continued application of the flame. From some specimens of platinum I have procured as much as ninety per cent. of malleable metal. The malleability is not inferior to that of the best specimens obtained by reducing it to the state of sponge, through the agency of aqua regia and sal-ammoniac. There is however a greater liability to tarnish, arising probably from the presence of a minute portion of palladium.

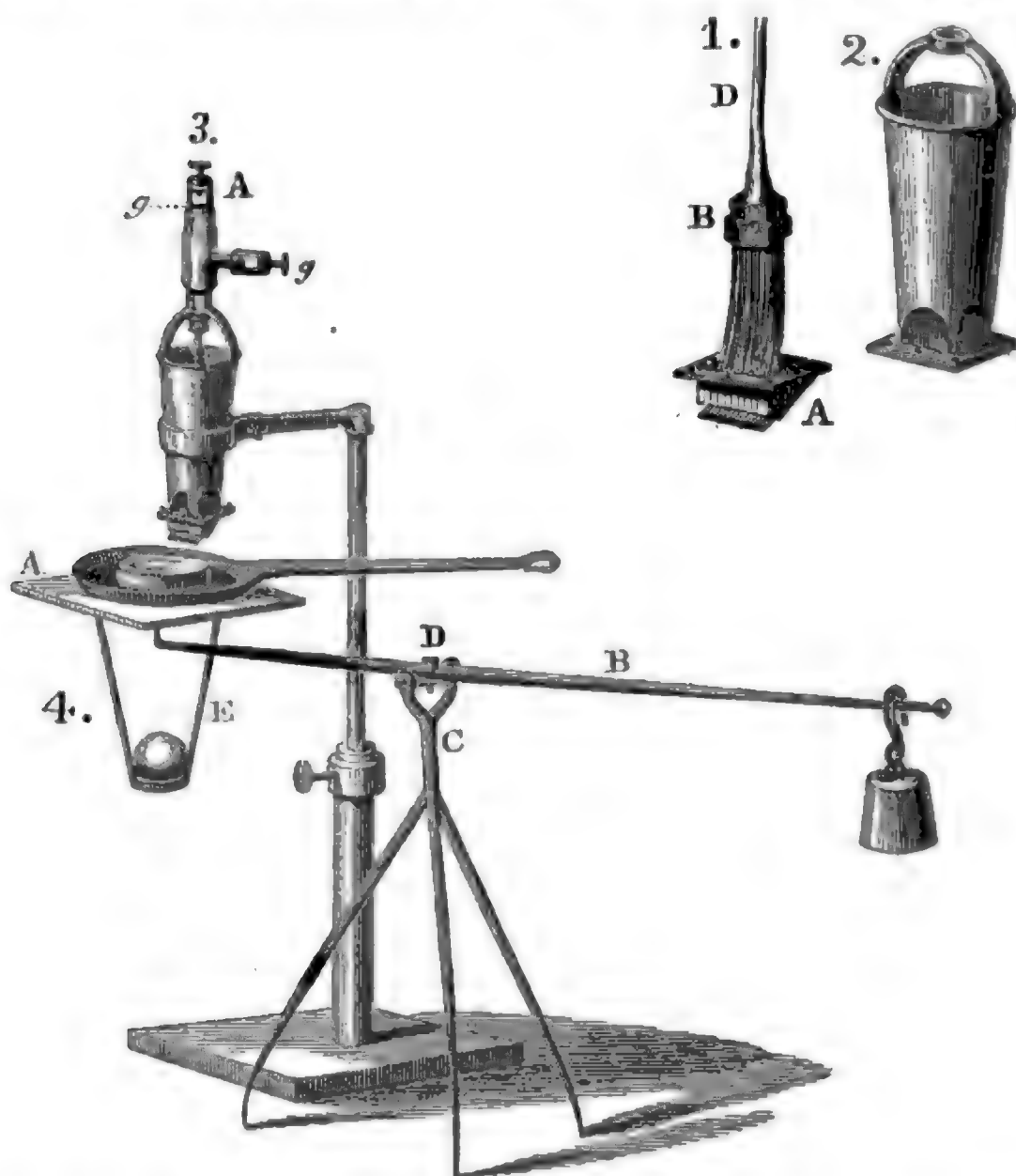
Of the fusion of iridium and rhodium, I have already given an account in the *Bulletin of the American Philosophical Society*, which was subsequently embodied in an article in this *Journal* for August 1847.

It remains now to give an account of the apparatus employed in the fusion of platina on a large scale.

Fig. 1 represents the association of fifteen jet-pipes of platinum with one large pipe B D at their upper ends, so that



their bores communicate, by means of an appropriate brass casting, with that of the large pipe, the joints secured by hard solder. Their lower extremities are made to protrude about half an inch from a box A, of cast brass, their junctures, with the appropriate perforations severally made for them, being secured by silver solder. They come out obliquely in a line along one corner of the box, an interval of about a quarter of an inch alternating with each orifice. By means of flanges, the brass box is secured to a conical frustum of copper (fig. 2),



so as to form the bottom thereof, while the pipe, extending above the copper case, is screwed to a hollow cylinder of brass A, fig. 3, provided with two nozzles and gallows-screws *g g*, for the attachment of appropriate hollow knobs, to which pipes are soldered, proceeding from the reservoirs of oxygen and hydrogen. Cocks are interposed by which to regulate the emission of the gases in due proportion.

In connecting the pipes conveying the gases with the brass

cylinder A, fig. 3, care should be taken to attach that conveying oxygen to the upper nozzle, while the other, conveying hydrogen, should be attached to the lower nozzle; since by these means their great difference in density tends to promote admixture, which evidently it must be advantageous to effect.

The object of surrounding the jet-pipes with water by means of the copper box\*, is to secure them against being heated to such a degree as to cause the flame to retrocede and burn within them, so as finally to explode within the cylinder A, gg, fig. 3. It is preferable to add ice or snow to the water, in order to prevent undue heat.

Fig. 4 represents a moveable platform A, of cast iron, wholly supported upon the point of the iron lever DB, which is curved towards the extremity under the platform, so as to point upwards, and to enter a small central conical cavity made for its reception. The lever is supported by a universal joint upon the fulcrum C, so that by means of a sliding weight at one end, the platform and its appurtenances are counterpoised at the other. The platform is kept in a horizontal position by the cannon-ball, supported in a sort of iron stirrup terminating in a ring, in which the ball is placed. Upon the platform is situated an iron pan with a handle holding the brick, on a cavity in which, as already mentioned, the metal is supported. The apparatus being duly prepared and connected with the supply-pipes, the hydrogen is first allowed to escape and then the oxygen, until the ignition has attained apparently a maximum. The accomplishment of this object may of course require the adjustment of either cock several times, especially where there is any decline in the pressure either of the one or the other gas in its appropriate reservoir.

By means of the handles of the lever and of the pan, the operator is enabled to bring the metal into the position most favourable for the influence of the heat, while his hands and face are sufficiently remote to render the process supportable. In fusing any quantity, not being more than four ounces, the platform may be dispensed with, the handle of the pan being held in one hand of the operator, while by the other the cocks may be adjusted.

When the blowpipe of fifteen jets, or any larger, may be

\* Since the engraving was made, I have preferred to use water-tight boxes, with gallows-screws and nozzles, situated one near the bottom on one side, the other on the opposite side near the top. By means of the lower nozzle, a pipe is attached, communicating with a head of cold water, the other being so situated as to carry the water into a waste pipe or large tub: a circulation may be kept up during the whole time that the operation is going on,

As a support, a brick kaolin is used, having an oblong ellipsoidal depression on the upper face for the reception of the metal to be fused.









boiler iron, each capable of holding forty gallons. They are lined internally with copper, being situated upon a wooden frame, so that the bottom of one is two-thirds as high as the top of the other. The upper portions of these vessels communicate by a leaden pipe B, of about half an inch bore, furnished with a cock, while the lower portions communicate by another leaden pipe of a bore of one and a half inch.

The upper vessel is surmounted by a globular copper vessel, of about twelve inches in diameter, which, from its construction, renders it possible to introduce an additional supply of concentrated acid, while the apparatus is in operation, without reducing the pressure within the reservoir, by permitting the excess above the pressure of the atmosphere to escape. This object is accomplished as follows:—

The valve at the end of the rod attached to the lever L being kept shut by the catch M, the screw-plug H removed, the acid is introduced through the aperture thus opened. In the next place, the plug being replaced, and the valve depressed by means of the lever and rod, so as no longer to close the opening which it had occupied, the acid descends from the chamber into the cavity of the vessel beneath it. The valve is of course restored to its previous position as soon as the acid has effected its descent.

The lowermost vessel is furnished with a perforated copper tray, supported by a copper sliding rod, in a way quite analogous to that already described in the case of the copper reservoir. It is also supplied with zinc and its solvent in like manner, being made half-full of the diluted sulphuric acid. Of course, on contact being produced between the zinc and its solvent, the generation of hydrogen will take place. So long as the communication between the upper portions of the two vessels is open, the gas will extend itself into both, occupying the whole of the upper vessel, and that half of the lower one which is unoccupied by the liquid. But if in this way the pressure reaches to two atmospheres, as indicated by the gauge\*, on shutting the communication through the pipe B, the pressure in the inferior vessel will augment, that in the superior vessel remaining as before; but the liquid will consequently begin to pass out of the inferior vessel through the pipe A, and thus may lessen the contact between the acid and zinc, and finally suspend it altogether. Meanwhile the gas in the upper vessel being condensed to nearly half its previous bulk, the pressure will be nearly four atmospheres. It will,

\* I have used for a gauge an instrument like G, fig. 5, the tube being about two feet in length, and sealed at the upper end.

in fact, always be nearly double that which existed before the pipe B was closed.

In order that nearly the whole of the acid shall be expelled from the inferior vessel, the tray must be depressed till it touches the bottom of that vessel.

The pressure being four atmospheres at commencement, as soon as, by means of a pipe attached to the valve-cock N, an escape of gas is allowed, the acid is forced again upon the zinc, and thus prevents a decline of pressure to any extent sufficient to interfere with the process.

The gases may be used from a receiver in which they exist, in due proportion, safely by the following means :—

Two safety-tubes are to be made, not by Hemming's process exactly, but as follows :

A copper tube, silver soldered, of which the metal is about the eighth of an inch in thickness, is stuffed with the finest copper wire, great care being taken to have the filaments straight and parallel. The tube is then to be subjected to the wire-drawing apparatus, so as to compress the tube on its contents until the draught becomes so hard, as that it cannot be pushed further without annealing. The stuffed tube thus made is to be cut into segments, in lengths about equal to the diameter, by a fine saw. The surfaces of the sections are to be filed gently with a smooth file. By these means they appear to the naked eye like the superficies of a solid metallic cylinder. Brass caps being fitted on these sections, they are to be interposed by soldering, at the distance of a foot or more, into the pipe for supplying the jet. Under these circumstances, the posterior section becoming hot, may allow the flame to retrocede ; but the anterior section being beyond the reach of any possible combustion and remaining cold, will not allow of the retrocession ; and as soon as the flame passes the first section, the operator, being warned, will of course close the cock, and subject the posterior section to refrigeration before proceeding again.

But this plan of operating may be rendered still more secure by interposing a mercury bottle, or other suitable iron vessel, half-full of oil of turpentine, between the reservoir and safety tubes, as in the arrangement of a Woulfe's bottle. A leaden pipe proceeding from the reservoir is, by a gallows-screw, attached to an iron tube which descends into the bottle, so that its orifice may be near the bottom. The leaden pipe communicating through the safety tubes with the jet-pipe, is attached to the neck of the bottle. Thus the gaseous mixture has to bubble through the oil of turpentine in order to proceed through the safety tubes to the jet-pipe. If, while

this process is going on, the flame should, by retrocession, reach the cavity of the bottle, exploding in contact with the turpentine, a compound is formed, which is, *per se*, inexplosive from the excess of carbonaceous matter. Meanwhile the shock, acting on the surface of the oil, drives it into the bore of the iron tube, and thus, both by its chemical and mechanical influence, renders it utterly impossible that the flame should reach the cavity of the reservoir.

*Apparatus for the Fusion of Iridium or Rhodium or masses of Platinum less than five ounces in weight.*

For the fusion of either iridium or rhodium or masses of platinum not exceeding the weight of half an ounce, an instrument with three jets has been employed, the bore of each jet-pipe being such as not to admit a wire larger than the  $\frac{1}{32}$ nd of an inch in diameter. The flame produced by these means was quite sufficient to envelope the mass to which it was applied.

In fusing any lumps or congeries of platinum, not exceeding five ounces, an instrument has been used capable of giving seven jets of gas, issuing of course from as many pipes. Of these pipes, six protrude through the brass casting forming the bottom of the copper case constituting the refrigerator, so as to be equidistant from each other upon a circumference of three-fourths of an inch in diameter, the seventh protruding from the centre. The bores of these jets are such as not to admit a wire larger than  $\frac{1}{32}$ nd of an inch in thickness. Those of the larger instruments, represented by the accompanying engravings, were such as to admit wires of  $\frac{1}{24}$ th of an inch in thickness.

The jet-pipes may be made by the following process:—A thin strip of sheet metal, somewhat wider than the length of the circumference required in the proposed pipe, after being roughly turned about a wire so as to form an imperfect tube, is drawn through several suitable holes in a steel plate, as in the wire-drawer's process. Under this treatment the strip becomes converted into a hollow wire; the edges of the strip being brought into contact reciprocally, so as to leave only an almost imperceptible crevice. Having drawn one strip of platina in this way, another strip sufficiently wide nearly to inclose it is to be drawn over that first drawn, care being taken to have the crevices left at the meeting of the edges on contrary sides. The compound hollow wire or tube thus fabricated, is finally to be drawn upon a steel wire of the diameter of the requisite bore.

The following method of making jet-pipes, though more



difficult, is preferable, as there is less liability of the water of the refrigerator leaking into the bore.

Select a very sound and malleable cylinder of platina, of about three-eighths of an inch in thickness, perforate it by drilling in a lathe, so that the perforation may be concentric with the axis. A drill between one-sixteenth and one-eighth of an inch in diameter may be employed. In the next place, the cylinder may be elongated by the wire-drawing process until the proper reduction of metallic thickness is effected, the diameter of the bore being prevented from undergoing an undue diminution by the timely introduction of a steel wire.

Of course the metal must be annealed as often as it hardens, by drawing. For this purpose a much higher temperature is necessary in the case of platinum, than in that of either copper, silver, or gold.

The annealing is best performed by the hydro-oxygen flame. If charcoal be used, the greatest care must be taken to have the fireplace clean.

Agreeably to a trial made last spring, palladium may be used as a solder for platinum; and as it is nearly as difficult to fuse as this metal, it is of course for that purpose preferable to gold, where great heat is to be resisted. No doubt by employing palladium to solder the exterior juncture of the double drawn tubes above mentioned, they might answer as well nearly as when constructed of solid platinum.

This idea has been verified by a successful trial: and, moreover, silver has been successfully employed to solder the portions of the tubes, protected from heating by being within the cavity occupied by water. The portions which protrude beyond the brass box (see fig. 1) may be left unsoldered.

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LIX. *On the Composition of the Bile of the Sheep.* By J. W. GRIFFITH, M.D., F.L.S., *Physician to the Finsbury Dispensary*.\*

THE following analysis was made with the view of comparing the composition of this fluid with that of the biliary secretion in other animals; the conditions under which the analyses of the bile in them were performed have therefore been observed as closely as possible.

The bile in a perfectly fresh state was evaporated to dryness in a water-bath, the residue powdered and exhausted with alcohol of 840 specific gravity, the solution filtered, and the alcohol distilled off at 212° F.; the dry residue was next powdered, dissolved in absolute alcohol, the solution filtered

\* Communicated by the Author.

and digested with animal charcoal: when decolorized, it was again filtered, the alcohol distilled off, and the residue powdered, exhausted with æther, and perfectly dried at 212° F.

In this state it was almost white, having a slight tinge of buff.

The ash was prepared in a muffle at a low red heat.

I. 3.02 grains of the dried bile gave 0.305 ash = 10.09 per cent.; 4.83 gave 0.48 ash = 9.93 per cent.; 4.41, charred and washed in the manner proposed by Rose, gave 0.45 ash = 10.20 per cent.

The amount of chloride of sodium present in the prepared bile was very small; thus—

II. The 0.48 of ash from the 4.83 bile (I.) yielded 0.04 chloride of silver = 0.43 per cent. of chloride of sodium; 6.83 of the bile gave 0.05 chloride of silver = 0.38 per cent. of the chloride of sodium; the soda remaining, determined as sulphate, amounted to 0.99 = 6.32 per cent.

III. 4.045 bile burnt with chromate of lead, yielded 8.94 carbonic acid and 3.275 water, = carbon 60.07, and hydrogen 8.97 per cent.

IV. 4.005 bile gave 8.845 carbonic acid and 3.20 water, = carbon 60.22, and hydrogen 8.87 per cent.

V. 3.62 gave 2.29 ammonio-chloride of platinum = 3.97 nitrogen per cent.

Hence

	I.	II.
Carbon . . . . .	60.07	60.22
Hydrogen . . . . .	8.97	8.87
Nitrogen . . . . .	3.97	
Oxygen . . . . .	20.29	
Soda . . . . .	6.32	
Chloride of sodium . .	0.38	0.43
	<hr/> 100.00	

In all the specimens of the ash of bile which I have examined, on solution in water and the addition of nitrate of silver, the yellow colour resulting from the formation of the tribasic phosphate of silver was distinctly perceptible in admixture with the white colour of the chloride. The yellow precipitate was dissolved by a drop of nitric acid. Whether the phosphate thus indicated arises from the solubility of the phosphate of soda existing in the bile prior to the separation of the mucus in an alcoholic solution of bile, or to the oxidation by the heat of a certain amount of phosphorus existing in the electro-negative constituent of this fluid, and its subsequent combination with the soda, I have not determined.

By comparing the above results with those obtained by Kemp, Theyer and Schlosser, &c., from the analysis of the bile of the ox, the two fluids are seen to exhibit the same composition.

The nature of the true constitution of the bile is still a matter of doubt; the opinion that it was a compound of an electro-negative substance (bilic acid) with the base soda seemed latterly to have been almost established. If however the experiments of Mulder, which have recently been published, should be confirmed, no dependence can be placed upon direct analysis, since from the moment of the secretion of that fluid it begins to undergo decomposition: even on drying at  $212^{\circ}$  F. ammonia is evolved, and the bile ceases to be perfectly soluble in water; and all fresh bile contains ammonia. Should these results be proved correct, the analysis of this fluid must be conducted in a different way from that which has been ordinarily adopted. On dissolving some purified fresh sheep's bile in alcohol, adding a drop of muriatic acid, then a little chloride of platinum, and setting the mixture aside, I obtained a precipitate of the ammonio-chloride, the crystals of which were perfectly distinct under the microscope. This appears to give support to Mulder's statement that ammonia is present in the bile.

9 St. John's Square, August 1847.

LX. *Notice respecting the Meteor of September 25, 1846.*

*By the Rev. J. SLATTER.*

*To the Editors of the Philosophical Magazine and Journal.*

Rose Hill, near Oxford,  
October 23, 1847.

GENTLEMEN,

AS I do not generally see your Publication, I was quite unaware of any accurate notice having been put on record of a large meteor which appeared one night in the end of September 1846. During the late meeting of the British Association at Oxford, a conversation arose, from which I learnt that Sir John Lubbock had observed it also, and made a communication to your Magazine respecting it\*.

I saw it myself in lat.  $51^{\circ} 43' 50''$  N., and long.  $1^{\circ} 13' 45''$  W. It passed from E. to N.E. at an altitude at first of about  $50^{\circ}$ , declining somewhat towards the end of its course, but not more to my notion than would be caused by perspective, supposing its path to have been on a meridian line, and parallel to a tangent at the earth's surface. The night was very cloudy, but there were many openings between the clouds. The body

\* In the January Number for this year, p. 4.

of the meteor was visible at these points, and appeared round, and certainly not less than 15' in diameter,—I should say double that measure. I was in some degree enabled to judge by estimating, after it had passed, the size of the gaps in the clouds where it was fully visible. The light was very great, enabling me to see surrounding objects as plainly as during a vivid flash of lightning, and lasted about two seconds.

Now to compare my observation with the diagram and notice sent you last year by Sir J. Lubbock, I conclude he must have seen the meteor just before its disappearance; in which case, the course being very much foreshortened, it would occupy the portion of the heavens which he has indicated by a blurred mark of his pencil. On this hypothesis it must have passed about  $8^{\circ}$  or  $10^{\circ}$  from the zenith of his place of observation, which I suppose to be in longitude  $0^{\circ} 4' 5''$  W., lat.  $51^{\circ} 20'$  N.

I consider then that the meteor at the end of the phenomenon bore N. by  $10^{\circ}$  W. at Sir J. Lubbock's station at an altitude of about  $40^{\circ}$ ; at my station at the same instant it bore N.E. at an altitude of  $45^{\circ}$ . From these data, I calculate its height to have been sixty-one miles nearly.

But taking its course as upon a meridian line, and the estimated altitude when due east of me, I make its height about fifty-six miles. Considering the roughness of the data, I regard this degree of accordance, proceeding upon two independent methods, as tolerably satisfactory. Then, if my estimate be at all correct, it had a diameter of at least 700 yards, and its velocity was thirty-six miles in a second.

I remain, Gentlemen,

Your obedient servant,

JOHN SLATTER.

LXI. *On the Aurora Borealis, as it was seen on Sunday evening, October 24, 1847, at Blackheath. By JAMES GLAISHER, Esq., of the Royal Observatory, Greenwich.*

THIS day having been remarkable for one of the most brilliant displays of Aurora Borealis which it has ever been my good fortune to witness, it has occurred to me that a notice of its principal phases, so far as they fell under my own observation, may not be unacceptable to your readers.

The barometer reading during the day previous had declined rapidly, and during this day it had increased as rapidly. The day had been for the most part overcast, and light rain had fallen occasionally; towards evening the sky became per-

*Phil. Mag. S. 3. Vol. 31. No. 209. Nov. 1847. 2 B*



fectly cloudless; the night was beautiful, and the full moon shone with unusual brilliancy.

At about 6<sup>h</sup> 30<sup>m</sup> P.M. a bright red streamer was seen to spring up from the N.W.

At 6<sup>h</sup> 40<sup>m</sup> another streamer was seen in the N.W., and at the same instant one sprung up from the N.; both of which were of a beautiful red.

At 6<sup>h</sup> 56<sup>m</sup> a less brilliant streamer was seen in the N.W., and within three minutes after this time, several faint streamers were seen in the N., N.N.W. and N.W.

From 7<sup>h</sup> to 7<sup>h</sup> 12<sup>m</sup> a few streamers were seen, and after this time no trace of the Aurora could be seen for some time.

Between 7<sup>h</sup> 30<sup>m</sup> and 9<sup>h</sup> 40<sup>m</sup> there were occasional streamers, both white and red, appearing between the N.W. and the E.N.E.

At 9<sup>h</sup> 55<sup>m</sup> a splendid column of red light appeared in the N.W., whose base was about 5° in breadth. This pyramid exhibited all the tints of the most brilliant sunset, and appeared to be composed of streamers whose colours shaded from the most intense crimson into the ruddiest and most brilliant orange, which orange parts again contrasted with the ruddy hue of the next portion, forming by means of contrast upon contrast an endless gradation of shade and colour,—a truly sublime and gorgeous appearance. About this time, the furnace glow which pervaded this appearance increased in intensity, and had all the appearance of the reflexion from an immense conflagration; in the mean time the orange colour entirely disappeared, and gave place to an uniform deep crimson, increasing, as before stated, in intensity, and apparently in denseness.

At 10<sup>h</sup> 0<sup>m</sup> the same appearance continued as above; but in addition to it, there was a collection of vertical columns of light from 2° to 3° in breadth; and from the E.N.E. there was a column similar in form and colour to the one in the N.W., with the exception of being less brilliant. These two red columns formed the east and west boundaries of the fan-like appearance of the whole mass, all the columns of which converged to a point a few degrees S. of the zenith.

The columnar appearances situated between the red columns were of the most silvery light, shaded with a most delicate and pure gray; they were perpetually glancing and shifting upwards and downwards; the lower parts of each column would suddenly glance into the place of the upper portion of the same column, whilst the upper portion would shoot higher towards the zenith, and then both together suddenly descend. This vibrating motion was simultaneous in all the columns, excepting the splendid

red portions at either termination, which remained immoveable, though it rather appeared, that as the central silvery light fluctuated, now bright, now dim, these rosy extremities fluctuated in direct opposition, their rosy hue becoming fainter and inclining to a neutral tint in proportion to the increase of the silvery brightness. The whole variation of appearance somewhat resembled the reflexion cast upon a wall by a Gothic casement lighted from within by some fitful and inconstant light. Towards 10<sup>h</sup> 12<sup>m</sup> a considerable diminution in the brilliancy of the light, fleecy, silvery columns took place; the regular and casement-like appearance disappeared by degrees and assumed more of the character of the extremities, although they still continued their fitful, glancing and radiating motion. During these appearances two or three milk-white, cloud-like masses came up from the N.W. and slowly moved towards the S.E.; each of these masses seemed to have a kind of pulsation within themselves.

At 10<sup>h</sup> 19<sup>m</sup> little could be seen of the Aurora, excepting the red column in the N.W.; this still retained much brilliancy, though all else seemed merged into the sky, when at times, like the bursting of a firework, a stream would spring up from this column, white and brilliant, except at their upper portions, which were tinged with rose colour.

About this time, the moon, which had been shining upon a cloudless sky, was suddenly surrounded with a splendid corona, exhibiting concentric circles, first of a neutral tint, next of violet, then green, and the outermost red; the external boundary of the latter passed nearly midway between the moon and the planet Mars; this appearance continued at its extreme brilliancy a short time only, but more dimly it continued for a long time.

From 10<sup>h</sup> 30<sup>m</sup> to 11<sup>h</sup> 0<sup>m</sup>, with the exception of an occasional streamer, there was no appearance of the Aurora; and at times no aurora at all was visible.

At 11<sup>h</sup> 14<sup>m</sup>, to this time no arch-formation had been seen, or bank of vapour; a bright arch however was supposed to have formed at about this time, but, if so, it continued a short time only.

Shortly after 11<sup>h</sup> 15<sup>m</sup> a faint stream or column of white light was seen in the N.N.E., and a splendid red patch of light, nearly in the east, was seen, which grew very bright, and the phenomenon at midnight exhibited an appearance as beautiful as any of those that had preceded it. An arch appeared extending from the N.W. to the S.E.; from this arch very bright and flickering pencils of light darted out, both upwards and downwards.

At 12<sup>h</sup> 30<sup>m</sup> the streams were frequent; the arch now extended from the N. by W. to the E. by N., and at every part of this arch an occasional streamer, with its taper-like form, sprung up; and this appearance continued till after 13<sup>h</sup>.

I did not observe any halo around the moon at any time, and the Aurora, with the exception of the beautiful white clouds, was confined to the northern hemisphere.

On Friday the 22nd, and on Saturday the 23rd, the magnetic instruments at the Royal Observatory were greatly disturbed, as they were during the auroral appearances on the 24th ultimo\*.

Many of the preceding observations were made by an assistant at my residence, as my own attention was almost completely occupied by observations of the magnetical instruments; so much so, that I was obliged to neglect some of its finest appearances, but which I believe were pretty well observed as above described. The watch by which the times were taken was compared at about midnight, so that the several times are true Greenwich mean solar times.

JAMES GLAISHER.

Blackheath, Oct. 26, 1847.

P.S. An Engraving of its appearance, as seen at about 10<sup>h</sup>, will appear in the Illustrated London News of Oct. 31.

## LXII. *Proceedings of Learned Societies.*

### ROYAL SOCIETY.

[Continued from p. 227.]

June 17, "ON the Solution of Linear Differential Equations." By 1847. Charles James Hargreave, Esq., B.L., F.R.S., Professor of Jurisprudence in University College, London.

1. By the aid of two simple theorems expressing the laws under which the operations of differentiation combine with operations denoted by factors, functions of the independent variable, the author arrives at a principle extensively applicable to the solution of equations, which may be stated as follows:—"if any linear equation  $\phi(x, D).u = X$  have for its solution  $u = \psi(x, D).X$ , this solution being so written that the operations included under the function  $\psi$  are not performed or suppressed, then  $\phi(D, -x).u = X$  has for its solution  $u = \psi(D, -x).X$ ." The solution thus obtained may not be, and often is not, interpretable, at least in finite terms; but if by any transformation a meaning can be attached to this form, it will be found to represent a true result.

An important solution immediately deducible from this principle is given by Mr. Boole in the Philosophical Magazine for February

\* See the weekly reports of the weather supplied by the Astronomer Royal to the Registrar-General.

1847, and is extensively employed in the present paper. It is immediately obtained by making the conversion above proposed in the general equation of the first order and its solution.

2. By the use of this theorem and the general theorems above referred to, the solution of the equation

$$D^2u + 2Q.Du + \left(c^2 + Q^2 + Q' - \frac{m(m+1)}{x^2}\right)u = P,$$

is found in the form

$$u = x^m \varepsilon^{-\int Q dx} (D^2 + c^2)^{m-1} \{x^{-1} (D^2 + c^2)^{-m} (x^{-(m-1)} \cdot \varepsilon^{\int Q dx} \cdot P)\};$$

of which various particular cases and transformations are given and discussed; including the well-known forms

$$D^2u + \frac{2m}{x} Du \pm c^2 \cdot u = P,$$

$$D^2u + bDu + \left(c^2 - \frac{m(m-1)}{x^2}\right)u = P,$$

$$\frac{d^2u}{dz^2} + \left(\frac{c}{2n-1}\right)^2 z^{-\frac{4n}{2n-1}} \cdot u = 0,$$

and extensions of these forms.

The application of the process to equations of the third and higher orders gives rise to solutions of analogous forms; and in particular the equation

$$(a_n x + b_n) D^n u + \dots + (a_1 x + b_1) Du + (a_0 x + b_0) u = X$$

is solved in the form

$$u = (a_n D^n + \dots + a_1 D + a_0)_{-1} \varepsilon^{\frac{b_n}{a_n} D} (D - \alpha)^A (D - \beta)^B \dots \left( x^{-1} \left\{ \varepsilon^{-\frac{b_n}{a_n} D} (D - \alpha)^{-A} (D - \beta)^{-B} \dots X \right\} \right),$$

$$\text{where } \frac{b_n z^n + b_{n-1} z^{n-1} + \dots}{a_n z^n + a_{n-1} z^{n-1} + \dots} = \frac{b_n}{a_n} + \frac{A}{x - \alpha} + \frac{B}{x - \beta} + \dots;$$

and by the application of the theorems first referred to, a still more general form is solved.

The solutions above-mentioned are subject to the important restriction that  $m$ ,  $A$ ,  $B$ , &c. (denoting the number of times that the operations are to be repeated) must be integer; but in the subsequent part of the paper, a mode is suggested of instantaneously converting these solutions into definite integrals not affected by the restriction.

3. The interchange of symbols above suggested frequently renders available forms of solution which otherwise would not be interpretable in finite terms. The operation  $(\phi D)^m$  is not intelligible if  $m$  be a fraction; but if by any legitimate process this be changed into the factor  $(\phi(-x))^m$ , the restriction ceases to operate. By the ap-



plication of this principle, solutions of a simple character are obtained for ( $b$  being integer),

$$(x^2 + c^2)D^2u - 2axDu + b(2a - b + 1)u = P,$$

$$\frac{d^2u}{dt^2} - \frac{b(b+1)}{\cos^2 t}u = P,$$

$$\frac{d^2u}{dt^2} - b(b+2)\frac{1}{(1-t^2)^2}u = P,$$

$$\phi x.D^2u + \psi x.Du + (\psi'x - \phi''x)u = P.$$

4. The advantages of the forms above given in this particular, that the number and order of the operations in the solution are expressed *generally*, and not by a series of substitutions involving changes of the variable as in the ordinary mode of solving Riccati's equation, appear more clearly in the application to partial linear differential equations. Thus, the equation

$$\frac{d^2u}{dx^2} + \frac{2n}{x} \frac{du}{xdxdy} + \left( \frac{n^2}{x^2} - k^2 \right) \frac{d^2u}{dy^2} - \frac{n}{x^2} \frac{du}{dy} - \frac{m(m-1)}{x^2} u = \psi(x, y),$$

which may be solved by  $m$  successive substitutions, receives its solution in the general form

$$u = x^m \varepsilon^{-n \log x} . D' (D^2 - k^2 D'^2)^{m-1}$$

$$\left\{ x^{-1} (D^2 - k^2 D'^2)^{-m} \{ x^{-(m-1)} \varepsilon^{n \log x} D' \psi(x, y) \} \right\};$$

which exhibits at a glance all the successive processes to be performed upon  $\psi(x, y)$  in order to arrive at the result. It will be observed that the process  $\varepsilon^{\phi x} . D'$  performed upon  $\psi y$  denotes  $\psi(y + \phi x)$ . Among other results worthy of notice on this branch of the subject may be noticed the solution of

$$\frac{d^2u}{dpdq} + \frac{a}{p+q} \left( \frac{du}{dp} + \frac{du}{dq} \right) + \frac{a(a-1) - m(m-1)}{(p+q)^2} u = \phi(p, q)$$

(solved by Euler in a series when there is no second term); viz.

$$u = x^{m-a} (D^2 - D'^2)^{m-1} \left\{ x^{-1} (D^2 - D'^2)^{-m} \{ x^{a-m+1} . \psi(x, y) \} \right\};$$

$\psi$  being determined from  $\phi$  by the equations  $\frac{p}{q} = x \pm y$ ; and the solution of

$$(a_n x + b_n) \frac{d^nu}{dx^n} + (a_{n-1} x + b_{n-1}) \frac{d^nu}{dx^{n-1} dy} + \dots + (a_0 x + b_0) \frac{d^nu}{dy^n} = \phi(x, y)$$

which is readily deduced from the solution of the corresponding form in ordinary equations.

5. The character of most of the solutions may be described as follows: they consist in the performance (repeated  $m$  times) of ope-

rations of the form  $\phi D$  upon the second side  $X$ ; multiplication by the factor  $x^{-1}$ ; and the performance (repeated  $m-1$  times) of the inverse operation  $(\phi D)^{-1}$ ; and it will be seen that, in all cases where  $X=0$ , it is sufficient to perform the direct operation  $\phi D$  a single time.

It is a remarkable phenomenon connected with the solutions last mentioned, that they are instantaneously convertible into definite integrals by changing  $\phi D$  into  $\phi z$ , multiplying by  $\epsilon^{zx}$ , changing  $x^{-1}$  into  $D'^{-1}$  ( $D'$  denoting differentiation with regard to  $z$ ), and assigning proper limits for the integral. In this manner definite integrals are immediately found for

$$D^2u + 2Q.Du + \left( Q^2 + Q' - c^2 - \frac{m(m-1)}{x^2} \right) u = 0,$$

$$D^nu + \frac{u}{x} = 0,$$

$$D^nu + x.u = 0,$$

$$(a_nx + b_n)D^nu + .. + (a_0x + b_0)u = 0,$$

and other forms.

6. The application of the principle above stated to equations of finite differences gives solutions for the equations

$$(a_nx + b_n)u_{x+n} + \dots + (a_1x + b_1)u_{x+1} + (a_0x + b_0)u_x = Q_x,$$

$$(a_nx + b_n)\Delta^nu_x + \dots + (a_1x + b_1)\Delta u_x + (a_0x + b_0)u_x = Q_x;$$

and where the number of operations to be performed is denoted by a fraction, solutions are found in the form of definite integrals.

The solution of the first when  $Q_x=0$  is

$$\begin{aligned} u_x = & c_1 \int_0^\alpha (a_nv^n + .. a_1v + a_0)^{-1} v^{b_0} (v-\alpha)^{A_1} (v-\beta)^{A_2} \dots v^{x-1} dv \\ & + c_2 \int_0^\beta (a_nv^n + .. a_1v + a_0)^{-1} v^{b_0} (v-\alpha)^{A_1} (v-\beta)^{A_2} \dots v^{x-1} dv \\ & + \&c.; \end{aligned}$$

and that of the second is somewhat similar.

From some investigations effected by interchanging the symbols  $x$  and  $D$  in the solution of the general linear equation in finite differences of the first order, it would seem that definite summations may be used to represent the solutions of certain forms of equations. Thus a partial solution of

$$\epsilon^{-x}.u - D^nu = c$$

is  $c\Sigma(\Gamma z)^n \epsilon^{zx}$  from  $z = -\alpha$  to  $z = 0$ .

7. In attempting the solution of some equations by means of successive operations, not consisting exclusively of  $D$  combined with

constants, but involving also functions of  $x$ , the only result which appeared to the author worthy of notice is the solution of

$$D^2u + bDu + c^2u - n(n+1)\frac{u}{\cos^2x} = X;$$

from a particular case of which, the general solution of Laplace's equation,

$$\frac{d}{d\mu}\left((1-\mu^2)\frac{du}{d\mu}\right) + \frac{1}{1-\mu^2}\frac{d^2u}{dy^2} + n(n+1).u = 0,$$

may be found in the simple form

$$u = e^{\tan^{-1}(\mu\sqrt{-1})} \frac{d}{dy} \left( \frac{d}{d\mu} \right)^n \left\{ (1-\mu^2)^n \varphi(y - 2\tan^{-1}(\mu\sqrt{-1})) \right\},$$

with a similar function using  $-\sqrt{-1}$  for  $\sqrt{-1}$ .

#### CAMBRIDGE PHILOSOPHICAL SOCIETY.

[Continued from p. 311.]

On the Symbolical Equation of Vibratory Motion of an Elastic Medium, whether Crystallized or Uncrystallized. By the Rev. M. O'Brien, late Fellow of Caius College, Professor of Natural Philosophy and Astronomy in King's College, London.

The object of the author in this paper is twofold: *first*, to show that the equations of vibratory motion of a crystallized or uncrystallized medium may be obtained in their most general form, and very simply, without making any assumption as to the nature of the molecular forces; and *secondly*, to exemplify the use of the symbolical method and notation explained in two papers read before the Society during the present academical year.

First, with regard to the method of obtaining the equations of vibratory motion.

This method consists in representing the *disarrangement* (or state of relative displacement) of the medium in the vicinity of the point  $xyz$  by the equation

$$\delta v = \frac{dv}{dx} \delta x + \frac{dv}{dy} \delta y + \frac{dv}{dz} \delta z + \frac{1}{2} \frac{d^2v}{dx^2} \delta x^2 + \frac{d^2v}{dxdy} \delta x \delta y + \&c. - \&c.$$

(where  $v = \xi\alpha + \eta\beta + \zeta\gamma$ ,  $\xi\eta\zeta$  denoting, as usual, the displacements at the point  $xyz$ , and  $\alpha\beta\gamma$  being the *direction units* of the three coordinate axes), and in finding the *whole* force brought into play at the point  $xyz$  (in consequence of this disarrangement) by the *symbolical addition* of the different forces brought into play by the several terms of  $\delta v$ , each considered separately. It is easy to see that these different forces may be found with great facility, without assuming anything respecting the constitution of the medium more than this, that it possesses *direct* and *lateral elasticity*. By *direct elasticity* we mean that elasticity in virtue of which *direct* or *normal* vibrations

take place ; and by *lateral*, that in virtue of which *lateral* or *transverse* vibrations take place.

The forces due to the several terms of  $\delta v$  are obtained by means of the following simple considerations.

Let AB be any line in a perfectly uniform medium, and conceive the medium to be divided into elementary slices by planes perpendicular to AB ; let OM(= $x$ ) be the distance of any slice PP' from any particular point O of AB, and suppose this slice to suffer a displacement equal to  $\frac{1}{2} cx^2$  ( $c$  being a constant) in the direction OAB, and the other slices to be similarly displaced. Then it is evident that the medium suffers by these displacements a uniformly increasing *expansion* in the direction OB, and a uniformly increasing *condensation* in the direction OA ; the rate of increase both of the expansion and condensation being  $c$ . Now in all known substances, whether solid, fluid, or gaseous, a disarrangement of this kind would bring into play on the slice O a force along the line AB proportional to the rate of increase  $c$ , *i. e.* a force  $Ac$ ,  $A$  being a constant depending upon what we may call the *direct elasticity* of the substance.

Again, suppose that the slice PP' receives a displacement  $\frac{1}{2} cx^2$  in the direction OC *perpendicular* to AB, and the other slices similar displacements. Then the line AB will become curved into a parabola A'OB', and all the lines of the medium parallel to AB will be similarly curved, the radius of curvature being equal to  $\frac{1}{c}$  and perpendicular to AB. Now in all known substances\* a disarrangement of this kind would bring into play upon the slice O a force in the direction OC proportional to the curvature  $c$ , *i. e.* a force  $Bc$  depending upon what we may call the *lateral elasticity* of the substance.

Lastly, suppose that MP= $y$ , and that the point P of the medium receives a displacement  $cxy$  parallel to AB, and the other points similar displacements. Then the slice PP' will, in consequence of this kind of displacement, turn through an angle  $\tan^{-1}(cx)$  into the dotted position, and the other slices will suffer similar rotations, those on the other side of O, such as QQ', turning the opposite way. Now it is easy to see that a disarrangement of this kind produces a uniformly increasing expansion in the direction OC, and a uniformly increasing condensation in the direction OC', the rate of increase both of the expansion and condensation being  $c$ . But the expansion and condensation here described are quite different from that previously noticed ; since it is produced, not by displacements parallel to C'C, but by *lateral* displacements, *i. e.* *perpendicular* to C'C. On this account all that we can assert without further investigation is, that the force brought into play upon an element at O by this disarrangement acts along the line C'C, and is proportional to  $c$ , *i. e.* equal to  $Cc$ , where  $C$  is some constant evidently depending in some way both upon the *direct* and *lateral* elasticity of the medium.

\* Fluids and gases possess lateral elasticity as well as solids, only in a comparatively feeble degree.



There is however a very simple way of finding the precise value of the force brought into play by a disarrangement of this kind; for if we turn the axes of  $x$  and  $y$  in the plane of the paper through an angle of  $45^\circ$ , it appears that this disarrangement is nothing but a combination of the two kinds of disarrangement previously noticed; and from this it immediately follows, in the case of an uncrystallized medium, that the force brought into play at  $O$  is  $(A - B)c$ ; in other words, the coefficient  $C$ , which must be multiplied into  $c$ , in order to give the force brought into play by the disarrangement  $cxy$ , is equal to the coefficient of direct elasticity ( $A$ ) minus the coefficient of lateral elasticity ( $B$ ).

In the case of a crystallized medium, it may be shown that *six relations*, corresponding to the relation  $C = A - B$ , are most probably true, and are *essential* to Fresnel's theory of transverse vibrations; that is to say, the medium is capable of propagating waves of transverse vibrations if these six conditions hold, but otherwise it is not.

In employing the above considerations to determine the equations of vibratory motion, the directions  $AB$  and  $C'C$  are always taken so as to coincide with some two of the three coordinate axes; and it is this circumstance that makes the method peculiarly applicable to crystallized media. Indeed, if it were necessary to take the lines  $AB$  and  $C'C$  in any directions but those of the axes of symmetry, the above considerations would not apply without considerable modification.

The equations of vibratory motion obtained by this method for an uncrystallized medium, are the well-known equations involving the two constants  $A$  and  $B$ . The equations obtained for a crystallized medium are perfectly free from any restriction of any kind, are applicable to all kinds of substance, whether we suppose its structure to be analogous to that of a solid fluid or gas, and hold for all kinds of disarrangement, whether consisting of normal or transverse displacements, or both.

When we introduce the six relations between the constants above alluded to, and moreover assume that the vibrations constituting a polarized ray are *in* the plane of polarization, we arrive at Professor MacCullagh's equations\*. If, on the contrary, we suppose the vibrations to be *perpendicular* to the plane of polarization, we arrive at equations which agree exactly with Fresnel's theory in every particular†.

If we introduce these six relations into the equations for crystallized media deduced from M. Cauchy's hypothesis, that the molecular forces act along the lines joining the different particles of the medium, it will be found that these equations are immediately reduced to the equations for an uncrystallized medium. From this it follows that M. Cauchy's hypothesis cannot be applied to any but uncrystallized media. In fact, it may be easily proved that if the

\* Given in a paper read to the Royal Irish Academy, Dec. 9, 1839, page 14.

† On this subject see a paper by the late Mr. Greene in the seventh volume of the Cambridge Transactions, p. 121.

equations derived from this hypothesis be true, a crystallized medium is incapable of propagating transverse vibrations.

*Secondly, respecting the use of the symbolical method and notation above alluded to.*

The application of the *symbolical method and notation* to the subject of vibratory motion is very remarkable, and leads to equations of great simplicity. In the case of an uncrystallized medium, the three ordinary equations of motion are included in the single symbolical equation

$$\frac{d^2v}{dt^2} = B \left\{ \frac{d^2}{dx^2} + \frac{d^2}{dy^2} + \frac{d^2}{dz^2} \right\} v + (A - B) \left( \alpha \frac{d}{dx} + \beta \frac{d}{dy} + \gamma \frac{d}{dz} \right) \left( \frac{d\xi}{dx} + \frac{d\eta}{dy} + \frac{d\zeta}{dz} \right).$$

If we employ the notation  $\Delta u'.u$ , and assume the symbol  $\mathfrak{D}$  to represent the operation

$$\alpha \frac{d}{dx} + \beta \frac{d}{dy} + \gamma \frac{d}{dz},$$

the equation of motion becomes

$$\frac{d^2v}{dt^2} = B(\Delta \mathfrak{D}.\mathfrak{D})v + (A - B) + \Delta \mathfrak{D}.v;$$

or, by using the notation  $Du'.u$  also, it may be put in the form

$$\frac{d^2v}{dt^2} = \{ A \mathfrak{D} \Delta \mathfrak{D} - B(D \mathfrak{D}.)^2 \} v.$$

The symbol  $\mathfrak{D}$  written before any quantity  $U$  which is a function of  $xyz$ , has a very remarkable signification; the *direction unit* of the symbol  $\mathfrak{D}U$  is that direction *perpendicular* to which there is no variation of  $U$  at the point  $xyz$ , and the *numerical magnitude* of  $\mathfrak{D}U$  is the *rate of variation* of  $U$ , when we pass from point to point *in that direction*.

The symbols  $\Delta \mathfrak{D}.v$  and  $D \mathfrak{D}.v$  have also remarkable significations.  $\Delta \mathfrak{D}.v$  is a numerical quantity representing the *degree of expansion*, or what is called the *rarefaction* of the medium at the point  $xyz$ .  $D \mathfrak{D}.v$  represents, in magnitude, the degree of *lateral disarrangement* of the medium at the point  $xyz$ , and, in direction, the *axis* about which that displacement takes place.

These two symbols may be found separately by the integration of an equation of the form

$$\frac{d^2U}{dt^2} = C \left( \frac{d^2U}{dx^2} + \frac{d^2U}{dy^2} + \frac{d^2U}{dz^2} \right).$$

When the six conditions above alluded to are introduced, the equation of motion for a crystallized medium becomes

$$\begin{aligned} \frac{d^2v}{dt^2} = & \left( A_1 \alpha \frac{d}{dx} + A_2 \beta \frac{d}{dy} + A_3 \gamma \frac{d}{dz} \right) \Delta \mathfrak{D}.v \\ & + D \mathfrak{D}. \left\{ \left( B_2 \frac{d\eta}{dz} - B'_2 \frac{d\zeta}{dy} \right) \alpha + \left( B_3 \frac{d\zeta}{dx} - B'_1 \frac{d\xi}{dz} \right) \beta + \right. \\ & \left. \left( B_1 \frac{d\xi}{dy} - B'_3 \frac{d\eta}{dx} \right) \gamma \right\}, \end{aligned}$$

where  $A_1 A_2 A_3$  are the three coefficients of *direct* elasticity with reference to the three axes of symmetry, and  $B_1 B_1' B_2 B_2' B_3 B_3'$  the six coefficients of *lateral* elasticity with reference to the same axes.

If the vibrations be transverse, this equation is reducible to the form

$$\begin{aligned}\frac{d^2v}{dt^2} &= -(D\mathbb{D}.)^2(a^2\xi\alpha + b^2\eta\beta + c^2\zeta\gamma) \\ &= -(D\mathbb{D}.)^2(a^2\alpha\Delta\alpha + b^2\beta\Delta\beta + c^2\gamma\Delta\gamma)v,\end{aligned}$$

assuming the vibrations of a polarized ray to be *perpendicular* to the plane of polarization.

The well-known condition that a plane polarized ray may be transmissible without subdivision, and the velocity of propagation may be immediately deduced from this equation.

If we assume the vibrations of a polarized ray to be *in* the plane of polarization, the equation becomes

$$\frac{d^2v}{dt^2} = -D\mathbb{D}.(a^2\alpha\Delta\alpha + b^2\beta\Delta\beta + c^2\gamma\Delta\gamma)D\mathbb{D}.v.$$

This includes Professor MacCullagh's three equations.

#### ROYAL ASTRONOMICAL SOCIETY.

[Continued from p. 146.]

May 14, 1847.—Extract of a letter from Mr. Adams, with new Elements of Neptune.

"The following elements of Neptune have been obtained by taking into account Prof. Challis's observations made since the reappearance. \* \* \* The elements are now sufficiently correct to enable me to approximate to the perturbations of Neptune by the action of Uranus, in order to compare more accurately the ancient observations of 1795 with those . . . made recently. I have used the old observations, supposing the elements not to have changed. I hope immediately to set about a new solution of the perturbations of Uranus, starting with a very approximate value of the mean distance. \* \* \* I do not think, with Professor Pierce, that the near commensurability of the mean motions will interfere seriously with the results obtained by the treatment of perturbations; but it will be interesting to see how nearly the real elements can be obtained by means of the perturbations."

#### *Elements of the Orbit of Neptune.*

Mean longitude, Jan. 1, 1847, G. M. T...	328° 13' 54.5"	} M. Eq. 1847.0
Longitude of perihelion (on the orbit)...	11 13 41.5	
Longitude of ascending node .....	130 5 39.0	
Inclination to ecliptic .....	1 47 1.5	
Mean daily motion .....	21.3774	
Semi-axis major.....	30.2026	
Eccentricity of orbit .....	0.0083835	

On the communication of Mr. Adams's paper, the Astronomer

Royal gave orally a continuation of the history of Neptune, embracing the principal points that have been ascertained since his communication of Nov. 14, 1846. The planet having been actually discovered in the heavens by means of certain predicted elements, the fair presumption was that those elements were very approximately correct. Adopting these elements, therefore, Mr. Hind examined Lalande's and other observations, with the hope of finding some former observation of the planet as a star now missing, but satisfied himself that there was none. In the meantime, the continuation of the observations of the planet in the last months of 1846, and the comparison of them with Professor Challis's early observations of August, led to some unexpected conclusions. It was found that, though one *place* of the planet might be very well represented by M. Le Verrier's or Mr. Adams's elements, yet the apparent *movement* of the planet could not be represented within several minutes. Elements were then investigated from the observations themselves (without any reference to the preceding deductions from the perturbations of Uranus) by Mr. Adams in England (see Monthly Notices for March, p. 244), and by Professor Pierce and Mr. Sears C. Walker in America. Attention is particularly due to the former of these investigations, in which are exhibited, not only the results for the different elements, but also for the probable error of each. The most important conclusion was, that the planet certainly moved in a much smaller orbit, and probably in an orbit of much smaller eccentricity, than that indicated by the calculations of perturbation. With elements thus roughly corrected, the orbit was again traced back through the ancient observations; and it was found by Dr. Petersen of Altona, and Mr. Sears C. Walker, that a star observed by Lalande on May 10, 1795, and now missing from the heavens, was very probably the planet. The observation however was marked doubtful in Lalande's printed volume: and to this circumstance is probably due a most remarkable discovery. The manuscripts of Lalande's observations were some years ago transferred by his representatives to the observatory of Paris. To examine into the presumption of doubt in the observation, the astronomers of the Observatory of Paris referred to the originals, and there they found that the observation of May 10, 1795, was entered without any expression of doubt at the time; that an observation of May 8, 1795, was omitted in the printed volume; that it was omitted solely because it could not be reconciled with the observation of May 10; and that, upon reducing both properly, they exhibit most distinctly the retrograde motion of a planet nearly parallel to the plane of the ecliptic, the right ascension and the polar distance having both changed in the proper proportion. It seems now inconceivable to us that an astronomer, having his attention strongly called to the difference between the two days' results, should rather assume that there were in the observations two independent errors (one of right ascension and one of polar distance), than that the body observed was really a planet. With the place of the planet at an epoch so distant, its elements are ascertained with great accuracy.



It is remarkable that the missing star, to which allusion has been made, is actually entered as an observed star in the Berlin Star-Map; and this circumstance prevented Mr. Adams from tracing the new orbit of the planet so soon as he would otherwise have done. This insertion of an unobserved star can be accounted for only on the supposition that the star had been taken by the observer in his working-catalogue as a zero-star, and had then been inserted as a matter of course.

The mean distance of Neptune from the sun now appears, instead of 38, to be something near 30; and its periodic time, instead of 220 years, to be nearly 166. It is certainly a most curious thing (in which much is owing to chance) that elements, now known to be extremely erroneous, should have accounted for the perturbations of Uranus through 150 years with such accuracy, and should also have given the planet's place, *for the particular year in which the attention of astronomers was first strongly directed to it*, with such precision. It remains to be seen whether the new elements of Neptune will, with any possible mass, explain the perturbations of Uranus. In any case, Bode's law, on the assumption of which the original investigations of M. Le Verrier and Mr. Adams entirely depended, fails completely.

Calcul détaillé d'une Inégalité Nouvelle à Longue Période, qui existe dans la Longitude moyenne de la Lune. By M. Hansen.

The author states that he has lately made known to some astronomers a discovery of two inequalities in the motion of the moon, whose periods are respectively nearly 273 and 239 years. Denoting by  $g$ ,  $g'$ ,  $g''$  the geocentric mean anomaly of the moon, and the heliocentric mean anomalies of the earth and Venus, these inequalities are—

$$27''\cdot4 \times \sin(-g - 16g' + 18g'' + 35^\circ 20'\cdot2) \\ + 23''\cdot2 \times \sin(8g'' - 13g' + 315^\circ 30');$$

of which the first depends on a new argument, while the second depends on the argument of an equation of long period in the motion of the earth, discovered by Mr. Airy.

As the calculation of those parts of the coefficients which depend on the product of the square and cube of the sun's disturbing force by the disturbing force of Venus is extremely laborious, and is moreover connected with other unpublished calculations of other inequalities of the moon, it does not appear possible to publish it at present. Indeed M. Hansen does not consider himself able yet to answer for their perfect correctness, though he has the strongest reason to believe that they are very nearly correct. The present paper therefore includes only the calculation of that part of the coefficient of the first inequality which depends on the first power of the disturbing force.

It appears difficult to abstract very completely the remainder of this paper, but the following indications will enable a person acquainted with the developments of physical astronomy to follow the whole process.

The perturbing function  $\Omega$  for the moon as disturbed by Venus being formed, it will be found that it may be expanded in a rapidly

converging series of fractions, whose numerators contain successive powers of  $r$ , the moon's radius vector, and whose denominators contain different powers of the same multinomial (which, when eccentricities and inclinations are omitted, is a trinomial) that occurs in computing the perturbations of the earth by Venus. Upon expanding any of these fractions with trinomial denominator, there occur terms depending on  $16g'' - 16g'$ ,  $17g'' - 17g'$ , and  $18g'' - 18g'$ : then, upon introducing the inclinations and eccentricities, the first (among other combinations) will be multiplied by  $\sin^2 \frac{1}{2}$  inclin.  $\times \cos 2g'' - 2v$  (where  $v$  is the difference of longitude of node and perihelion of Venus), and also (in other terms) by  $e''^2 \cos 2g''$ ; the second by  $e'' \cdot e' \cos g'' + g'$ ; and the third by  $e'^2 \cos 2g'$ . Each of these combinations produces terms whose argument is  $18g'' - 16g'$ . Then upon multiplying these terms by a power of  $r$ , since the expression for any power of  $r$  contains  $e \cos g$ , the product will contain terms depending on  $18g'' - 16g' - g$ . The coefficient necessarily contains one of the following products of three small quantities:  $e \sin^2 \frac{1}{2}$  inclin.,  $e \cdot e''^2$ ,  $e \cdot e'' \cdot e'$ ,  $e \cdot e'^2$  (of which the first is the most important), and it is therefore extremely small; but the resulting perturbation is made important by the excessive smallness of the divisor introduced in integration. It is well known that the divisor in this case will be proportional to  $\left(18 \frac{dg''}{dt} - 16 \frac{dg'}{dt} - \frac{dg}{dt}\right)^2$ ; and, taking for  $\frac{dg''}{dt}$ , &c., the value in sexagesimal seconds corresponding to a Julian year.

$$\frac{dg''}{dt} = 2106641''.3$$

$$\frac{dg'}{dt} = 1295977.4$$

$$\frac{dg}{dt} = 17179157.4$$

$$\text{whence } 18 \frac{dg''}{dt} - 16 \frac{dg'}{dt} - \frac{dg}{dt} = 4747''.7,$$

a quantity very small in comparison with  $\frac{dg}{dt}$ .

In this manner the greatest part of the term in question is produced. Other parts arise from the circumstance that, the dimensions of the moon's orbit being slightly altered, the perturbing force of the sun upon the moon is not the same as it would otherwise be.

M. Hansen remarks that this term is remarkable as depending upon higher multiples of the anomalies than have ever before been considered, and as having the longest period in proportion to the periodic time of the disturbed body that is yet known.

The term depending on  $8g'' - 13g'$  arises mainly from the circumstance, that, the earth's motion in its orbit being different from what it would have been without the perturbation by Venus, the disturbing force of the sun upon the moon is not the same as if that perturbation had not existed.

M. Hansen states that he has examined several inequalities of long period in the moon's motion which hitherto have escaped notice, but that in no other instance does the coefficient amount to  $1''$ .

In concluding the account of this remarkable discovery, it is gratifying to add that it explains almost precisely the observed inequality in the moon's mean motion, which for the last fifty years has troubled physical astronomers.

After the reading by the Secretary of a portion of this paper, the Astronomer Royal gave an oral explanation of its general subject in the following manner:—

The disturbing effect of Venus upon the moon is not the whole attraction of Venus upon the moon, but the difference of the two attractions, of Venus upon the moon and of Venus upon the earth. Thus, when the moon is between the earth and Venus, the attraction of Venus upon the moon is stronger than that of Venus upon the earth, and therefore it tends to pull Venus away from the earth. When the moon is more distant from Venus than the earth is, the attraction of Venus on the earth is the stronger, and tends to pull it away from the moon, which, in regard to the disturbance of the relative places of the earth and moon, is the same thing as pulling the moon away from the earth. In both these positions, therefore, the disturbing force of Venus tends to pull the moon away from the earth. When the earth and the moon are equally distant from Venus, the attractions of Venus upon the two are equal, but not in parallel lines; the attractions tend to draw them along the sides of a wedge whose point is at Venus, and, therefore, to diminish the distance between them, or to push the moon *towards* the earth.

Inasmuch as, in one pair of positions of the earth and moon, the disturbing force of Venus tends to increase the distance between them, and in another pair of positions it tends to diminish that distance, it is important to ascertain which of these disturbances is the greater. Suppose the distance of the moon from the earth to be  $\frac{1}{100}$  part of the distance of the earth from Venus. Then, when the moon is between the earth and Venus, its distance from Venus is  $\frac{99}{100}$  of the whole; the force upon it is  $\frac{10000}{9801}$  of that upon the earth; the excess of this (or the disturbing force tending to pull the moon away from the earth) is  $\frac{199}{9801}$ , or nearly  $\frac{1}{50}$  of that on the earth. In like manner, when the moon is further from Venus than the earth is, its distance from Venus is  $\frac{101}{100}$  of the earth's distance; the force upon it is  $\frac{10000}{10201}$  of that upon the earth; the defect of this (or the disturbing force tending to pull the earth away from the moon) is  $\frac{201}{10201}$ , or nearly  $\frac{1}{51}$  of that on the earth. But when the earth and the moon are at equal distances from Venus, the proportion of their relative approach (as produced by the action of Venus) to the whole effect of Venus upon them, is evidently represented by the inclination of the two lines drawn from them to Venus, or is the same as the proportion of the distance of the moon from the earth, to the distance of the earth from Venus, and is therefore  $\frac{1}{100}$  of the whole. Thus the force tending to pull the moon from the earth at one time is about double the force tending to push the moon towards the earth at another time; and therefore, upon the whole, the tendency of the

disturbing force of Venus is to pull the moon from the earth. To arrive at this conclusion, we have considered only four points of the moon's orbit: in other points the effects of the perturbation are more complicated; but they do not alter this general conclusion.

The same remark applies to the disturbing effect of Venus upon the moon when at a given point of its orbit, provided the nature of that point be such that at different times it is in all possible positions relative to Venus. For instance, the moon's apogee is (in consequence of the motion of the line of apses, and of the relative motions of the earth and Venus) sometimes between the earth and Venus, sometimes more distant from Venus than the earth is, sometimes  $90^\circ$  to the right, sometimes  $90^\circ$  to the left. We may assert therefore that, upon the whole, the disturbing force of Venus upon the moon, when she is in apogee, tends to draw her away from the earth. The same may be predicated when the moon is in perigee.

Next, it is important to ascertain how the disturbing force depends upon the moon's distance from the earth. For this purpose, instead of supposing, as before, that the moon's distance is  $\frac{1}{10}$  part of the distance of the earth from Venus, let us suppose it  $\frac{2}{10}$  part of that distance. Then when the moon is between the earth and Venus, the force upon the moon is  $\frac{1}{9}$  of that upon the earth, and therefore the excess, or the disturbing force, is  $\frac{2}{9}$ , or nearly  $\frac{1}{5}$  of the whole force upon the earth. In the former assumed instance it was  $\frac{1}{10}$ . Thus, upon doubling the moon's distance from the earth, the disturbing force is doubled. And similarly for other distances of the moon from the earth, the disturbing force (in similar positions with regard to Venus) is proportional to the moon's distance. Thus, when the moon is at apogee, in a given position with regard to Venus, the disturbing force is greater than when the moon is in perigee in the same position. And, upon the whole, in all possible relative positions of the moon and Venus, the action of Venus pulls away the moon from the earth, more when she is in apogee than when she is in perigee.

Now we may consider the general effect of these forces upon the dimensions of the moon's orbit. So long as the force which draws the moon towards the earth is always the same at the same distance, the moon will continue to describe an orbit of the same dimensions over and over again. But if at any time the force directed towards the earth *suddenly* grows smaller, the moon will *immediately* rush off in an orbit which, on the opposite side, is larger. If the force towards the earth *gradually* grows smaller, the dimensions of the orbit will *gradually* increase. And the periodic time in the orbit described at every successive revolution will undergo the change corresponding to the change of dimensions (that is, to the change of major axis) of the orbit, and will therefore become continually greater and greater.

These are the changes which produce the most serious disturbance in the apparent place of the moon. If a force, after acting for a long time, produce a small change in the eccentricity of the moon's orbit, the effect on the moon's place is simply the amount of the corre-



sponding change in the equation of the centre, and cannot possibly exceed that amount. But if the force have been for a long time gradually altering the major axis, and consequently the periodic time in the moon's orbit, then during the whole of that time the moon has been performing her revolutions quicker or slower than we expected, and therefore at the end of that time she is in advance or in retard of her expected place by an amount equal to the accumulation of all the advances or retards in all the revolutions through which the change has been going on. The planetary inequalities of long period are all of this kind. The major axis here plays the same part as the pendulum of a clock. If a small force acting for a year pushed the seconds-hand forwards by an inch, the clock would be merely a few seconds wrong; but if in the same time it shortened the pendulum by an inch, the clock would have gained fifty hours; and if the time occupied by the change had been greater, the disturbance in the clock indication would have been proportionably greater.

In order then to find inequalities of long period in the motion of the moon produced by Venus, we must seek for some alternate increase and decrease, occupying a very long period, in the force by which Venus draws the moon from the earth.

No such slow increase and decrease have been found in the general force by which Venus disturbs the moon.

The next point of inquiry is, whether a combination of the changes in the force of Venus with the changes in the position of the moon in its orbit can produce a force, which, for a very long time together, gradually increases the force drawing the moon from the earth, and then for an equal time gradually diminishes that force.

A force which acts in opposite ways, nearly on opposite sides of the moon's orbit (pulling the moon from the earth on one side and pushing it towards the earth on the other side), may produce this effect, provided the period of the change in the nature of the force (from pulling to pushing) correspond *nearly*, but *not exactly*, with the time in which the moon moves from apogee to perigee. For (as we have seen) the effect of a certain force of Venus is to produce a greater disturbing force on the moon at apogee than at perigee; and this force, or a change in this force, will, at apogee, produce a greater effect on the dimensions of the moon's orbit than at perigee, both because the disturbing force is actually greater, and because it acts on the moon when the moon's velocity is smaller. Therefore, if a pulling force, gradually increasing in magnitude, act on the moon at apogee, it will gradually increase the dimensions of the moon's orbit: if a corresponding pushing force act at perigee, it will gradually diminish the dimensions of the moon's orbit; but the former prevails, and the orbit will gradually increase in size. If after a time the pulling force at apogee gradually diminish, and at length become a pushing force, while the pushing force at perigee gradually diminishes, and at length becomes a pulling force, then the orbit will gradually diminish in size. And this change of forces would be produced by such a modification in Venus's force, as that

of which we have spoken, namely, a force which acts in opposite ways on opposite sides of the moon's orbit, and in which the period in the change of the nature of the force coincides *nearly*, but *not exactly*, with the time in which the moon moves from apogee to perigee; for then the pulling force at apogee will after a long time be changed to a pushing force, and the pushing force at perigee will in the same time be changed to a pulling force. If, for instance, the change in the disturbing forces of Venus (from pushing to pulling) occupied fourteen days exactly, and if the moon's motion from apogee to perigee occupied fourteen days and five minutes, then in 4032 anomalistic semi-revolutions of the moon (which would bring her from apogee to apogee), there would have been 4033 changes of the force (which would change it from pulling to pushing), and therefore in this time, and no sooner, a complete pulling force at apogee would be changed to a complete pushing force at apogee.

It is necessary now to point out how such a modification of the force of Venus can be found.

The only disturbing forces which are yet completely brought under the management of mathematicians are of two kinds; a constant force (always pushing or always pulling with the same amount of force), and a force alternately pushing and pulling, having equal periods and equal maximum magnitudes in each state. The latter of these, if projected graphically, with the time for abscissa, is represented by the ordinates of a *line of sines*: algebraically, it is expressed by  $a \cdot \cos(bt + c)$ .

Now, while the relative positions of the earth and Venus change, the disturbing force on the moon (estimated by the force which, on the whole, it exerts to pull the moon from the earth) undergoes very great changes. When Venus is nearest to the earth, this force is about 250 times as great as when Venus is furthest from the earth. It declines very rapidly from its greatest magnitude. If therefore we represent the disturbing force from one conjunction to the next by a curve, this curve will be very high at the beginning and end, and very near the line of abscissa at the middle, and through the greater part of its extent.

The separation of this force into a number of different forces, following the two laws mentioned above, is effected by a process suggested and facilitated by algebra, but in which, nevertheless, every step has its physical meaning. It may be stated at once, that this remark applies universally to the algebraical operations of physical mathematics. As a simple instance, we may refer to the equation  $(a + b)^3 = a^3 + 3a^2b + 3ab^2 + b^3$ , which probably was suggested by algebra; but which may be illustrated by taking a cube, whose side is  $a + b$ , and (by three saw-cuts) cutting it into eight pieces, when the single piece representing  $a^3$ , the three pieces each representing  $a^2b$ , the three pieces each representing  $ab^2$ , and the single piece representing  $b^3$ , will be found. And there is perhaps no better discipline for the mind than thus tracing the evidence of the truth of algebra, especially in its more profound processes.

The separation, then, of the force of Venus goes on by the following steps:—

1st. A constant pulling force, equal to the mean value of the force.

2nd. A force pulling when Venus is in conjunction, pushing at the time intermediate to the conjunctions, and pulling when Venus is in conjunction again; thus going completely through its changes *once* between conjunction and conjunction.

3rd. A force pulling when Venus is in conjunction, then pushing, &c., going through its changes *twice*.

4th. A force pulling when Venus is in conjunction, then pushing, &c., going through its changes *thrice*.

In this manner the forces go on, continually diminishing in magnitude. When we arrive at the 18th, the force is extremely small.

The algebraical expression for the collection of these terms, putting  $\theta$  for the difference of mean longitude of the earth and Venus, is

$$A + B \cdot \cos \theta + C \cdot \cos 2\theta + D \cdot \cos 3\theta + \&c.$$

This is on the supposition that the orbits of the two planets are circular and in the same plane. But, in consequence of their eccentricities and inclinations, the forces of any one system alternately pushing and pulling (Nos. 2, or 3, or 4, &c.) will not have the same *maximum* magnitude throughout. But each can, in all cases, be expressed by the combination of three such forces, in each of which the maximum forces are equal throughout. Thus, if we combine a large force, going through its changes twenty times in a certain period, with a small force going through its changes nineteen times in the same period, and another small force going through its changes twenty-one times in the same period, then it will be found that both the small forces increase the large force (whether in its pulling or in its pushing state) near the beginning and the end of the time; that both diminish the large force near the middle of the time; and that the two small ones destroy each other at a quarter and three-quarters of the time. The effect of this combination is therefore precisely such as is spoken of above.

Thus, then, for the complete expression of the force, we are driven to an infinite number of forces following the law of alternately pulling and pushing, but with very great variety of magnitudes of force and of periodic time. The greatest portion of these produce no sensible effect; some because (though their magnitudes are large) they act for so short time in one way, or their periods are so little related to the periods of any movement of the moon, that their effects never accumulate; others because their magnitudes are small, and there is no unusual circumstance favourable to their increase.

But there is one of these forces which, in the algebraical expression, depends on  $18 \times$  mean longitude of Venus  $- 16 \times$  mean longitude of the earth, whose coefficient is exceedingly small, but which goes through all its changes, from pulling to pushing again, in the time,

$$27^d 13^h 7^m 35^s.6;$$

or from pulling to pushing, in the time

$$13^d 18^h 33^m 47^s.8.$$

Now, the anomalistic revolution of the moon, from apogee to apogee again, is performed in the time

$27^d\ 13^h\ 18^m\ 32^s.3;$

or from apogee to perigee, in the time

$13^d\ 18^h\ 39^m\ 16^s.1.$

Here we have a real instance, exactly corresponding to the case which, for the sake of explanation, we assumed a short time back, and the results are truly such as were there described. During about 4000 half-revolutions of the moon, or 2000 revolutions, the pulling force at apogee is gradually diminishing till it becomes a pushing force, and during about 2000 more revolutions, the pushing force at apogee is gradually diminishing till it becomes again a pulling force; the opposite changes going on in the force at perigee: and thus, for reasons fully explained before, the moon's orbit is gradually contracting during 2000 revolutions, and gradually expanding during 2000 revolutions more. And although the change in the size of the orbit is totally insensible in observation (for, according to a rough calculation, the utmost accumulation of change in the major axis of the moon's orbit is only ten *feet*, sometimes in increase and sometimes in decrease), yet the consequent alteration in its periodic time, continued through so many revolutions, is sufficient to cause the irregularity in question. The inequality in longitude, as measured on the moon's orbit, exceeds thirty *miles*, sometimes in advance, and sometimes in retard.

For a complete understanding of this matter, it must carefully be borne in mind that the force at the apogee, which has been described as a pushing force through 136 years, is not absolutely a pushing force through every month of that time, but that (in consequence of the motion of the moon's line of apsides) if we take any period of nine or ten years, the moon's apogee will in that time have passed through every position with regard to Venus, and therefore, *upon the whole*, during that period of nine or ten years, the force at apogee will have been a pushing force. In like manner, in another period of 136 years, if we take any period of nine or ten years, *upon the whole*, during that period of nine or ten years, the force at apogee will have been a pulling force.

The general cause of the inequality depending on the argument  $8g'' - 13g'$ , has been sufficiently stated in one of the last paragraphs of the abstract of M. Hansen's paper.

### LXIII. *Intelligence and Miscellaneous Articles.*

#### ON THE GELATINOUS SUBSTANCES OF VEGETABLES.

**M.** FREMY, in a memoir read before the Academy of Sciences, has arrived at the following conclusions:—

1st. There exists in vegetables, along with cellulose, a substance which is insoluble in water, alcohol and æther, which the author



names *pectose*, and which, by the action of the weakest acids, is converted into pectin. Diluted acids produce this effect only at the temperature of ebullition; and acetic acid, which, as is well known, does not act upon starch, is also without action on pectose. Pectose cannot be confounded with cellulose, for the latter, as was ascertained by M. Payen, gives no traces of pectin when treated with acids. M. Fremy's experiments confirm those of M. Payen.

2nd. The author has found in the greater number of fruits and roots, an amorphous substance, comparable to ferments, and especially to diastase: the gelatinous substances contained in vegetables experience by its action a series of isomeric transformations. This substance M. Fremy calls *pectase*; in acting upon the gelatinous substances it gives rise to the different phenomena which constitute pectic fermentation.

3rd. The acids which are employed to convert pectose into pectin, may, according to their nature and proportion, form different substances, each of which possesses well-defined distinctive properties. Thus, when the acid is very weak, pectin, properly so called, is obtained, which does not render acetate of lead turbid. If the acid be more concentrated, or if the ebullition has been longer continued, the substance formed precipitates the neutral acetate of lead; this substance the author calls *parapectin*; and lastly, by employing a powerful acid, a third substance may be formed, which is distinguished by the name of *metapectin*; this is feebly acid to coloured test-papers, and precipitates chloride of barium; the other compounds are neutral.

4th. If a small quantity of pectase be added to a solution of pectin, and the temperature be kept at about 86° F., the pectin is soon observed to change into a gelatinous, consistent substance. This curious transformation, which explains the production of vegetable jellies, may be effected without the contact of the air; there are formed in this case two acids; one is new, and termed *pectosic acid*, and the other is pectic acid. Pectosic acid, which might be confounded with pectic acid, is immediately distinguishable from it by its perfect solubility in boiling water. In the reaction of pectase on pectin, pectosic acid is first produced, and is afterwards changed into pectic acid by the prolonged action of the pectase. The free alkalies or their carbonates are capable of converting in the cold, pectin at first into pectosates and afterwards into pectates.

The phenomena now described are so easy of observation, according to M. Fremy, and characterize pectin so distinctly, that he finds it difficult to imagine how in later times pectin has been confounded with gums, mucilages, and especially with pectic acid, which is insoluble in water.

The author has particularly examined pectic acid, and is of opinion that he has overcome the difficulties attendant upon its analysis, and especially the determination of its equivalent. He has also found that pectic acid, heated to 392° F., loses water and carbonic acid, and a new pyrogenous acid, which he calls *pyropectic acid*, is produced.

Pectic acid possesses the singular property of dissolving in considerable quantity in neutral or acid salts; it then forms compounds precipitable as jellies by alcohol; these precipitates are often mixed with pectin, render it gelatinous, and prevent by their presence the recognition, by means of elementary analysis, of the simple relations which connect pectin with the other gelatinous bodies.

5th. The gelatinous bodies may undergo a last period of transformation, and be changed into two very soluble and energetic acids. It is sufficient to boil pectic acid in water for a certain time to convert it into an acid, called by the author *parapectic acid*, and into another acid termed *metapectic acid*. The parapectic and metapectic acids are also formed during the action of acids or alkalies on pectin or pectic acid: the pectates may by long boiling be converted into metapectates. These two acids are readily distinguished from each other; for the first precipitates barytes water, and the second does not; they decompose the double tartrate of copper and potash, as glucose does. To be certain that this property was not derived from the presence of sugar, the author had recourse to a polarizing apparatus and the action of yest. Guided by the advice of M. Biot, M. Fremy found that the parapectic and metapectic acids effected no rotary action on polarized light, and that the presence of yest produced no traces of fermentation.

6th. After having examined all the properties of the gelatinous bodies, and found that by employing very weak agents, comparable to those which exist in vegetables, their acidity might be successively developed, and from neutral bodies, which they originally were, they might be transformed into energetic acids, the author examined whether, during the act of vegetation, gelatinous substances did not undergo changes comparable to those which he had produced artificially. On following for two years, with this intention, the modifications which are effected in fruits during their maturation, M. Fremy found that the gelatinous bodies which occur in them could pass through the different intermediate states which he has described; thus green fruits contain abundance of pectose. As maturation advances the pectose is changed into pectin; and when the fruits are perfectly ripe, the pectin is frequently completely converted into metapectic acid. The modifications examined in this memoir are then precisely those which occur during the maturation of fruits.

The author found in the numerous analyses which he performed that the composition of the gelatinous bodies could not be represented by carbon and water, and consequently that they were far removed from neutral bodies, properly so called. As experiment always indicates a larger quantity of hydrogen than really exists in organic bodies, the author states that he cannot attribute the difference which he has obtained to an error of analysis.

The table presented to the Academy shows that all gelatinous substances, similar to those which are derived from starch, are isomeric, or at least they differ only by the elements of water. This

result might be foreseen; for when a mixture of pectase and pectin is put into a bottle, and it is hermetically sealed, the pectin is successively converted into pectosic, pectic, parapectic, and metapectic acids, without forming any secondary product.

The capacities of saturation given in the following table prove that the acidity of the gelatinous bodies increases in proportion as their equivalent diminishes. Thus parapectin, the equivalent of which is very heavy, forms a neutral salt with lead which contains 10 per cent. of oxide, and does not redden tincture of litmus; and metapectic acid, the equivalent of which is very light, produces a salt of lead which contains 67 of oxide, and its acidity resembles that of malic or citric acid.

Names of the gelatinous substances.	Composition of the gelatinous substances.	Composition of the salts of lead.
Pectose		
Pectin . . . . .	$C^{64} H^{40} O^{56}, 8HO$	
Parapectin . . . . .	$C^{64} H^{40} O^{56}, 8HO$	$C^{64} H^{40} O^{56}, 7HO, PbO$
Metapectin . . . . .	$C^{64} H^{40} O^{56}, 8HO$	$C^{64} H^{40} O^{56}, 6HO, 2PbO$
Pectosic acid . . . .	$C^{32} H^{20} O^{28}, 3HO$	$C^{32} H^{20} O^{28}, HO, 2PbO$
Pectic acid . . . . .	$C^{32} H^{20} O^{28}, 2HO$	$C^{32} H^{20} O^{28}, 2PbO$
Parapectic acid . .	$C^{24} H^{15} O^{21}, 2HO$	$C^{24} H^{15} O^{21}, 2PbO$
Metapectic acid . .	$C^8 H^5 O^7, 2HO$	$C^8 H^5 O^7, 2PbO$

M. Fremy states that the formula of pectic acid, which he has here adopted, gives in 100 parts exactly the same quantities as determined by M. Regnault, and as published by himself in his first memoir on gelatinous bodies.

The author concludes that he has succeeded in proving that vegetables contain a neutral insoluble substance, which is convertible during vegetation into an energetic acid.—*Comptes Rendus*, Juin 14, 1847.

#### PREPARATION OF PROTOXIDE OF TIN.

M. Roth gives the following process for preparing the red protoxide of tin:—The white hydrate is to be prepared, and after being well-washed it is to be digested at  $132^{\circ}$  F. in a solution of protoacetate of tin, with a slight excess of acid, and of specific gravity about 1.06. The protoxide is then converted into hard heavy grains, which yield a greenish-brown powder; these grains inflame when heated, and readily blacken in the sunshine. They behave with reagents like common protoxide.—*Journ. de Ph. et de Ch.*, Août 1847.

#### ON THE PRESENCE OF ARSENIC, COPPER AND TIN, IN THE MINERAL WATERS OF BAVARIA.

According to the experiments of M. Buchner, Jun., the brownish-yellow ochrey deposit of the springs of Ragoczy and of Pandour, at Kissingen, contain only doubtful traces of copper; but they con-

tain sufficient quantities of arsenic to admit of the extraction of the metal.

The reddish-brown ochre of the ferruginous spring of Brückenau contains mere traces of arsenic, but there is much copper. Tin has been discovered in the ochres of Kissingen and of Brückenau. Experiments performed to ascertain the presence of arsenic and copper in the brownish-yellow ochre of the ferruginous waters of Kellberg were not followed by any positive results.—*Journ. de Ph. et de Ch.*, Août 1847.

#### SOLUBILITY OF COMMON SALT IN ALCOHOL.

M. Wagner has determined the degree of solubility of chloride of sodium in alcohol of different densities and at various temperatures. The results are that—

Alcohol of 75 per cent. dissolves at 57°20 F. 0·661 part of salt.					
....	75	....	59·45	0·700	....
....	75	....	100·40	0·736	....
....	75	....	160·70	1·033	....
....	95·5	....	59·0	0·174	....
....	95·5	....	171·05	0·171	....

*Ibid,*

#### ON SOME IMPROVED FORMS OF CHEMICAL APPARATUS.

BY THOMAS TAYLOR, ESQ.

Among the many advantages possessed by the Chemical Society, it appears to me not the least, that it affords to its members a ready mode of communicating to one another many of those little practical facts and modes of operating, which, although perhaps not of sufficient importance to merit distinct notice in the scientific journals, are nevertheless of considerable value to those engaged in the prosecution of the science. In furtherance of this view I will therefore describe some new forms of apparatus which I have myself been in the habit of using for some time past.

The first of these is a mode of closing the mouths of gas-bottles, or indeed of any wide-mouthed vessel into which tubes are to pass, as in Woolf's apparatus, gas generators, &c. To effect this the top of the bottle is first to be slightly ground, so as to procure a level surface, a piece of sheet caoutchouc is then laid upon it, and this is covered by a disc of wood of the same size as the top of the bottle, and from a quarter to half an inch in thickness. The wooden cover is held in its place by means of a small double clamp of brass or of varnished sheet iron, which passes across the cover, and the ends of which are bent under the rim of the bottle, against which they are pressed by a screw fixed in the centre of the clamp. By turning the screw the caoutchouc is sufficiently compressed to render the joint perfectly air-tight. The tubes intended to pass into and out



of the bottle are cemented into the wooden cover, usually on one side of the clamp; and they pass of course through corresponding holes in the caoutchouc. By making these holes somewhat smaller than the diameter of the tubes, the caoutchouc contracts so closely around them, that not only is any liquid which might be accidentally thrown up effectually prevented from getting between the caoutchouc and the wooden cover, but the necessity of cementing the tube into the cover may be even dispensed with. This method is so effectual and easily arranged, that I am quite convinced it will supersede the use of corks in the preparation of all gases which only require the application of a moderate heat and do not act upon caoutchouc. Ground glass plates might of course be substituted where caoutchouc is inapplicable, or a sheet of ground glass might be cemented upon the lower part of the wooden cover; but these modes would be rather expensive, and the cases in which they would be required are not very numerous. In small bottles the use of a clamp is not essential, as sufficient pressure may be obtained by inserting two wedges of wood beneath a string tied around the neck, and over the top of the bottle.

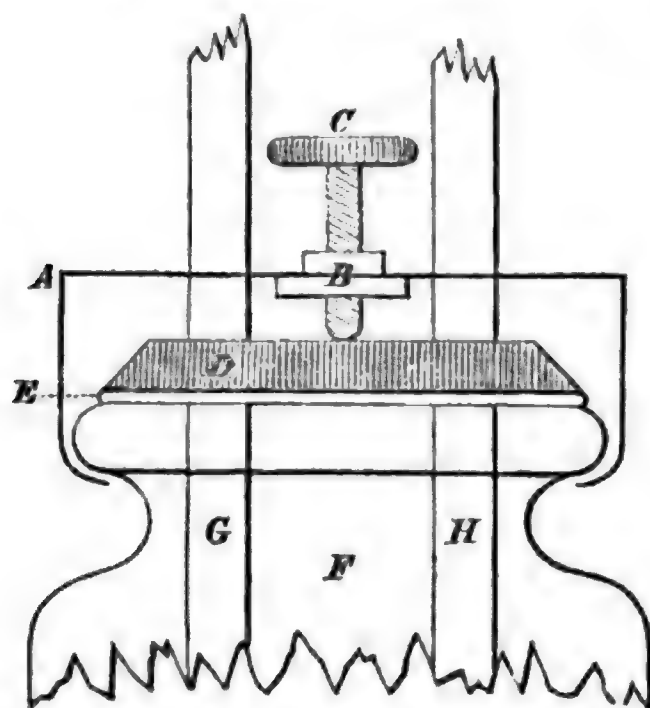


Fig. 1.

FIG. 1. A clamp of sheet iron having a small centre of brass B, in which the screw C works. D disc of wood. E sheet of caoutchouc. F glass bottle. G H glass tubes.

I will next direct the attention of the Members to a new mode of cupelling, or rather to a new form of muffle. Cupellation is an operation not often performed by amateurs, chiefly I believe on account of the difficulty in doing it unless provided with furnaces built expressly for the purpose. The following plan I have found to afford most accurate results, while it may be performed in almost any furnace:—The mouths of two black lead crucibles of the same size are to be ground flat, so that when applied one to the other they may stand quite steady. An oblong or semicircular notch is to be cut out of the mouth of one of the crucibles, and a hole is also to be

drilled through its bottom. This crucible when placed upon the top of the other constitutes the muffle, and of course resembles in shape a skittle. To cupel with this apparatus, the lower crucible is nearly filled with clean sand, set upon the bars of the grate in the centre of the furnace, and brought to a low red heat. The cupel containing the lead and the alloy is then placed upon the sand and immediately covered by the other crucible, taking care that the notch in its side shall be opposite to, and correspond with the furnace-door; more fuel is added, during which it is well to cover the hole in the top of the muffle with a crucible lid, in order to prevent the admission of dirt. When the muffle has become throughout of a bright red heat, the furnace-door is thrown open, and the ignited fuel gently moved aside, so as to permit a view of the side opening in the muffle. The current of air which is thus established through the muffle instantly causes rapid oxidation of the lead, and this may be regulated at pleasure by closing the door more or less. If from the fuel falling down any difficulty should be experienced in maintaining a free passage for the air, a portion of a porcelain tube or a gun-barrel may be passed through the furnace-door to within an inch of the muffle; but this proceeding is generally rendered quite unnecessary by taking care to place some large pieces of coke immediately around the door of the furnace.

In many cases it will be found advantageous to convert the lower crucible itself into the cupel by first half-filling it with sand and then ramming in pounded bone-earth. I have found the above method to possess the following advantages:—In the first place, the crucibles may be maintained at a much higher temperature than can be readily obtained when the ordinary muffle is used, while the degree of heat and the quantity of air admitted may be regulated with the greatest nicety. Secondly, owing to the greater draught of air, the oxidation of the lead is more quickly effected; and lastly, by looking through an opening in the furnace cover, the operation may be watched from first to last.

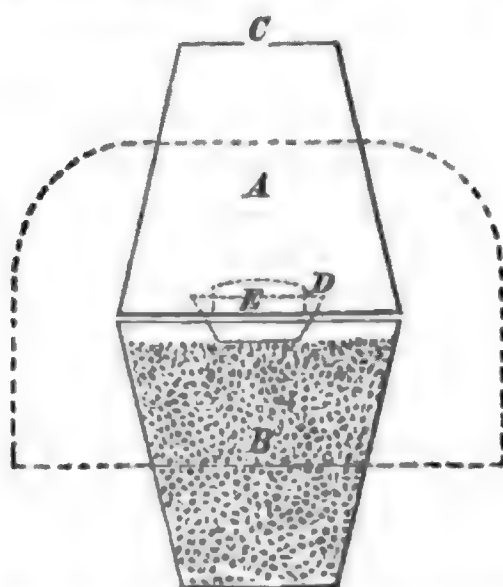


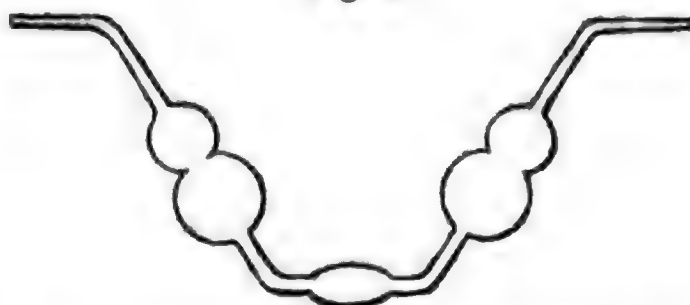
Fig. 2.

FIG. 2. A B black lead crucibles. C the upper opening. D the lower opening. E the cupel. The dotted semicircle represents the position of the furnace-door.

*Improved Form of Messrs. Will's and Varrentrapp's Apparatus.*

The only inconvenience I have found in the process proposed by Drs. Will and Varrentrapp for the estimation of nitrogen in organic bodies, is the liability of the liquid in the condenser being thrown back into the combustion-tube by sudden absorption taking place, or from too violent an evolution of the gases, part of it being ejected from the other extremity of the condenser. So well-aware were its authors of this inconvenience, that they recommend in the analyses of substances rich in nitrogen the introduction of sugar, or some other body abounding in carbon, into the combustion-tube. I have found that the necessity of this addition, which is of course open to many objections, may be entirely avoided by using a condenser nearly three times as large as that generally employed, and by surmounting each of the bulbs with another bulb of about half its capacity. The opening between the bulbs should be very wide, they being run into one another in the same manner as in the lower bulbs of Liebig's potash apparatus. With a condenser of this description, the large bulbs being  $1\frac{3}{4}$  inch in diameter and about 4 inches apart, I have never experienced the least accident, nor am I compelled to pay that constant attention to the progress of the combustion which Drs. Will's and Varrentrapp's condenser usually requires.

Fig. 3.



Mr. Taylor also exhibited a small instrument for holding Daguerreotype plates during the process of washing off. It consisted of two pieces of brass or plaited wire fitted into a wooden handle. One of the wires is bent into the form of an acute triangle, its base being slightly turned up, so as to form a ledge for the silver plate to rest upon. The other wire is placed between the sides of the triangle curved, so as to form a spring, which rests upon the top of the plate, and keeps it in its place. By inserting the fore-finger in the loop of the spring, the plate may be shaken violently without becoming dislodged.

Fig. 4.



*From the Proceedings of the Chemical Society.*

PREPARATION AND COMPOSITION OF LIGNIN.

MM. Poumarède and Figuer state as a test of the purity of lignin, that when immersed in concentrated sulphuric acid it is not rendered black. In order to procure it in this state, a piece of wood is to be transversely rasped, and the raspings are to be immersed in soap ley for twenty-four hours; the mixture is then to be diluted with once or twice its weight of water and poured off; the insoluble residue is to be largely washed with water, treated with a slight excess of dilute hydrochloric acid, and again washed with water. After this the ligneous fibre is to be treated with great excess of a solution of common salt; the digestion is to be continued with occasional stirring for two or three days, a fresh portion of the solution being once used; this being poured off, the fibrous matter is to be treated with a weak alkaline solution till it comes away colourless; it is to be again washed, and the remaining alkali is to be saturated by slight excess of hydrochloric acid, and after again washing with distilled water till litmus is not reddened, the product, placed on a sieve, is to be dried either in the sun or a stove.

The lignin thus obtained, after being washed with alcohol and æther, is not coloured by concentrated sulphuric acid, and is to be considered as absolutely pure. It is white and silky, and possesses the organic structure of the wood from which it has been obtained; and the authors consider themselves authorized to conclude that in analysing this substance, they operate on the vegetable skeleton such as it exists in plants.

The authors find that the results of their analyses differ but very little from those obtained by M. Payen; they nevertheless deem it necessary to state them as satisfactorily proving the agreement which exists between the various kinds of lignin of very different origin.

Lignin of the poplar, dried at 288° F.; mean of three experiments:—

Carbon .....	43·88
Hydrogen .....	6·23
Oxygen .....	49·89
	<hr/>
	100·00

Lignin of the beech, dried at 288° F.:—

Carbon.....	43·85
Hydrogen .....	6·22
Oxygen .....	49·93
	<hr/>
	100·00

Blotting-paper treated with acids, alkalies, water, and alcohol, dried at 288° F.:—

	I.	II.
Carbon .....	43·87	43·84
Hydrogen.....	6·12	6·22
Oxygen .....	50·01	49·94
	<hr/>	<hr/>
	100·00	100·00



Cotton treated only with boiling water, hydrochloric acid, and dilute solution of potash, cold :—

	I.	II.
Carbon.....	43.46	43.10
Hydrogen ....	6.38	6.43
Oxygen .....	50.16	50.45
	<u>100.00</u>	<u>99.98</u>

Flax, treated like cotton :—

	I.	II.
Carbon.....	43.92	43.33
Hydrogen.....	6.01	6.41
Oxygen .....	50.07	50.26
	<u>100.00</u>	<u>100.00</u>

*Papyrin*.—In employing sulphuric acid to determine the purity of lignin, the authors have discovered a new substance which constitutes a very curious modification of ligneous tissues. It results from the first action of sulphuric acid on lignin, and is the product which arises before its conversion into dextrin.

Let blotting-paper be immersed for not more than half a minute in concentrated sulphuric acid, and then be immediately washed with a large quantity of water to prevent the action of the acid ; and if it be then immersed for a few moments into water containing a few drops of ammonia, a substance is obtained which possesses all the physical characters of an animal membrane. When moistened with water, it has the soft and greasy feel of animal membrane softened in water ; when dried it has the appearance and the toughness of parchment, and when glazed it has considerable transparency.

This substance, which the authors call *papyrin*, is identical in composition with lignin. It was found to yield—

	I.	II.	III.
Carbon.....	43.30	43.89	44.44
Hydrogen....	6.28	6.27	6.23
Oxygen ....	50.42	49.84	49.33
	<u>100.00</u>	<u>100.00</u>	<u>100.00</u>

*Journ. de Ph. et de Ch.*, Août 1847.

#### SOLUBILITY OF CHLORIDE OF SILVER IN HYDROCHLORIC ACID.

M. Pierre states that concentrated hydrochloric acid is capable of dissolving  $\frac{1}{360}$  dth of its weight of chloride of silver ; when it has been diluted with twice its weight of water, it is capable of retaining more than  $\frac{1}{600}$  dth of its weight.

M. Gerhardt observes that this fact is important, and says he had previously stated it ; and it appears to him to be the cause of the difference of the numbers obtained by MM. Berzelius and Margnac as to the theoretical number expressing the atomic weight of chlorine according to Dr. Prout's law of multiples.—*Ibid.* Sept. 1847.

*Daubeny on Active and Extinct Volcanos.*

Professor Daubeny of Oxford has in the press, and nearly ready for publication, a new and much-enlarged edition of his Description of Active and Extinct Volcanos.

The present Edition will be found to contain nearly twice the amount of matter included in the preceding one, embracing not only such new facts and observations with respect to volcanos as have been brought to light since its first appearance in 1826, but likewise the allied phænomena of Earthquakes and Thermal Springs, as well as a fuller discussion of the theories connected with those subjects.

METEOROLOGICAL OBSERVATIONS FOR SEPT. 1847.

*Chiswick.*—September 1. Clear : cloudy : clear. 2. Cloudy : boisterous. 3. Cold rain : overcast. 4. Fine. 5. Clear : shower : clear. 6. Very fine. 7. Clear and cold : cloudy : rain at night. 8. Rain. 9. Very fine. 10. Overcast : very fine. 11, 12. Very fine. 13. Densely overcast : rain. 14. Very fine : slight shower : clear and cold at night. 15. Fine : boisterous, with rain at night. 16. Boisterous. 17. Rain. 18. Cloudy, with very clear intervals. 19. Cloudy : heavy rain at night. 20. Fine : slight showers. 21. Rain. 22. Cloudy : fine. 23. Cloudy and mild. 24. Foggy : very fine. 25, 26. Fine. 27. Frosty : clear : very fine : clear and frosty at night. 28. Slight fog : overcast. 29. Slight fog : very fine. 30. Dry haze : overcast.

Mean temperature of the month .....	53°·40
Mean temperature of Sept. 1846 .....	60·79
Mean temperature of Sept. for the last twenty years .....	52·77
Average amount of rain in Sept. ....	2·73 inches.

*Boston.*—Sept. 1. Fine. 2. Windy. 3. Cloudy : rain p.m. 4. Fine. 5. Fine : rain p.m. 6, 7. Fine. 8. Cloudy. 9—11. Fine. 12. Windy. 13. Rain : rain a.m. and p.m. 14. Fine. 15. Fine : rain p.m. 16. Fine : stormy from 10 a.m. 17. Cloudy. 18—20. Fine. 21. Fine : rain p.m. 22. Cloudy : rain a.m. 23. Cloudy. 24—28. Fine. 29. Cloudy. 30. Fine.

*Sandwick Manse, Orkney.*—Sept. 1, 2. Showers. 3. Bright : showers : sleet. 4—6. Showers. 7, 8. Cloudy : showers. 9. Drizzle : showers. 10. Cloudy. 11. Cloudy : rain. 12. Showers. 13. Cloudy : clear. 14. Cloudy. 15, 16. Bright : rain. 17. Cloudy : showers. 18. Showers. 19. Clear : showers : sleet. 20. Showers : rain : cloudy. 21. Bright : fine. 22. Damp : rain. 23. Showers. 24. Showers : cloudy. 25. Rain : clear. 26. Bright : clear. 27, 28. Clear. 29. Clear : aurora. 30. Clear.

*Applegarth Manse, Dumfries-shire.*—Sept. 1. Sharp showers and high wind. 2. Clear and fine harvest day. 3. Rain. 4. Fine clear sharp weather. 5. Fine harvest day. 6. Clear and bracing. 7. Rain, though not heavy. 8. Fair, but cloudy. 9. Close rain. 10. Fine : some drops p.m. 11. Fair a.m. : rain p.m. 12. Fair, but threatening. 13. Fine. 14. Bracing day : flying showers. 15. Fine a.m. : heavy rain p.m. 16. Rain and high wind. 17. Few drops of rain. 18. Fair, but dull. 19. Frequent showers. 20. A few drops. 21. Rain p.m. 22, 23. Showery. 24. Fair and fine. 25. Slight drizzle. 26. Very fine day. 27. Very fine day : frost a.m. 28, 29. Very fine days : no frost. 30. Fair, but cold.

Mean temperature of the month .....	50°·9
Mean temperature of Sept. 1846 .....	59·6
Mean temperature of Sept. for 25 years .....	53·2
Mean rain in Sept. for 20 years .....	3·13 inches.

Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at CHISWICK, near London; by Mr. Veall, at BOSTON; by the Rev. W. Dunbar, at Applegarth Manse, DUMFRIES-SHIRE; and by the Rev. C. Clouston, at Sandwick Manse, ORKNEY.

Days of Month.	Barometer.						Thermometer.						Wind.				Rain.										
	Chiswick.			Dumfries-shire.			Orkney, Sandwick.			Boston.			Dumfries-shire.			Orkney, Sandwick.			Boston.			Dumfries-shire.			Orkney, Sandwick.		
	84 a.m.		Min.	9 a.m.		9 p.m.	84 a.m.		84 p.m.	84 a.m.		84 p.m.	84 a.m.		84 p.m.	84 a.m.		84 p.m.	84 a.m.		84 p.m.	84 a.m.		84 p.m.	84 a.m.		84 p.m.
	Max.	Min.		Max.	Min.		Max.	Min.		Max.	Min.		Max.	Min.		Max.	Min.		Max.	Min.		Max.	Min.		Max.	Min.	
1847. Sept.																											
1.	29.896	29.725		29.36	29.52	29.45	29.12	29.45	29.30	66	45	57	59½	52½	53	53	44½	53	sw.	w.	01	40					
2.	29.967	29.768		29.30	29.71	29.88	29.76	29.88	29.86	65	39	59	60½	50	49	44½			w.	nw.		22					
3.	29.967	29.698		29.50	29.70	29.69	29.77	29.69	29.77	60	41	51.5	54	42	47	43½			sw.	w.	03	11					
4.	29.879	29.827		29.42	29.76	29.73	29.77	29.73	29.75	63	34	52	58½	38	47	45			n.	n.	23	16					
5.	29.878	29.849		29.39	29.72	29.68	29.66	29.68	29.66	66	39	52	56	36½	45	46			calm	calm		10					
6.	29.924	29.845		29.39	29.76	29.82	29.78	29.82	29.84	66	31	51	58½	44½	48	45			nw.	nw.	01	54					
7.	29.984	29.870		29.54	29.76	29.63	29.69	29.63	29.63	67	44	51	56½	35	51½	53			sw.	sw.	10	04					
8.	29.977	29.807		29.40	29.74	29.82	29.81	29.82	29.60	66	39	57	59	51	51½	51			calm	calm	08	25					
9.	30.099	30.053		29.58	29.76	29.82	29.56	29.82	29.73	71	43	55.5	63	51	55	51			se.	se.		17					
10.	30.123	30.085		29.60	29.95	29.98	29.80	29.98	29.94	75	43	62	63	50½	51	46			sw.	sw.		05					
11.	30.122	30.010		29.67	29.98	29.55	29.75	29.55	28.80	72	47	56	55½	37½	52	52			calm	calm		24					
12.	29.917	29.884		29.35	29.51	29.51	28.80	29.51	29.30	69	58	65	60½	54	52	49			w.	w.		32					
13.	29.788	29.762		29.32	29.65	29.67	29.60	29.67	29.71	64	41	58	62	41	50½	42			calm	calm	27	08					
14.	29.911	29.863		29.37	29.68	29.69	29.71	29.69	29.71	64	39	53	57½	43½	49	43			sw.	sw.	01	16					
15.	29.889	29.708		29.40	29.68	29.14	29.67	29.14	29.04	65	51	52	57½	38	52	45½			w.	sw.	16	79					
16.	29.472	29.391		28.90	28.87	29.08	28.88	29.08	29.00	64	46	58.5	59	49	51½	50			sw.	sw.	01	30					
17.	29.553	29.389		29.02	29.22	29.15	29.19	29.15	29.16	61	42	57	56	48½	49½	45			w.	sw.	22	24					
18.	29.707	29.455		29.00	29.26	29.53	29.21	29.53	29.47	58	36	52	55	38	48	45			w.	nw.	03	08					
19.	29.957	29.757		29.48	29.64	29.51	29.55	29.51	29.37	60	48	52	51½	40	46	44½			w.	nw.	50	60					
20.	29.934	29.729		29.23	29.46	29.78	29.33	29.78	29.64	64	35	54	52	37	46	45			w.	sw.	03	21					
21.	30.108	30.011		29.65	29.88	29.75	29.84	29.75	29.71	62	53	49	55	36	48	47			w.	sw.	19	03					
22.	30.058	30.023		29.50	29.77	29.78	29.72	29.78	29.64	70	47	51	60½	48	53	54½			calm	calm		13					
23.	30.005	29.935		29.46	29.57	29.80	29.27	29.80	29.53	69	47	57	60	53½	54½	50			w.	sw.	53	12					
24.	30.199	30.141		29.65	30.00	30.02	29.90	30.02	29.94	65	34	55	60½	49	48	50			sw.	sw.		70					
25.	30.088	29.954		29.53	29.69	29.90	29.65	29.90	30.01	65	51	50	60	51	52	45			sw.	sw.		01					
26.	30.237	30.130		29.67	30.10	30.15	30.18	30.15	30.26	62	30	56.5	58½	45½	47	43			w.	w.							
27.	30.276	30.260		29.83	30.18	30.18	30.24	30.18	30.23	60	28	46	58½	33	47	46			n.	n.							
28.	30.323	30.297		29.92	30.22	30.22	30.30	30.22	30.33	59	33	49	62	48	53½	50			ne.	ne.							
29.	30.310	30.235		29.84	30.27	30.26	30.33	30.26	30.34	63	49	54	59	39	50	48½			e.	e.							
30.	30.241	30.117		29.61	30.23	30.23	30.43	30.23	30.43	63	47	56	57	43	51	47			sw.	sw.							

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[THIRD SERIES.]

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DECEMBER 1847.

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LXIV. *On the Diamagnetic conditions of Flame and Gases.*  
By MICHAEL FARADAY, F.R.S., Foreign Associate of the  
Academy of Sciences, &c.

*To Richard Taylor, Esq.*

Royal Institution,  
Nov. 8, 1847.

MY DEAR SIR,

I LATELY received a paper from Professor Zantedeschi, published by him, and containing an account of the discovery, by P. Bancalari, of the magnetism (diamagnetism) of flame, and of the further experiments of Zantedeschi, by which he confirms the result, and shows that flame is repelled from the axial line joining two magnetic poles. I send you the paper that you may, if you estimate its importance as highly as I do, reprint it in the Philosophical Magazine; and I send also with it these further experimental confirmations and extensions of my own. As M. Zantedeschi has published his results, I have felt myself at liberty to work on the subject, which of course interested me very closely. Probably what I may describe will only come in confirmation of that which has been done already in Italy or elsewhere; and if so, I hope to stand excused; for a second witness to an important fact is by no means superfluous, and may in the present case help to induce others to enter actively into the new line of investigation presented by diamagnetic bodies generally.

I soon verified the chief result of the diamagnetic affection of flame, and scarcely know how I could have failed to observe the effect years ago. As I suppose I have obtained much more striking evidence than that referred to in Zantedeschi's paper, I will describe the shape and arrangement of the essential parts of my apparatus. The electro-magnet used was the powerful one described in the Experimental Researches (2247\*). The two terminal pieces of iron forming the virtual magnetic poles were each 1·7 inch square and six inches long;

\* Page 398 of this Journal for May 1846.



but the ends were shaped to a form approaching that of a cone, of which the sides have an angle of about  $100^\circ$ , and the axis of which is horizontal and in the upper surface of the pieces of iron. The apex of each end was rounded; nearly a tenth of an inch of the cone being in this way removed. When these terminations are brought near to each other, they give a powerful effect in the magnetic field, and the axial line of magnetic force is of course horizontal, and on a level nearly with the upper surface of the bars. I have found this form exceedingly advantageous in a great variety of experiments.

When the flame of a wax taper was held near the axial line, but on one side or the other, about one-third of the flame rising above the level of the upper surface of the poles, as soon as the magnetic force was on, the flame was affected; and receded from the axial line, moving equatorially, until it took an inclined position, as if a gentle wind was causing its deflection from the upright position; an effect which ceased the instant the magnetism was removed.

The effect was not instantaneous, but rose gradually to a maximum. It ceased very quickly when the magnetism was removed. The progressive increase is due to the gradual production of currents in the air about the magnetic field, which tend to be, and are, formed on the assumption of the magnetic conditions, in the presence of the flame.

When the flame was placed so as to rise truly across the magnetic axis, the effect of the magnetism was to compress the flame between the points of the poles, making it recede in the direction of the axial line from the poles towards the middle transverse plane, and also to shorten the top of the flame. At the same time the top and sides of the compressed part burnt more vividly, because of two streams of air which set in from the poles on each side directly against the flame, and then passed out with it in the equatorial direction. But there was at the same time a repulsion or recession of the parts of the flame from the axial line; for those portions which were below did not ascend so quickly as before, and in ascending they also passed off in an inclined and equatorial direction.

On raising the flame a little more, the effect of the magnetic force was to increase the intensity of the results just described, and the flame actually became of a fish-tail shape, disposed across the magnetic axis.

If the flame was raised until about two-thirds of it were above the level of the axial line, and the poles approached so near to each other (about 0.3 of an inch) that they began to cool and compress the part of the flame at the axial line, yet without interfering with its rising freely between them; then,

on rendering the magnet active, the flame became more and more compressed and shortened; and as the effects proceeded to a maximum, the top at last descended, and the flame no more rose between the magnetic poles, but spread out right and left on each side of the axial line, producing a double flame with two long tongues. This flame was very bright along the upper extended forked edge, being there invigorated by a current of air which *descended* from between the poles on to the flame at this part, and in fact drove it away in the equatorial direction.

When the magnet was thrown out of action, the flame resumed its ordinary upright form between the poles, at once; being depressed and redivided again by the renewal of the magnetic action.

When a small flame, only about one-third of an inch high, was placed between the poles, the magnetic force instantly flattened it into an equatorial disc.

If a ball of cotton about the size of a nut be bound up by wire, soaked in æther and inflamed, it will give a flame six or seven inches high. This large flame rises freely and naturally between the poles; but as soon as the magnet is rendered active, it divides and passes off in two flames, the one on one side, and the other on the other side of the axial line.

Such therefore is the general and very striking effect which may be produced on a flame by magnetic action, the important discovery of which we owe to P. Bancalari.

I verified the results obtained by M. Zantedeschi with different flames, and found that those produced by alcohol, æther, coal-gas, hydrogen, sulphur, phosphorus, and camphor were all affected in the same manner, though not apparently with equal strength. The brightest flames appeared to be most affected.

The chief results may be shown in a manner in some respects still more striking and instructive than those obtained with flame, by using a smoking taper. A taper made of wax, coloured green by verdigris, if suffered to burn upright for a minute and then blown out, will usually leave a wick with a spark of fire on the top. The subdued combustion will however still go on, even for an hour or more, sending up a thin dense stream of smoke, which, in a quiet atmosphere, will rise vertically for six or eight inches; and in a moving atmosphere will show every change of its motion, both as to direction and intensity. When the taper is held beneath the poles, so that the stream of smoke passes a little on one side of the axial line, the stream is scarcely affected by the power of the magnet, the taper being three or four inches below the poles; but if the taper be raised, so that the coal is not more than an inch

below the axial line, the stream of smoke is much more affected, being bent outwards; and if it be brought still higher, there is a point at which the smoke leaves the taper-wick even in a horizontal direction, to go equatorially. If the taper be held so that the smoke-stream passes *through* the axial line, and then the distances be varied as before, there is little or no sensible effect when the wick is four inches below: but being raised, as soon as the warm part of the stream is between the poles, it tends to divide; and when the ignited wick is about an inch below the axial line, the smoke rises vertically in one column until about two-thirds of that distance is passed over, and then it divides, going right and left, leaving the space between the poles clear. As the taper is slowly raised, the division of the smoke descends, taking place lower down, until it occurs upon the wick, at the distance of 0·4 or 0·5 of an inch below the axial line. If the taper be raised still more, the magnetic effect is so great, as not only to divide the stream, but to make it descend on each side of the ignited wick, producing a form resembling that of the letter W; and at the same time the top of the burning wick is greatly brightened by the stream of air that is impelled downwards upon it. In these experiments the magnetic poles should be about 0·25 of an inch apart.

A burning piece of amadou, or the end of a splinter of wood, produced the same effect.

By means of a small spark and stream of smoke, I have even rendered the power of an ordinary magnet, in affecting them, evident. The magnet was a good one, and the poles were close to each other and conical in form.

Before leaving this description of the general phænomenon and proceeding to a consideration of the principles of magnetic action concerned in it, I may say that a single pole of the magnet produces similar effects upon flame and smoke, but that they are much less striking and observable.

Though the effect be so manifest in a flame, it is not, at first sight, evident what is the chief cause or causes of the result. The *heat* of the flame is the most apparent and probable condition; but there are other circumstances which may be equally or more influential. Chemical action is going on at the time:—solid matter, which is known to be diamagnetic, exists in several of the flames used: and a great difference exists between the matter of the flame and the surrounding air. Now any or all of these circumstances of temperature, chemical action, solidity of part of the matter, and differential composition in respect to the surrounding air, may concur in producing or influencing the result.

I placed the wires of an electrometer, and also of a galvanometer, in various parts of the affected flame, but could not procure any indications of the evolution of electricity by any action on the instruments.

I examined the neighbourhood of the axial line as to the existence of any current in the air when there was no flame or heat there, using the visible fumes produced when little pellets of paper dipped in strong solutions of ammonia and muriatic acid were held near each other; and though I found that a stream of such smoke was feebly affected by the magnetic power, yet I was satisfied there was no current or motion in the common air, as such, between the poles. The smoke itself was feebly diamagnetic; due, I believe, to the solid particles in it.

But when flame or a glowing taper is used, strong currents are, under favourable circumstances, produced in the air. If the flame be between the poles, these currents take their course along the surface of the poles, which they leave at the opposite faces connected by the axial line, and passing parallel to the axial line, impinge on the opposite sides of the flame; and feeding the flame, they make part of it, and proceed out equatorially. If the flame be driven asunder by the force of these currents and retreat, the currents follow it; and so, when the flame is forked, the air which is between the poles forms a current which sets from the poles downwards and sideways towards the flame. I do not mean that the air in *every* case travels along the surface of the poles or along the axial lines, or even from between the poles; for in the case of the glowing taper, held half an inch or so beneath the axial line, it is the cool air which is next nearest to the taper, and (generally) between the taper and the axial line, that falls with most force upon it. In fact the movements of the parts of the air and flame are due to a differential action. We shall see presently that the air is diamagnetic as well as flame or hot smoke; *i. e.* that both tend, according to the general law which I have expressed in the Experimental Researches (2267, &c.), to move from stronger to weaker places of magnetic force, but that hot air and flame are more so than cold or cooler air: so, when flame and air, or air at different temperatures, exist at the same time within a space under the influence of magnetic forces, differing in intensity of action, the hotter particles will tend to pass from stronger to weaker places of action, to be replaced by the colder particles; the former therefore will have the effect of being repelled; and the currents that are set up are produced by this action, combined with the mechanical force or current possessed by the flame in its ordinary relation to the atmosphere.



It will be evident to you that I have considered flame only as a particular case of a general law. It is a most important and beautiful one, and it has given us the discovery of diamagnetism in gaseous bodies: but it is a complicated one, as I shall now proceed to show, by analysing some of its conditions and separating their effects.

For the purpose of examining the effect of heat alone in conducing to the diamagnetic condition of flame, a small helix of fine platina wire was attached to two stronger wires of copper, so that the helix could be placed in any given position as regarded the magnetic poles, and at the same time be ignited at pleasure by a voltaic battery. In this manner it was substituted for the burning taper, and gave a beautiful highly-heated current of air, unchanged in its chemical condition. When the helix was placed directly under the axial line, the hot air rose up between the poles freely, being rendered evident above by a thermometer, or by burning the finger, or even scorching paper; but as soon as the magnet was rendered active, the hot air divided into a double stream, and was found ascending on the two sides of the axial line; but a descending current was formed between the poles, flowing downwards towards the helix and the hot air, which rose and passed off sideways from it.

It is therefore perfectly manifest that hot air is diamagnetic in relation to, or more diamagnetic than, cold air; and, from this fact I concluded, that, by cooling the air below the natural temperature, I should cause it to approach the magnetic axis, or appear to be magnetic in relation to ordinary air. I had a little apparatus made, in which a vertical tube delivering air was passed through a vessel containing a frigorific mixture; the latter being so clothed with flannel that the external air should not be cooled, and so invade the whole of the magnetic field. The central current of cold air was directed downwards a little on one side of the axial line, and falling into a tube containing a delicate air-thermometer, there showed its effect. On rendering the magnet active, this effect however ceased, and the thermometer rose; but on bringing the latter under the axial line it again fell, showing that the cold current of air had been drawn inwards or attracted towards the axial line, *i. e.* had been rendered magnetic in relation to air at common temperatures, or less diamagnetic than it. The lower temperature was  $0^{\circ}$  F. The effect was but small; still it was distinct.

The effect of heat upon air, in so greatly increasing its diamagnetic condition, is very remarkable. It is not, I think, at all probable that the mere effect of expanding the air is the cause of the change in its condition, because one would be led

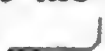
to expect that a certain bulk of expanded air would be less sensible in its diamagnetic effects than an equal bulk of denser air; just as one would anticipate that a vacuum would present no magnetic or diamagnetic effects whatever, but be at the zero point between the two classes of bodies (*Experimental Researches*, 2423, 2424). It is certainly true, that if the air were a body belonging to the magnetic class, then its expansion, being equivalent to dilution, would make it seem diamagnetic in relation to ordinary air (*Experimental Researches*, 2367, 2438); but that, I think, is not likely to be the case, as will be seen by the results described further on in reference to oxygen and nitrogen.

If the power conferred by heat is a direct consequence, and proportionate to the temperature, then it gives a very remarkable character to gases and vapours, which, as we shall see hereafter, possess it in common. In my former experiments (*Experimental Researches*, 2359, 2397) I heated various diamagnetic bodies, but could not perceive that their degree of magnetic force was at all increased or affected by the temperature given to them. I have again submitted small cylinders of copper and silver to the action of a single pole, at common temperatures and at a red heat, with the same result. If there was any effect of increased temperature, it was that of a very slight increase in the diamagnetic force, but I am not sure of the result. At present, therefore, the gaseous and vaporous bodies seem to be strikingly distinguished by the powerful effect which heat has in increasing their diamagnetic condition.

As all the experiments, whether on flame, smoke, or air, seemed to show that air had a distinct magnetic relation, which, though highly affected by temperature, still belonged to it at all temperatures; so it was a probable conclusion that other gaseous or vaporous bodies would be diamagnetic or magnetic, and that they would differ from each other even at common or equal temperatures. I proceeded therefore to examine them, delivering streams of each into the air, in the first instance, by fit apparatus and arrangements, and examining the course taken by these streams in passing across the magnetic field, the magnetic force being either induced or not at the time.

In delivering the various streams, I sometimes introduced the gases into a globe with a mouth and also a tubular spout, and then poured the gas out of the spout, upwards or downwards, according as it was lighter or heavier than air. At other times, as with muriatic acid or ammonia, I delivered the streams from the mouth of the retort. But as it is very im-

portant not to deluge the magnetic field with a quantity of invisible gas, I devised the following arrangement, which answered well for all the gases not soluble in water. A Woulf's bottle was chosen having three apertures at the top, *a*, *b* and *c*; a wide tube was fixed into aperture *a*, descending within the bottle to the bottom, and being open above and below; by this any water could be poured into the bottle and employed to displace the gas previously within it. Aperture *b* was closed by a stopper. Aperture *c* had an external tube, with a stop-cock fixed in it to conduct the gas to any place desired. To expel the gas and send it forward, a cistern of water was placed above the bottle, and its cock so plugged by a splinter of wood, that when full open it delivered only twelve cubic inches of fluid in a minute. This stream of water being directed into aperture *a*, and the cock of tube *c* open, twelve cubic inches of any gas within the Woulf's bottle was delivered in a minute of time; and this I found an excellent proportion for our magnet and apparatus.

With respect to the delivery of this gas at the magnetic poles, a piece of glass tube bent into this shape  was held by a clamp on the stage of the magnet, so that it could easily be slipped backward and forward, or to one side, and so its vertical part be placed anywhere below the axial line. The aperture at this end was about the one-eighth of an inch internal diameter. In the horizontal part near the angle was placed a piece of bibulous paper, moistened with strong solution of muriatic acid (when necessary). The horizontal part of the tube was connected and disconnected in a moment, when necessary, with the tube *c* of the gas-bottle, by a short piece of vulcanized rubber tube. If the gas to be employed as a stream were heavier than the surrounding medium, then the glass tube, instead of having the form delineated above, was so bent as to deliver its stream downwards and over the axial line. In this manner currents of different gases could be delivered, perfectly steady and under perfect command.

The next point was to detect and trace the course of these streams. A little ammonia vapour, delivered near the magnetic field, did this in some degree, but was not satisfactory; for, in the first place, the little cloud of muriate of ammonia particles formed, is itself diamagnetic; and further, the tranquil condition of the air in the magnetic field was then too much disturbed. Catch-tubes were therefore arranged, consisting of tubes of thin glass about the size and length of a finger, open at both ends, and fixed upon little stands so that they could be adjusted either over or under the magnetic poles at pleasure. When they were over the poles, I generally had

three at once; one over the axial line and one at each side. When they were under the poles, the lower end was turned up a little for the purpose of facilitating observation there.

The gas delivered at the poles, as already described, contained a little muriatic acid (obtained from the solution in the paper), but not enough to render it visible. To make it manifest up which catch-tube it passed, a little piece of bibulous paper, folded and bound round and suspended by a copper wire, was dipped in the solution of ammonia and hung in each of the tubes. It was then evident at once, by the visible fume formed at the top of one of the tubes, whether the gas delivered below passed up the one or the other tube, and which: and yet the gas was perfectly clear and transparent as it passed by the place of magnetic action.

In addition to these arrangements, I built up a sheltering chamber about the magnetic poles and field, to preserve the air undisturbed. This was about six inches long by four inches in width and height, and was easily made of thin plates of mica, which were put together or taken down in a moment. The chamber was frequently left more or less open at the top or bottom for the escape of gases, or the place of the catch-tubes. Its advantages were very great.

*Air.*—In the first place air was sent in under these arrangements, the stream being directed by the axial line. It made itself visible in the catch-tube above by the smoke produced; but whether the magnet was active or not, its course was the same; showing that, so far, the apparatus worked well, and did not of itself cause any erroneous indications.

*Nitrogen.*—This gas was sent from below upwards, and passed directly by the axial line into the catch-tube above; but when the magnet was made active, the stream was affected, and though not stopped in the middle catch-tube, part appeared in the side tubes. The jet was then arranged a little on one side of the axial line, so that, without the magnetic action, it still ascended and went up the middle catch-tube: then, when the magnetic action was brought on, it was clearly affected, and a great portion of it was sent to the side catch-tube. The nitrogen was, in fact, manifestly diamagnetic in relation to common air, when both were at the same temperature; but as four-fifths of the atmosphere consists of nitrogen, it seemed very evident, from the result, that nitrogen and oxygen must be very different from each other in their magnetic relations.

*Oxygen.*—A stream of oxygen was sent down through air between the poles. When there was no magnetic action it descended vertically, and when the magnetic action was on it



appeared to do the same; at all events it did not pass off equatorially. But as there was reason, from the above experiments with nitrogen, to expect that oxygen would appear, not diamagnetic but magnetic in air; so the place of the stream was changed and made to be on one side of the axial line. In this case it fell perfectly well at first into a catch-tube placed beneath; but as soon as the magnet was rendered active, the stream was deflected, being drawn towards the axial line, and fell into another catch-tube placed there to receive it. So oxygen appears to be magnetic in common air. Whether it be really so, or only less diamagnetic than air (a mixture of oxygen and nitrogen), we shall be better able to consider hereafter.

*Hydrogen.*—This gas proved to be clearly and even strongly diamagnetic; for notwithstanding the powerful ascensive force which its stream has in the atmosphere, because of its small specific gravity, still it was well deflected and sent equatorially. Considering the lightness of the gas, one might have expected that it would have been drawn towards the axial line, as a stream of rarefied air (if it could exist) would be. Its diamagnetic state, therefore, shows in a striking point of view, that gases, like solids, have peculiar and distinctive degrees of diamagnetic force.

*Carbonic acid.*—This gas made a beautiful experiment. The stream was delivered downwards a little on one side of the axial line; a catch-tube was placed a little further out, so that the stream should fall clear of it as long as there was no activity in the magnet. But on rendering the magnet efficient, the stream left its vertical direction, passed equatorially, and fell into the catch-tube; and by looking horizontally, could be seen flowing out at its lower extremity like a spring, and falling away through the air. Again, the magnet was thrown out of action, and a glass with lime-water placed beneath the lower end of the catch-tube; no carbonic acid appeared there, though the fluid in the glass was continually stirred; but the instant the magnet was made, the carbonic acid appeared in the catch-tube, fell into the glass and made the lime-water turbid. This gas therefore is diamagnetic in air.

*Carbonic oxide.*—This gas was carefully freed from carbonic acid before it was used. It was employed as a descending stream, and was apparently very diamagnetic: but it is to be remarked, that a substance which is so nearly the specific gravity of atmospheric air is easily dispersed right and left in it, and therefore that the facility of dispersion is not a correct indication of the diamagnetic force. By introducing a little ammonia into the mica chamber, it was, however, easily seen

that carbonic oxide was driven away equatorially with considerable power; and I judge from the appearance, that it is more diamagnetic than carbonic acid.

*Nitrous oxide.*—This gas was moderately, but clearly, diamagnetic in air. Much interest belongs to this and the other compounds of nitrogen and oxygen, both because they contain the same elements as air, and because of the relations of nitrogen and oxygen separately.

*Nitric oxide.*—I tried this gas both as an up and down current, but could not determine its magnetic condition. What with the action of the oxygen of the air, the change of the nature of the substances, and the heat produced, there was so much incidental disturbance and so little effect due to magnetic influence, that I could not be sure of the result. On the whole it was very slightly diamagnetic; but so little, that the effect might be due to the smoke particles which served to render it visible.

*Nitrous acid gas.*—Difficult to observe, but I believe it is slightly magnetic in relation to air.

*Olefiant gas* was diamagnetic, and well so. The little difference in specific gravity of this gas and air, even creates a difficulty in following the course of the olefiant gas, unless it be watched for on every side.

*Coal-gas.*—The coal-gas of London is lighter than air, being only about two-thirds in weight of the latter. It is very well diamagnetic, and gives exceedingly good and distinct results.

*Sulphurous acid gas* is diamagnetic in air. It was generated in a small tube containing liquid sulphurous acid; this being connected, in place of the gas bottle, with the delivery-tube and mouthpiece by the vulcanized rubber tube. The presence or absence of the gas in the catch-tube was well shown by ammonia, and still better by litmus paper.

*Muriatic acid.*—The retort in which it was generated was connected, as just described, with the delivery-tube. The gas was very decidedly diamagnetic in air.

*Hydriodic acid* was also diamagnetic in air. When there was an abundant stream of gas, its entrance into and passage through the side catch-tube, on rendering the magnet active, was very striking. When there was less gas, the stream was dispersed equatorially in all directions, and less entered the tube.

*Fluo-silicon.*—Diamagnetic in air.

*Ammonia.*—This gas was evolved from materials in a retort, and tested in the catch-tube above by muriatic acid in the paper. It was well diamagnetic, corresponding in this respect

with the character of its elements. It could also be very well indicated by reddened litmus paper held over the tubes.

*Chlorine* was sent from the Woulf's bottle apparatus, and proved to be decidedly diamagnetic in air. Either ammonia by its fumes, or litmus paper by its becoming bleached, served to indicate the entrance of the chlorine into the side catch-tube every time the magnet was rendered active.

*Iodine*.—A piece of glass tube was so shaped at its lower extremity as to form a chamber for the reception of iodine, which chamber had a prolonged mouth directed downwards so as to deliver the vapour formed within. On putting a little iodine into the chamber, then heating it, and especially the mouth part, by a spirit-lamp, and afterwards inclining the apparatus, abundance of the vapour of iodine was generated as the substance flowed on to the hotter parts, and passed in a good stream from the mouth downwards. This purple stream was diamagnetic in air, and could be seen flowing right and left from the axial line, when not too dense. If very dense and heavy, its gravity was such as to make it break through the axial line, notwithstanding the action of the magnet; still it was manifest that iodine is diamagnetic to air.

*Bromine*.—A little bromine was put into the horizontal part of the delivery tube, and then air passed over it by the apparatus already described. So much bromine rose into vapour as to make the air of a yellow colour, and caused it to fall well in a stream by the axial line. A little ammonia delivered near the magnetic field showed that this stream was diamagnetic, and hence it may fairly be presumed that the pure vapour of bromine would be diamagnetic also.

*Cyanogen*.—Strongly diamagnetic in air.

Taking air as the standard of comparison, it is very striking to observe, that much as gases appear to differ one from another in the degree of their diamagnetic condition, there are very few that are not more diamagnetic than it; and when the investigation is carried forward into the relation of the two chief constituents of air, oxygen and nitrogen, it is still more striking to observe the very low condition of oxygen, which, in fact, is the cause of the comparatively low condition of air. Of all the vapours and gases yet tried, oxygen seems to be that which has the least diamagnetic force. It is as yet a question where it stands; for it may be as low as a vacuum, or may even pass to the magnetic side of it, and experiment does not as yet give an answer to the question. I believe it to be diamagnetic; and this belief is strengthened by the action of heat upon it, to be described hereafter; but it is exceedingly low in the scale, and far below chlorine, iodine, and such like bodies.

All the compounds of oxygen and nitrogen seem to show the influence of the presence of the oxygen. Nitrous acid seems to be less diamagnetic than air. Nitric oxide mingled with nitrous acid and warm, is about as air. Nitrous oxide is clearly diamagnetic in air, though it contains more oxygen: but it also contains more nitrogen than air, and is also denser than it, so that there is more matter present; still I think the results are in favour of the idea that oxygen is diamagnetic. By referring to the relation of carbonic oxide to carbonic acid, described further on, it will be seen that the addition of oxygen seems to make a body less diamagnetic. But the truth may be, not that oxygen is really magnetic, but that a compound body possesses a specific diamagnetic force, which is not the sum of the forces of its particles.

It is very difficult to form more than a mere guess at the relative degree of diamagnetic force possessed by different gaseous bodies when they are examined only in air, because of the many circumstances which tend to confuse the results. First, there is the invisibility of the gas which deprives one of the power of adjusting by sight so as to obtain the best effect: then, there is the difference of gravity; for if a gas ascend or descend in a rapid stream, it may seem less deflected than another flowing more slowly, though it be more diamagnetic; and as to gases nearly of the specific gravity of air, whether more or less diamagnetic, they are almost entirely dispersed in different directions, so that little only enters the catch-tube. Another modifying circumstance is the distance of the aperture delivering gas from the axial line, which, to obtain the maximum effect, ought to vary with the gravity of the gases and their diamagnetic force. Again, it is important that the magnetic field be not filled with the gas to be examined, and that generally speaking only a moderate stream be employed; which however must depend again upon the specific gravity.

The only correct way therefore of comparing two gases together is to experiment with them one in the other. For the experiments made with gases, in gases or in air are differential, and similar in their nature with those made on a former occasion with solutions (*Experimental Researches*, 2362, &c.); I therefore changed the surrounding medium in a few experiments, substituting other gases for air; and first chose carbonic acid as a body easy to experiment with, and one that would, probably, be more powerfully than some other of the gases, diamagnetic (I speak as to the appearances or relative results only) in air.

I constructed a kind of tray or box, by folding up a doubled sheet of waxed paper; thus making a vessel thirteen inches



long, five inches wide, and five inches high. This was placed on the ends of the great magnet, and the terminal pieces of iron before described, placed in it. The box was covered over loosely by plates of mica, and formed a long square chamber in which were contained the magnetic poles and field. All the former arrangements in respect of the magnetic field, the delivery-tube, the catch-tubes, &c., were then made; and, lastly, the box was filled with carbonic acid by a tube, which entered it at one corner; and was, from time to time, supplied with a fresh portion of gas, as the previous contents became diluted with gases or air. Everything answered perfectly, and the following results were easily obtained.

*Air* passed axially, being less diamagnetic than carbonic acid gas.

*Oxygen* passed to the magnetic axis, as was to be expected.

*Nitrogen* went equatorially, being therefore diamagnetic, even in carbonic acid.

*Hydrogen, coal-gas, olefiant gas, muriatic acid and ammonia* passed equatorially in carbonic acid, and were fairly diamagnetic in relation to it.

*Carbonic oxide* was very fairly diamagnetic in carbonic acid gas. Here the effect of oxygen seems to be very well illustrated. Equal volumes of carbonic oxide and carbonic acid contain equal quantities of carbon; but the former contains only half as much oxygen as the latter. Yet it is more diamagnetic than the latter; so, that, though an additional volume and quantity of oxygen, equal to that in the carbonic oxide, is in the carbonic acid added and compressed into it, it does not add to, but actually takes from, the diamagnetic force.

*Nitrous oxide* appears to be slightly diamagnetic in relation to carbonic acid; but nitric oxide gas was in the contrary relation and passed towards the axial line.

Hence it seems that carbonic acid, though more diamagnetic than air, is not far removed from it in that respect; and this position it probably holds because of the quantity of oxygen in it. The apparent place of nitrous oxide close to it appears, in a great measure, to depend on the same circumstance of oxygen entering largely into its composition. Still it is manifest that the action is not *directly* as the oxygen, for then common air would be more diamagnetic than either of them. It seems rather that the forces are modified, as in the case also of iron and oxygen, and that each compound body has its peculiar but constant intensity of action.

In order to make similar experiments in light gases, the two terminal pieces of the magnet were raised, so that they might be covered by a French glass shade, which, with its

stand, made a very good chamber about them. The pipe to supply and change the gaseous medium, and also that for bringing the gas under trial as a stream into the magnetic field, passed through holes made in the bottom of the stand. The different gases to be compared with those employed as media, were, except in the cases of ammonia and chlorine, mingled with a trace of muriatic acid, as before described. The gaseous media used were two, coal-gas and hydrogen. Whilst using coal-gas, I observed the direction of the currents of the other gases in it by bringing a little piece of paper, at the end of a wire and dipped in ammonia solution, near the stream. In the case of the hydrogen, I diffused a little ammonia through the whole of the gas in the first instance.

*Air* passed towards the axial line in coal-gas, but was not much affected.

Oxygen had the appearance of being strongly magnetic in coal-gas, passing with great impetuosity to the magnetic axis, and clinging about it; and if much muriate of ammonia fume were purposely formed at the time, it was carried by the oxygen to the magnetic field with such force as to hide the ends of the magnetic poles. If then the magnetic action were suspended for a moment, this cloud descended by its gravity; but being quite below the poles, if the magnet were again rendered active, the oxygen cloud immediately started up and took its former place. The attraction of iron filings to a magnetic pole is not more striking than the appearance presented by the oxygen under these circumstances.

*Nitrogen*.—Clearly diamagnetic in coal-gas.

*Olefant, carbonic oxide, and carbonic acid* gases were all slightly, but more or less diamagnetic in the coal-gas.

On substituting hydrogen as the surrounding medium in place of coal-gas, more care was taken in the experiments. Each gas experimented upon was tried in it twice at least; first in the hydrogen of a previous experiment, and then in a new atmosphere of hydrogen.

*Air*.—Air passes axially in hydrogen when there is very little smoke in it: when there is much smoke in the stream the latter is either indifferent or tends to pass equatorially. I believe that air and hydrogen cannot be far from each other.

*Nitrogen* is strikingly diamagnetic in hydrogen.

*Oxygen* is as strikingly magnetic in relation to hydrogen. It presented the appearances already described as occurring in coal-gas; but as the jet delivered the descending stream of oxygen a little on one side of the axial line, its centrifugal power, in relation to the axial line, was so balanced by the centripetal power produced by the magnetic action, that the

stream at first revolved in a regular ring round the axial line, and produced a cloud that continued to spin round it as long as the magnetic force was continued, but fell down to the bottom of the chamber when that force was removed.

*Nitrous oxide.*—This gas was clearly diamagnetic in the hydrogen, and gave rise to a very beautiful result in consequence of its following the oxygen; for at the beginning of the experiment, the little oxygen contained in the conducting tube passed axially; but the instant that was expelled, and the nitrous oxide issued forth, the stream changed its direction, and passed off diamagnetically in the most striking manner.

*Nitric oxide.*—This gas passed equally in hydrogen, and therefore is magnetic in relation to it.

*Ammonia.*—Diamagnetic in hydrogen.

*Carbonic oxide, carbonic acid, and olefiant gases* were diamagnetic in hydrogen; the last most so, and the carbonic acid apparently the least.

*Chlorine* was slightly diamagnetic in hydrogen. It was clearly so; but the cloudy particles might conduce much to the small effect produced.

*Muriatic acid gas.*—I think it was a little diamagnetic in the hydrogen.

Notwithstanding the many disturbing causes which interfere with first and hasty experiments of this kind, and produce results which occasionally cross and contradict each other, still there are some very striking considerations which arise in comparing the gases with each other at the same temperature. Foremost amongst these is the place of oxygen; for of all the gaseous bodies yet tried it is the least diamagnetic, and seems in this respect to stand far apart from the rest of them. The condition of nitrogen, as being highly diamagnetic, is also important. The place of hydrogen, as being less diamagnetic than nitrogen, and of chlorine, which, instead of approaching to oxygen, is above hydrogen, and also of iodine, which is probably far above chlorine, are marked circumstances.

*Air* of course owes its place to the proportion and the individual diamagnetic character of the oxygen and nitrogen in it. The great difference existing between these two bodies in respect of magnetic relation, and the striking effect presented by oxygen in coal-gas and hydrogen, bodies not far removed from nitrogen in diamagnetic force, made me think it might not be impossible to separate air into its two chief constituents by magnetic force alone. I made an experiment for this purpose but did not succeed; but I am not convinced that it cannot be done. For since we can actually distinguish certain gases, and especially these by their magnetic properties, it does not



seem impossible that sufficient power might cause their separation from a state of mixture.

In the course of these experiments I subjected several of the gases to heat, to ascertain whether they generally underwent the same exaltation of their diamagnetic power which occurred with common air. For this purpose a helix of platina wire was placed in the mouth of the delivering tube, which itself was placed below the magnetic axis between the poles. The helix could be raised to any temperature by a little voltaic battery, and any gas could be sent through it and upwards across the magnetic field by means of the Woulf's bottle apparatus already described. It was easy to ascertain whether the gas went directly up between the poles, or, on making the magnet, left that direction and formed two equatorial side-streams, either by the sensation on the finger, or by a spiral thermoscope formed of a compound lamina of platinum and silver placed in a tube above. In every case the hot gas was diamagnetic in the air, and I think far more so than if the gas had been at common temperatures. The gases tried were as follows: oxygen, nitrogen, hydrogen, nitrous oxide, carbonic acid, muriatic acid, ammonia, coal-gas, olefiant gas.

But as in these experiments the surrounding air would, of necessity, mingle with the gas first heated, and so form, in fact, a part of the heated stream, I arranged the platinum helix so that I could heat it in a given gas, and thus compare the same gas at different temperatures with itself.

A stream of hot oxygen in cold oxygen was powerfully diamagnetic. The effect and its degree may be judged of by the following circumstances. When the platinum helix below the axial line was ignited, the effect of heat on the indicating compound spiral, placed in a tube over the axial line, was such as to cause its lower extremity to pass through one and a half revolutions, or  $540^\circ$ : when the magnetic force was rendered active, the spiral returned through all these degrees to its first position, as if the ignited helix below had been lowered to the common temperature or taken away; and, yet in respect of it, nothing had been changed. On rendering the magnet inactive, the current of hot oxygen instantly resumed its perpendicular course and affected the thermoscope as before.

On experimenting with carbonic acid, it was found that hot carbonic acid was diamagnetic to cold carbonic acid; and the effects were apparently as great in amount as in oxygen.

On making the same arrangement in hydrogen, I failed to obtain any result regarding the relation of the hot and cold gas, for this reason:—that I could not, in any case, either

*Phil. Mag. S. 3. Vol. 31. No. 210. Dec. 1847.* 2 E



with or without the magnetic action, obtain any signs of heat on the thermoscopic spiral above, even when the platinum helix, not more than an inch below it, was nearly white hot. This effect is, I think, greatly dependent upon the rapidity with which hydrogen is heated and cooled in comparison with other gases, and also upon the vicinity of the cold masses of iron forming the magnetic poles, between which the hot gas has to pass in its way upwards: and it is most probably connected with the fact observed by Mr. Grove of the difficulty of igniting a platinum wire in hydrogen.

When the igniting helix was placed in coal-gas, it was found that the hot gas was diamagnetic to that which was cold; as in all the other cases. Here, again, an effect like that which was observed in hydrogen occurred; for when there was no magnetic action, the ascending stream of hot coal-gas could cause the thermoscopic spiral to revolve through only  $280^{\circ}$  or  $300^{\circ}$ , in place of above  $540^{\circ}$ ; through which it could pass when the surrounding gas was oxygen, air, or carbonic acid; and that even when the helix was at a higher temperature in the coal-gas than in any of these gases.

The proof is clear then that oxygen, carbonic acid, and coal-gas, are more diamagnetic hot than cold. The same is the case with air; and as air consists of four-fifths nitrogen and only one-fifth oxygen, and yet shows an effect of this kind as strongly as oxygen, it is manifest that nitrogen also has the same relation when hot and cold.

Of the other gases also I have no doubt; though to be quite certain, they ought to be tried in atmospheres of their own substance, or else in gases more diamagnetic at common temperatures than they. The olefiant and coal-gases in air easily bore the elevation of the helix to a full red heat, without inflaming when out of the exit-tube: the hydrogen required that the helix should be at a lower temperature. Muriatic acid and ammonia showed the division of the one stream into two, very beautifully, on holding blue and red litmus paper above.

There is another mode of observing the diamagnetic condition of flame, and experimenting with the various gases, which is sometimes useful, and should always be understood, lest it inadvertently might lead to confusion. I have a pair of terminal magnetic poles which are pierced in a horizontal direction, that a ray of light may pass through them. The opposed faces of these vertical poles are not, as in the former case, the rounded ends of cones; but, though rounded at the edges, may be considered as flat over an extent of surface an inch in diameter. The pierced passages are in the form of cones, the truncation of which in this flat surface is rather

more than half an inch in diameter. When these poles were in their place, and from 0·3 to 0·4 of an inch apart, a taper flame, burning freely between them, was for a few moments unaffected by throwing the magnet into action; but then it suddenly changed its form, and extending itself axially, threw off two horizontal tongues, which entered the passages in the poles; and thus it continued as long as the magnetism continued, and no part of it passed equatorially.

On using a large flame made with the cotton ball and æther, two arms could be thrown off from the flame by the force of the magnetism, which passed in an equatorial direction, as before; and other two parts entered the passages in the magnetic poles, and actually issued out occasionally at their further extremities.

When the poles were about 0·25 of an inch apart, and the smoking taper was placed in the middle between them level with the centres of the passages, the effect was very good; for the smoke passed axially and issued out at the further ends of the pole passages.

Coal-gas delivered in the same place also passed axially, *i. e.* into the pole passages and parallel to the line joining them.

A little consideration easily leads to the true cause of these effects, and shows that they are not inconsistent with the former results. The law of all these actions is, that if a particle, placed amongst other particles, be more diamagnetic (or less magnetic) than them, and free to move, it will go from strong to weaker places of magnetic action; also, that particles less diamagnetic will go from weaker to stronger places of action. Now with the poles just described, the line or lines of maximum force, are not coincident with the axis of the holes pierced in the poles, but lie in a circle having a diameter, probably, a little larger than the diameter of the holes; and the lines within that circle will be of lesser power, diminishing in force towards the centre. A hot particle therefore within that circle will be driven inwards, and, being urged by successive portions of matter driven also inwards, will find its way out at the other ends of the passages, and therefore seem to go in an axial direction; whilst a hot particle outside of that circle of lines of maximum force will be driven outwards, and so, with others, will form the two tongues of flame which pass off in the equatorial direction. By bringing the glowing taper to different parts, the circle of lines of maximum magnetic intensity can be very beautifully traced; and by placing the taper inside or outside of that circle, the smoke could be made to pass axially or equatorially at pleasure.

I arranged an apparatus on this principle for trying the

gases, but did not find it better than, or so good as, the one I have described.

Such are the results I have obtained in verifying and extending the discovery made by P. Bancalari. I would have pursued them much further, but my present state of health will not permit it: I therefore send them to you with, probably, many imperfections. It is now almost proved that many gaseous bodies are diamagnetic in their relations, and probably all will be found to be so. I say almost proved; for it is not, as yet, proved in fact. That many, and most, gaseous bodies are subject to magnetic force is proved; but the zero is not yet distinguished. Now, until it is distinguished, we cannot tell which gaseous bodies will rank as diamagnetic and which as magnetic; and, also, whether there may not be some standing at zero. There is evidently no natural impossibility to some gases or vapours being magnetic, or that some should be neither magnetic nor diamagnetic. It is the province of experiment to decide such points; and the affirmative or negative may not be asserted before such proof is given, though it may, very philosophically, be believed.

For myself I have always believed that the zero was represented by a vacuum, and that no body really stood with it. But though I have only guarded myself from asserting more than I knew, Zantedeschi (and I think also De la Rive), with some others, seem to think that I have asserted the gases are *not* subject to magnetic action; whereas I only wished to say that I could not find that they were, and perhaps were not: I will therefore quote a few of my words from the *Experimental Researches*. Speaking of the preparation of a liquid medium at zero, I say, "Thus a *fluid* medium was obtained, which practically, as far as I could perceive, had every magnetic character and effect of a gas, *and even of a vacuum*, &c."—*Experimental Researches*, 2423. Again, at (2433) I say, "At one time I looked to air and gases as the bodies which allowing attenuation of their substance without addition, would permit of the observation of corresponding variations in their magnetic properties, but now all such power by rarefaction *appears* to be taken away." And further down at (2435), "Whether the negative results obtained by the use of gases and vapours depend upon the *smaller quantity of matter* in a given volume, or whether they are the direct consequences of the altered physical condition of the substance, is a point of very great importance to the theory of magnetism. I have imagined in elucidation of the subject an experiment, &c., but expect to find great difficulty in carrying it into execution, &c." Happily P. Bancalari's discovery has now settled this matter

for us in a most satisfactory manner. But where the true zero is, or that every body is more or less removed from it on one side or the other, is not, as yet, experimentally shown or proved.

I cannot conclude this letter without expressing a hope that since gases are shown to be magnetically affected, they will also shortly be found, when under magnetic influence, to have the power of affecting light (Experimental Researches, 2186, 2212). Neither can I refrain from signaling the very remarkable and direct relation between the forces of heat and magnetism which is presented in the experiments on flame, and heated air and gases. I did not find on a former occasion (Experimental Researches, 2397) that solid diamagnetic bodies were sensibly affected by heat, but shall repeat the experiments and make more extensive ones, if the Italian philosophers have not already done so. In reference to the effect upon the diamagnetic gases, it may be observed that, speaking generally, it is in the same direction as that of heat upon iron, nickel and cobalt; *i. e.* heat tends in the two sets of cases, either to the diminution of magnetic force, or the increase of diamagnetic force; but the results are too few to allow of any general conclusion as yet.

As air at different temperatures has different diamagnetic relations, and as the atmosphere is at different temperatures in the upper and lower strata, such conditions may have some general influence and effect upon its final motion and action, subject as it is continually to the magnetic influence of the earth.

I have for the sake of brevity frequently spoken in this letter of bodies as being magnetic or diamagnetic in relation one to another, but I trust that in all the cases no mistake of my meaning could arise from such use of the terms, or any vague notion arise respecting the clear distinction between the two classes, especially as my view of the true zero has been given only a page or two back.

I am, my dear Sir,

Yours, &c.,

M. FARADAY.

*Richard Taylor, Esq.,  
Ed. Phil. Mag., &c. &c.*

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LXV. *On the Motions presented by Flame when under the Electro-Magnetic Influence.* By Prof. ZANTEDESCHI.

THE most eminent philosophers have at all times maintained the universality of the magnetism of bodies\*; and in our days Faraday is the only one who has placed the expansi-

\* *Raccolta Fisico-Chimica Italiana*, t. iii. Dei corpi magnetici e diamagnetici.



ble fluids at the zero of the scale of action among magnetic and diamagnetic bodies. On the 21st of September 1847, at the Physical Section of the Ninth Italian Scientific Congress in Venice, Padre Bancalari, Professor of Physics in the Royal University of Genoa, read a memoir on the universality of magnetism; and the argument was considered by philosophers to be of such importance, that a desire arose to verify chiefly the action of magnetism on expansible fluids. It was announced by the Reporter Belli at the sitting of the 27th of September, that it had been proved in the presence of various philosophers that, on the interposition of a flame between the two poles of an electro-magnet, it was repulsed at the instant the electric current was closed, to return to the first position the instant it was broken. This discovery received well-merited applause in the sitting of the 28th of September, from the General Secretary and the Secretary of the Section of Physics. A wish was expressed by some to witness the experiment of Bancalari; and a Daniell's apparatus having been got ready, of ten elements eighteen centimetres each in dimension, I endeavoured to repeat the experiment in the Cabinet of Physics of the Royal Imperial Lyceum of Venice; but I did not chance to see the asserted phænomenon. My temporary magnet had the power of sustaining above 48 kilogrms. weight; but as my principle is, that a negative argument never destroys a positive one, I for my further information requested the machinist Cobres to give me the particulars of the apparatus; Belli not having treated of these in his report, and they having escaped Prof. Zambra, the Secretary of the Section. I knew that the two pieces of soft iron, which constituted the interrupted anchor, were perforated in the axial direction. I suspected that the repulsion of the flame was not the immediate effect of the magnetism, but of two currents of air issuing from the apertures of the perforated keeper generated by a vorticoise movement produced by the magnetism, as the celebrated Faraday had observed in liquids\*; and I was confirmed in this suspicion by the negative experiment which I had instituted in Venice with solid pieces. On arriving in Turin, I communicated my doubts to the well-known mechanics Jest, father and son, who to their professional abilities unite a rare courtesy. They soon furnished me in their laboratory with a Bunsen's apparatus, and constructed terminal pieces of soft iron forming the interrupted anchor, both solid and pierced, of a parallelepipedon and cylindric form, as I pointed out to them; and I have repeated the experiments in their company: the temporary

\* *Raccolta*, cited above, t. ii. Relazione dell' influenza delle forze elettriche e magnetiche sulla luce ed il calorico.

magnet, made in the shape of a horseshoe, was formed of a cylinder of soft iron of the length of  $0^m\cdot335$  and the diameter of  $0^m\cdot015$ ; and its electro-magnetic spiral was formed of a copper wire  $33^m$  long, and of a diameter of a millimetre and a third; the internal distance of the poles was  $0^m\cdot027$ ; the two solid parallelepipedon contacts, forming the interrupted anchor, were  $0^m\cdot04$  long; and of the sides  $0^m\cdot011$  and  $0^m\cdot006$ : and the hollow terminal pieces were  $0^m\cdot035$  long; and of the side  $0^m\cdot009$ . They were placed at a distance from one another of four to five millimetres, the magnet being kept in a vertical position with the poles turned upwards. In front of the interval of the separation of the contact pieces was placed the flame of a small candle, or of a little oil or alcohol lamp, so that it surmounted with its top by nearly a fourth the thickness of the contacts. The electric circuit was closed by copper wires, and the metallic unions were maintained both at the magnetic poles and at those of the pile by clamps: one of the wires therefore was divided into two equal parts, and the ends being dipped into a tumbler of mercury, allowed the closing and opening of the circuit at pleasure.

*I have constantly observed repulsion in the act of closing the circle, which lasted the whole time that the magnetism was kept up; and, when in the act of opening the circle, I saw the flame return to its primitive position. Well-satisfied with having in this manner confirmed this important fact which reflects honour on its discoverer, I applied myself to the study of the phænomenon, and I found—*

I. *That this happens with contacts of both solid and hollow soft iron; whereupon I abandoned my suspicion that the movement of the flame was attributable to currents of air; I convinced myself that it was an immediate action of the magnetism upon the flame,—a fact of the greatest importance to science.*

II. *That the repulsion, when it is quite distinct and the flame quite pure, and terminated in a well-shaped top, is accompanied by depression: repulsion and depression are simultaneously observed at the closing of the circle; the return of the flame and rising of the same, at the opening of the circle.*

III. *That, ceteris paribus, the greatest effect takes place when the flame is touching the convex of the magnetic curves indicated by iron filings.*

IV. *That the action is null, or almost null, when the flame is placed in the centre of the interval which separates the two contacts.*

V. *That in the manifestation of the effects stated above, it is not necessary for the contacts to be entirely separated: they may*

be placed at an angle and touch at two corners; the flame placed within the base of this triangle, generally manifests the two phænomena indicated.

VI. *That there is a certain mass of the contacts (or keeper pieces) which is the most efficacious: beyond a limit, which can be shown by experiment, increase of the mass causes a diminution of the effect:* from this I found the cause of my negative results, which I obtained in Venice in the first experiments that I made.

VII. *That the movements of the flame increase with the number of the pairs (of battery plates). With one pair the effect was not perceptible to me\*: with two pairs the movements began to show themselves; with three pairs they became distinct, and increased with the increase of the number of pairs up to ten, which was the greatest that I employed in this experiment. The pairs were of the known ordinary size.*

On the repetition of the phænomena as above stated, the precaution was taken to cover the apparatus with a bell, which was open above and supported by two discs below, which left a free access to the air, by which to support the combustion: in this manner all agitation and danger of disturbance under the circumstances were avoided.

I must not forget, in concluding this article, to state that the celebrated Prof. Gazzaniga, starting from his numerous experiments, which demonstrate the influence of magnetism upon the same aëriiform fluids, in a manner therefore different from that of Bancalari, was induced to consider the sun and all the other celestial bodies as so many enormous magnets; by which he established that attraction is merely an effect of the magnetism of the great celestial masses placed at an enormous distance,—an idea which reappeared in 1846 in Prussia, and in 1847 in France, as we see from the *Comptes Rendus* of the Royal Academy of Sciences at Paris. The mystery that attraction operates at a distance without inter-media would be removed in this case, and the phænomena of attraction would enter again into the class of those of common dynamics.

Dalla Gazz. Piem., Oct. 12, 1847, No. 242.

\* Messrs. Jest prepared for me last evening an electro-magnet of a circular form interrupted by a prismatic section having an interval of two millimetres; and I had, without need of contact pieces, the phænomena distinct with a single element. The most conspicuous movements here appeared in the greater proximity of the flame to the section.

The complete apparatus, of a circular form, furnished with a glass bell with its accessories is sold in Turin by Messrs. Jest, at the price of thirty francs, not including the electro-motor.

LXVI. *On Asymptotic Straight Lines, Planes, Cones and Cylinders to Algebraical Surfaces.* By THOMAS WEDDLE\*.

IN the Cambridge Mathematical Journal, first series, vol. iv. pp. 42-47, the late D. F. Gregory gave a very excellent method of determining the asymptotes to algebraical curves. I here purpose considering the corresponding subject relative to algebraical surfaces; and as this seems to have as yet engaged but little attention (if any), I trust the discussion will not be unacceptable to the mathematical readers of this Journal.

*Definitions.*

1. A straight line which passes through a point at a finite distance and touches a surface at an infinite distance, is called an *asymptotic straight line*, or simply an *asymptote* to the surface.

2. If every straight line drawn in a plane be an asymptote to a surface, the plane is styled a *CONICAL asymptotic plane* to the surface.

3. If all straight lines drawn in a plane parallel to a straight line in that plane be asymptotes to a surface, the plane is denominated a *CYLINDRICAL asymptotic plane* to the surface.

4. An *asymptotic cone* or *cylinder* to a surface is a cone or cylinder having its generators asymptotes to the surface.

If  $\phi_q(xyz)$  denote a homogeneous function of  $x, y, z$  of the  $q$ th degree, it is plain that a surface of the  $p$ th degree may be denoted thus:

$$\phi_p(xyz) + \phi_{p-1}(xyz) + \dots + \phi_1(xyz) + \phi_0 = 0. \quad (1.)$$

Let

$$\frac{x-\alpha}{l} = \frac{y-\beta}{m} = \frac{z-\gamma}{n} = r \dagger \quad (2.)$$

be the equations of an asymptote to (1.) passing through the point  $(\alpha\beta\gamma)$ : hence

$$x = lr + \alpha, \quad y = mr + \beta, \quad \text{and} \quad z = nr + \gamma;$$

substitute these values of  $x, y$  and  $z$  in (1.) and develope each term, the result is,

\* Communicated by the Author.

† The axes may be either rectangular or oblique; only in the former case we shall have

$$l^2 + m^2 + n^2 = 1;$$

but in the latter,

$$l^2 + m^2 + n^2 + 2fmm + 2gln + 2hlm = 1,$$

$f, g, h$  denoting the cosines of the angles which the axes make with each other.



$$\left. \begin{aligned} & \varphi_p r^p + (D\varphi_p + \varphi_{p-1})r^{p-1} + \left(\frac{1}{2} D^2\varphi_p + D\varphi_{p-1} + \varphi_{p-2}\right)r^{p-2} \\ & \dots\dots + \left(\frac{1}{2.3\dots s} D^s\varphi_p + \frac{1}{2.3\dots(s-1)} D^{s-1}\varphi_{p-1} + \dots\dots \right. \\ & \left. \dots\dots + D\varphi_{p-s+1} + \varphi_{p-s}\right)r^{p-s}\dots\dots = 0^*, \end{aligned} \right\} \quad (3.)$$

where  $D$  denotes the operation

$$\alpha \frac{d}{dl} + \beta \frac{d}{dm} + \gamma \frac{d}{dn}.$$

This equation will determine the values of  $r$  at the points in which the straight line (2.) cuts the surface (1.); now for all lines parallel to an asymptote, one of these points is evidently at an infinite distance; hence a root of (3.) being infinite, we must have

$$\varphi_p = 0; \quad . \quad . \quad . \quad . \quad . \quad . \quad (4.)$$

and this equation determines the directions of the asymptotes. The equation (3.) hence becomes

$$\left. \begin{aligned} & (D\varphi_p + \varphi_{p-1})r^{p-1} + \left(\frac{1}{2} D^2\varphi_p + D\varphi_{p-1} + \varphi_{p-2}\right)r^{p-2} \dots \\ & + \left(\frac{1}{2\dots s} D^s\varphi_p + \frac{1}{2\dots(s-1)} D^{s-1}\varphi_{p-1} + \dots\dots \right. \\ & \left. + D\varphi_{p-s+1} + \varphi_{p-s}\right)r^{p-s} + \dots\dots = 0; \end{aligned} \right\} \quad (5.)$$

in which values of  $l, m, n$  satisfying (4.) must be substituted. Now an asymptote being a tangent at an infinite distance, it follows that the asymptote will be distinguished from all lines having the same direction by a root of (5.) being infinite; we must therefore have

$$D\varphi_p + \varphi_{p-1} = 0;$$

that is,

$$\frac{d\varphi_p}{dl} \alpha + \frac{d\varphi_p}{dm} \beta + \frac{d\varphi_p}{dn} \gamma + \varphi_{p-1} = 0. \quad . \quad . \quad . \quad . \quad (6.)$$

The equation (4.) shows that every asymptote is parallel to some generator or other of the cone

$$\varphi_p(xyz) = 0; \quad . \quad . \quad . \quad . \quad . \quad . \quad (7.)$$

\* In this paper I restrict  $\theta, \phi, \psi, \chi$  (either with or without a letter or figure subscribed) to denote homogeneous functions only; and when these symbols stand alone, they are to be understood as functions of  $l, m, n$ ; in other cases the symbols of quantity must be written; thus  $\chi_q(xyz)$  (a homogeneous function of  $x, y, z$  of the  $q$ th degree) means the same function of  $x, y, z$  that  $\chi_q$  does of  $l, m, n$ .

and since  $(\alpha\beta\gamma)$  may be any point in each asymptote, (6.) denotes the locus  $(\alpha, \beta, \gamma)$  being the variable coordinates) of the asymptotes parallel to the same generator of (7.); this locus is therefore a cylindrical asymptotic plane, and it is parallel to that tangent plane of the cone (7.) which touches along the generator. Hence, to find the equation of a cylindrical asymptotic plane, we have only to take such values of  $l, m, n$  as satisfy (4.) and substitute them in (6.). It thus appears that when the cone (7.) is not imaginary, there is an indefinite number of cylindrical asymptotic planes; one indeed parallel to every tangent plane of the cone (7.), with a few exceptions, which I shall consider presently.

Should (4.), or, which is the same thing, (7.) be resolvable into factors, then (7.) will in reality denote as many conical surfaces; and if any of these factors be of the first degree, the corresponding conical surface will degenerate into a plane.

Let  $\theta_q$  be any factor of  $\phi_p$ , and put

$$\psi_{p-q} = \frac{\phi_p}{\theta_q};$$

hence (6.) becomes

$$\theta_q \cdot D \psi_{p-q} + \psi_{p-q} D \theta_q + \phi_{p-1} = 0,$$

when  $\theta_q = 0$ , this reduces to

$$\psi_{p-q} \left\{ \frac{d\theta_q}{dl} \alpha + \frac{d\theta_q}{dm} \beta + \frac{d\theta_q}{dn} \gamma \right\} + \phi_{p-1} = 0; \quad (8.)$$

and this equation, together with  $\theta_q = 0$ , will supply the place of (4.) and (6.) for those cylindrical asymptotic planes that are parallel to the tangent planes of the cone  $\theta_q(xyz) = 0$ . Also similar equations may be found for every factor of  $\phi_p$ .

If the equations

$$\phi_p = 0, \quad \frac{d\phi_p}{dl} = 0, \quad \frac{d\phi_p}{dm} = 0, \quad \frac{d\phi_p}{dn} = 0,$$

can be satisfied by simultaneous values  $(l_1, m_1, n_1)$  of  $l, m, n$ , (6.) cannot be satisfied unless  $\phi_{p-1}$  also  $= 0$ ; if  $\phi_{p-1}$  should not  $= 0$ , there will be no cylindrical asymptotic plane corresponding to these values of  $l, m, n$ ; but if  $\phi_{p-1} = 0$ , so that we have

$$\phi_p = 0, \quad \frac{d\phi_p}{dl} = 0, \quad \frac{d\phi_p}{dm} = 0, \quad \frac{d\phi_p}{dn} = 0, \quad \phi_{p-1} = 0^*, \quad (9.)$$

then (6.) will be satisfied independently of  $\alpha, \beta, \gamma$ . We have only to recur however to (5.), and equate to zero the coefficient

\* Since  $\phi_p$  is a homogeneous function of  $l, m, n$  of the  $p$ th degree, we have

$$p\phi_p = \frac{d\phi_p}{dl} l + \frac{d\phi_p}{dm} m + \frac{d\phi_p}{dn} n;$$

hence the equations (9.) amount only to four independent equations—the last four.

428 Mr. T. Weddle on *Asymptotic Straight Lines, Planes,*  
of the first power of  $r$  that does not vanish independently of  
any relation among  $\alpha, \beta, \gamma$ . If this coefficient be that of  $r^{p-2}$ ,  
we have

$$\frac{1}{2} D^2 \phi_p + D \phi_{p-1} + \phi_{p-2} = 0;$$

that is,

$$\left. \begin{aligned} & \frac{1}{2} \frac{d^2 \phi_p}{dl^2} \alpha^2 + \frac{1}{2} \frac{d^2 \phi_p}{dm^2} \beta^2 + \frac{1}{2} \frac{d^2 \phi_p}{dn^2} \gamma^2 + \frac{d^2 \phi_p}{dm \cdot dn} \beta \gamma + \frac{d^2 \phi_p}{dl \cdot dn} \alpha \gamma \\ & + \frac{d^2 \phi_p}{dl \cdot dm} \alpha \beta + \frac{d \phi_{p-1}}{dl} \alpha + \frac{d \phi_{p-1}}{dm} \beta + \frac{d \phi_{p-1}}{dn} \gamma + \phi_{p-2} = 0 \end{aligned} \right\} \quad (10.)$$

This equation denotes a surface which is evidently the locus  
of the asymptotes which are parallel to that generator of (7.)

whose equations are  $\frac{x}{l_1} = \frac{y}{m_1} = \frac{z}{n_1}$ . Hence (10.) must denote

a cylindrical surface; and as its generators are all asymptotes,  
it is an asymptotic cylinder of the second degree (which may  
in certain cases degenerate into one or two cylindrical asymp-  
totic planes). Should the values of  $l, m, n$  satisfying (9.) also  
cause  $\alpha, \beta, \gamma$  to vanish from (10.), there will be no correspond-  
ing asymptotic cylinder, unless  $\phi_{p-2} = 0$ ; and in this case we  
must equate the coefficient of  $r^{p-3}$  in (5.) to zero, and we shall  
have an asymptotic cone of the third degree; and so on.

Hence, to determine the equations of the asymptotic cylin-  
ders to the surface (1.), we must find such values (if any) of  
 $l, m, n$  as satisfy (9.), and substitute them in (10.); if all the  
terms of (10.) also vanish, we must recur to the coefficient of  
 $r^{p-3}$  in (5.); and so on. There will be as many asymptotic  
cylinders as there are sets of values of  $l, m, n$  satisfying (9.),  
unless, after substituting any set in (10.), &c., the only term  
that does not vanish is that independent of  $\alpha, \beta, \gamma$ , in which  
case there will be no asymptotic cylinder for this set of values.

If  $\phi_p$  contain a factor of the form  $\{\theta_q\}^2$ , the first four equa-  
tions of (9.) will be satisfied by  $\theta_q = 0$ ; and this, combined with  
 $\phi_{p-1} = 0$ , will give determinate values for the ratios  $l+m+n$ ,  
and the corresponding asymptotic cylinders will be determined  
in the way just mentioned. It may happen however that  $\theta_q$  is  
also a factor of  $\phi_{p-1}$ ; and if so, all the equations (9.) will be  
satisfied by  $\theta_q = 0$ , and (10.) now admits of simplification as  
follows. Let

$$\phi_p = \{\theta_q\}^2 \cdot \psi_{p-2q}, \text{ and } \phi_{p-1} = \theta_q \cdot \psi'_{p-q-1},$$

then it may easily be shown that when  $\theta_q = 0$ ,

$$D^2 \phi_p = 2 \psi_{p-2q} \cdot \{D \theta_q\}^2 \text{ and } D \phi_{p-1} = \psi'_{p-q-1} \cdot D \theta_q.$$

Hence (10.) becomes

$$\psi_{p-2q} \cdot \{D\theta_q\}^2 + \psi'_{p-q-1} \cdot D\theta_q + \phi_{p-2} = 0;$$

that is,

$$\left. \begin{aligned} &\psi_{p-2q} \cdot \left\{ \frac{d\theta_q}{dl} \alpha + \frac{d\theta_q}{dm} \beta + \frac{d\theta_q}{dn} \gamma \right\}^2 \\ &+ \psi'_{p-q-1} \left\{ \frac{d\theta_q}{dl} \alpha + \frac{d\theta_q}{dm} \beta + \frac{d\theta_q}{dn} \gamma \right\} + \phi_{p-2} = 0; \end{aligned} \right\} \quad . \quad . \quad (11.)$$

which evidently denotes two parallel cylindrical asymptotic planes; also since  $l, m, n$  are here only connected by the equation  $\theta_q = 0$ , it appears that there are in general two cylindrical asymptotic planes parallel to every tangent plane of the cone  $\theta_q(xyz) = 0$ .

Generally, let  $\{\theta_q\}^s, \{\theta_q\}^{s-1}, \{\theta_q\}^{s-2} \dots \theta_q$  be factors of  $\phi_p, \phi_{p-1}, \phi_{p-2} \dots \phi_{p-s+1}$ , and put

$$\phi_p = \psi \cdot \{\theta_q\}^s, \phi_{p-1} = \psi' \cdot \{\theta_q\}^{s-1}, \phi_{p-2} = \psi'' \cdot \{\theta_q\}^{s-2} \dots \phi_{p-s+1} = \psi^{(s-1)} \cdot \theta_q$$

(here the subscribed letters relative to  $\psi, \psi', \&c.$  are omitted for simplicity), then it may easily be shown that when  $\theta_q = 0$ , we have

$$D\phi_p = 0, D^2\phi_p = 0 \dots D^{s-1}\phi_p = 0, D^s\phi_p = 2.3\dots s \cdot \psi \cdot \{D\theta_q\}^s, \&c.$$

Moreover, the equation to the asymptotic cylinder parallel to a generator of the cone  $\theta_q(xyz) = 0$ , will, by equating to zero the first coefficient of (5.) that does not vanish independently of  $\alpha, \beta, \gamma$ , be found to be

$$\frac{1}{2.3\dots s} D^s\phi_p + \frac{1}{2.3\dots(s-1)} D^{s-1}\phi_{p-1} + \dots + D\phi_{p-s+1} + \phi_{p-s} = 0;$$

and this, by what precedes, reduces to

$$\left. \begin{aligned} &\psi \cdot \{D\theta_q\}^s + \psi' \cdot \{D\theta_q\}^{s-1} + \psi'' \cdot \{D\theta_q\}^{s-2} + \dots \\ &+ \psi^{(s-1)} \cdot D\theta_q + \phi_{p-s} = 0. \end{aligned} \right\} \quad . \quad (12.)$$

Since

$$D\theta_q = \frac{d\theta_q}{dl} \alpha + \frac{d\theta_q}{dm} \beta + \frac{d\theta_q}{dn} \gamma,$$

it is evident that the asymptotic cylinder degenerates into  $s$  cylindrical asymptotic planes, all parallel to a tangent plane of the cone  $\theta_q(xyz) = 0$ ; and there is in general the same number parallel to every tangent plane of this cone.

The asymptotes to the surface (1.) passing through a given point  $(\alpha\beta\gamma)$  will be found by determining the ratios  $l+m+n$  by (4.) and (6.), and substituting, in succession, each set of



simultaneous values in (2.); the resulting equations will be those of the asymptotes to the surface that pass through the point  $(\alpha\beta\gamma)$ .

Since (4.) is of the  $p$ th degree and (6.) of the  $(p-1)$ th, the equation resulting from the elimination of  $l$  (suppose) from (4.) and (6.) cannot exceed the  $p(p-1)$ th degree, and consequently there cannot be more than  $p(p-1)$  values of the ratio  $m \div n$ . From this we learn, that through any point in space there cannot be drawn more than  $p(p-1)$  asymptotes to a surface of the  $p$ th degree.

This theorem suffers an exception, however, which I proceed to consider.

It may happen that the point  $(\alpha\beta\gamma)$  through which the asymptotes are to be drawn may be so taken as to cause (4.) and (6.) to have a common factor  $\chi_q$  (which I shall suppose to be their greatest common measure). In this case (4.) and (6.) will be satisfied if  $\chi_q = 0$ ; and eliminating  $l, m, n$  from this equation by means of (2.), we have

$$\chi_q(x-\alpha, y-\beta, z-\gamma) = 0$$

for the equation to the asymptotic cone, which is the locus of the innumerable asymptotes that pass through the point  $(\alpha\beta\gamma)$ . (The factor  $\chi_q$  may sometimes be resolvable into other factors, and then the preceding asymptotic cone of the  $q$ th degree will in fact consist of several cones of inferior degrees.)

The division of (4.) and (6.) by  $\chi_q$  will give two equations,  $\chi'_{p-q} = 0$ , and  $\chi''_{p-q-1} = 0$ , which admit of no common measure. Now (4.) and (6.) will be satisfied by these two equations; but the equations  $\chi'_{p-q} = 0$ ,  $\chi''_{p-q-1} = 0$ , will determine not more than  $(p-q)(p-q-1)$  sets of values of the ratios  $l \div m \div n$ , hence (excluding the generators of the cone corresponding to  $\chi_q$ ) not more than  $(p-q)(p-q-1)$  asymptotes can pass through the point  $(\alpha, \beta, \gamma)$ .

In order to find those points (if any) which are the vertices of asymptotic cones, eliminate one of the quantities  $l, m, n$  from (4.) and (6.), and find those values of  $\alpha, \beta, \gamma$  that will render all the coefficients of the resulting equation equal to zero. If no such values be possible, the surface (1.) does not admit of an asymptotic cone; but if values  $\alpha_1, \beta_1, \gamma_1$  of  $\alpha, \beta, \gamma$  can be found, then the point  $(\alpha_1 \beta_1 \gamma_1)$  will be the vertex of an asymptotic cone. To find the equation of this cone, we must substitute  $\alpha_1, \beta_1, \gamma_1$  for  $\alpha, \beta, \gamma$  in (6.), and ascertain  $\theta_q$  the common measure of (4.) and (6.) thus modified; then will  $\theta_q(x-\alpha_1, y-\beta_1, z-\gamma_1) = 0$  be the equation to the asymptotic cone, having its vertex at the point  $(\alpha_1 \beta_1 \gamma_1)$ . If the equation resulting from the elimination of  $l, m$ , or  $n$  from (4.) and (6.) can be rendered identically

zero by other simultaneous values of  $\alpha, \beta, \gamma$ , there will be as many asymptotic cones as there are sets of values. When the elimination referred to above is effected by the process for the common measure, the factor  $\theta_q$  will be the last of the remainders that do not vanish when  $\alpha_1, \beta_1, \gamma_1$  are substituted for  $\alpha, \beta, \gamma$ . It will sometimes be found, however, that (4.) and (6.) have a common measure independently of  $\alpha, \beta, \gamma$ , arising from  $\{\theta_q\}^2$  and  $\theta_q$  being factors of  $\phi_p$  and  $\phi_{p-1}$ ; and in this case we must proceed with this common measure in the way to be noticed presently.

When we know that (4.) cannot be resolved into factors, the determination of the asymptotic cone is very easy; for since (4.) admits of no measure but itself, and (6.) is of an *inferior degree*, it is evident that if there be an asymptotic cone, (6.) must be identically zero; hence if such values  $\alpha_1, \beta_1, \gamma_1$  can be given to  $\alpha, \beta, \gamma$  as to cause the coefficients of (6.) to vanish, there will be an asymptotic cone of the  $p$ th degree, namely,

$$\phi_p(x - \alpha_1, y - \beta_1, z - \gamma_1) = 0;$$

but if the coefficients cannot be rendered zero simultaneously, there will be no asymptotic cone. Since  $\alpha, \beta, \gamma$  enter (6.) in the first degree only, there will evidently be at most only one set of values of  $\alpha, \beta, \gamma$  that will render (6.) identically zero; and hence a surface of the  $p$ th degree *may* have one asymptotic cone of the  $p$ th degree, but not more, and it is plain that there cannot be an asymptotic cone of a higher degree.

If (4.) admits of being resolved into factors, and these factors can be found, the asymptotic cones may be determined as follows. Let  $\theta_q$  be one of the factors of  $\phi_p$ , and let  $\theta_q$  itself be irresolvable into factors. Arrange (6.), or rather (8.), and  $\theta_q$  according to the powers of either  $l, m$  or  $n$  ( $l$  suppose), and divide the former by the latter until the remainder is of lower dimensions in  $l$  than  $\theta_q$ ; then since  $\theta_q$  is irresolvable into factors, it is clear that this remainder must be identically zero: find therefore  $\alpha_1, \beta_1, \gamma_1$ , the values of  $\alpha, \beta, \gamma$ , that make the coefficients of the remainder vanish, then  $\theta_q(x - \alpha_1, y - \beta_1, z - \gamma_1) = 0$  will be the asymptotic cone. As  $\alpha, \beta, \gamma$  enter (8.) in the first degree and do not enter  $\theta_q$ , there cannot be more than one set of values of  $\alpha, \beta, \gamma$ , if indeed there be any. The same process being repeated with each of the other prime factors into which (4.) is resolvable, we shall have all the asymptotic cones which the surface admits of.

The preceding process requires modification when the second or any higher power of  $\theta_q$  is a factor of  $\phi_p$ . As an example, suppose that  $\{\theta_q\}^4$  enters as a factor into  $\phi_p$ , and put  $\phi_p = \psi \cdot \{\theta_q\}^4$  ( $\theta_q$  not being a factor of  $\psi$ ). When  $\theta_q = 0$ ,  $D\phi_p$

$+ \phi_{p-1} = 0$ , reduces to  $\phi_{p-1} = 0$ , and consequently there will be no asymptotic cone unless  $\theta_q$  be a factor of  $\phi_{p-1}$ ; if so, let  $\phi_{p-1} = \psi' \cdot \theta_q$ , then

$$\frac{1}{2} D^2 \phi_p + D \phi_{p-1} + \phi_{p-2} = 0$$

becomes  $\psi' \cdot D \theta_q + \phi_{p-2} = 0$ , which is of the first degree in  $\alpha, \beta, \gamma$ , and this (instead of (8.)) being combined with  $\theta_q = 0$ , may give an asymptotic cone. If  $\{\theta_q\}^2$  however be a factor of  $\phi_{p-1}$ , then

$$\frac{1}{2} D^2 \phi_p + D \phi_{p-1} + \phi_{p-2} = 0$$

becomes  $\phi_{p-2} = 0$ , and there will be no asymptotic cone unless  $\theta_q$  be a factor of  $\phi_{p-2}$ . If this be the case, assume  $\phi_{p-1} = \psi' \cdot \{\theta_q\}^2$ , and  $\phi_{p-2} = \psi'' \cdot \theta_q$ , then

$$\frac{1}{2 \cdot 3} D^3 \phi_p + \frac{1}{2} D^2 \phi_{p-1} + D \phi_{p-2} + \phi_{p-3} = 0$$

reduces to

$$\psi' \cdot \{D \theta_q\}^2 + \psi'' \cdot D \theta_q + \phi_{p-3} = 0;$$

and this equation, which replaces (8.), combined with  $\theta_q = 0$ , may give one or two asymptotic cones (but not more, as will be shown below), unless  $\theta_q$  should enter both  $\phi_{p-1}$  and  $\phi_{p-2}$  in a higher power than has been supposed; we shall then have  $\phi_{p-3} = 0$ ; and hence  $\theta_q$  must (if there be an asymptotic cone) be a factor of  $\phi_{p-3}$ . Suppose therefore

$$\phi_{p-1} = \psi' \cdot \{\theta_q\}^3, \quad \phi_{p-2} = \psi'' \cdot \{\theta_q\}^2, \quad \text{and} \quad \phi_{p-3} = \psi''' \cdot \theta_q,$$

then

$$\frac{1}{2 \cdot 3 \cdot 4} D^4 \phi_p + \dots + \phi_{p-4} = 0$$

becomes

$$\psi \cdot \{D \theta_q\}^4 + \psi' \cdot \{D \theta_q\}^3 + \psi'' \cdot \{D \theta_q\}^2 + \psi''' \cdot D \theta_q + \phi_{p-4} = 0;$$

and this equation (which cannot be satisfied independently of  $\alpha, \beta, \gamma$ , for  $\theta_q$  is not a factor of  $\psi$ ), combined with  $\theta_q = 0$ , may give four asymptotic cones.

Similarly, if  $\{\theta_q\}^s$  be the highest power of  $\theta_q$  that is a factor of  $\phi_p$ , it may be shown that  $\alpha, \beta, \gamma$  enter the equation to be combined with  $\theta_q = 0$ , only through  $D \theta_q$ , and that this equation may rise to any degree in  $D \theta_q$  (except the  $(s-1)$ th) not exceeding  $s$ .

Hence when a power  $(s)$  of  $\theta_q$  is a factor of  $\phi_p$ , we must ascertain the highest powers of  $\theta_q$  that are factors of  $\phi_{p-1}, \phi_{p-2}, \dots, \phi_{p-t+1}$ , also  $\phi_{p-t}$  the first term of (1.) that has not  $\theta_q$  for a factor; we must then equate to zero the coefficient (reduced as above) of the highest power of  $r$  in (5.) that does not vanish independently of  $\alpha, \beta, \gamma$ . If  $\alpha, \beta, \gamma$  disappear from this equation

so that it becomes  $\phi_{p-t}=0$ , there will be no asymptotic cone; but if this be not the case, then the reduced equation must be combined with  $\theta_q=0$ , in the same way as directed for (8.) and  $\theta_q=0$ , and we may get asymptotic cones though not more than  $s$  of them. I proceed to establish the last assertion.

It has been shown above that if  $\{\theta_q\}^s$  be the highest power of  $\theta_q$  that is a factor of  $\phi_p$ , then the equation to be combined with  $\theta_q=0$  will be of the form

$$\psi.\{D\theta_q\}^t + \psi'.\{D\theta_q\}^{t-1} + \dots = 0, \quad (13.)$$

where  $\psi, \psi' \dots$  do not involve  $\alpha, \beta, \gamma$ , and  $t$  may be equal to, but cannot be greater than  $s$ . Now if there be a corresponding asymptotic cone, let  $(\alpha_1, \beta_1, \gamma_1)$  denote its vertex; then if

$$\frac{d\theta_q}{dl} \alpha_1 + \frac{d\theta_q}{dm} \beta_1 + \frac{d\theta_q}{dn} \gamma_1$$

(which I shall denote by  $D_1\theta_q$ ) be substituted for  $D\theta_q$  in (13.), the resulting equation will be satisfied by aid (if necessary) of  $\theta_q=0$ ; hence (13.) must be divisible by  $D\theta_q - D_1\theta_q$ , so that it may be written

$$(D\theta_q - D_1\theta_q)(\psi.\{D\theta_q\}^{t-1} + \dots) = 0. \quad (14.)$$

Also, if  $\alpha_2, \beta_2, \gamma_2$  be another set of values of  $\alpha, \beta, \gamma$ , satisfying (13.), they must reduce the second factor of (14.) to zero, for the first is of a lower degree than  $\theta_q$ . Hence  $\psi.\{D\theta_q\}^{t-1} + \dots$  must be divisible by  $D\theta_q - D_2\theta_q$ , and so on. In this way we shall, after a certain number ( $v$ ) of divisions, get an equation,

$$\psi.\{D\theta_q\}^{t-v} + \dots = 0,$$

which either does not contain  $D\theta_q$  (and hence  $\alpha, \beta, \gamma$ ) at all, or which cannot be satisfied by any values of  $\alpha, \beta, \gamma$ . Rejecting this factor then as affording no solution, (13.) is equivalent to

$$(D\theta_q - D_1\theta_q)(D\theta_q - D_2\theta_q) \dots (D\theta_q - D_v\theta_q) = 0,$$

and each of these factors will give but one set of values of  $\alpha, \beta, \gamma$ ; hence there will be but  $v$  asymptotic cones,

$$\theta_q(x - \alpha_1, y - \beta_1, z - \gamma_1) = 0 \dots \theta_q(x - \alpha_v, y - \beta_v, z - \gamma_v) = 0;$$

and since  $v$  cannot exceed  $t$ , nor  $t$  exceed  $s$ , it follows that there cannot be more than  $s$  asymptotic cones resulting from a factor of  $\phi_p$  of the form  $\{\theta_q\}^s$ .

When  $\theta_q$  is of the first degree, it is clear that instead of an asymptotic cone we shall have a plane; and since any point in it may be regarded as the vertex, every straight line drawn in it will be an asymptote; hence the asymptotic cone will in this case become a conical asymptotic plane: also since  $\theta_q$  is here of the form  $Al + Bm + Cn$ ,

$$D\theta_q = A\alpha + B\beta + C\gamma,$$



which does not involve  $l, m$  or  $n$ . Hence to determine the conical asymptotic planes (if any) to the surface (1.), we must take those factors of  $\phi_p$  that are of the first degree, and proceed as directed above for asymptotic cones; with this modification, however, that  $D\theta_q$  not containing  $l, m$  or  $n$  must be regarded as a single constant, and consequently the process will be much simplified. If  $V_1, V_2 \dots V_t$  ( $t$  not  $\geq s$ ) be the values of  $D\theta_q$  corresponding to the factor

$$\{\theta_q\}^s = \{Al + Bm + Cn\}^s,$$

we shall have

$Ax + By + Cz = V_1, Ax + By + Cz = V_2 \dots Ax + By + Cz = V_t$   
as the equations to the conical asymptotic planes relative to this factor.

It appears from the preceding reasoning, that if the equation (4.), or, which is the same thing, the highest homogeneous function in the equation to the surface (1.) can be resolved into  $a$  factors of the first degree,  $b$  factors of the second degree,  $c$  factors of the third degree, &c. (here a factor of the form  $\{\theta_q\}^s$  is to be accounted  $s$  factors), then the surface may admit of, but cannot have more than  $a$  asymptotic cones of the first degree, that is,  $a$  conical asymptotic planes,  $b$  asymptotic cones of the second degree,  $c$  asymptotic cones of the third degree, &c. Some of these cones may have the same vertex; and since  $a + 2b + 3c \dots = p$ , the degree of the aggregate of all the asymptotic cones to a surface can never exceed that of the surface itself.

It will be seen that unless equal factors enter the highest homogeneous function, the asymptotic cones to a surface depend only on the two highest homogeneous functions in its equation; and hence (the above case excepted) all surfaces having the two highest homogeneous functions in their equations identical, will have the same asymptotic cones. Also conversely, it is plain that those surfaces that have the same asymptotic cones must have the two highest homogeneous functions in their equations identical, providing the degree of the equations to the surfaces be exactly equal to that of the aggregate of the cones. Now this aggregate may be considered one of these surfaces; hence if

$$u_1 = 0, u_2 = 0, \dots u_t = 0$$

be the equations to cones, the aggregate of which is of the  $p$ th degree, the equation to all the surfaces of the  $p$ th degree having these for asymptotic cones may be denoted by

$$u_1 u_2 \dots u_t + \chi_{p-2}(xyz) + \chi_{p-3}(xyz) \dots + \chi_1(xyz) + \chi_0 = 0. \quad (15.)$$

Wimbledon, Surrey, Nov. 10, 1847.

LXVII. *On the Chemical Composition of the Substances employed in Pottery.* By Mr. R. A. COUPER\*.

ALL kinds of earthenware are composed of two parts, viz. the body and the glaze.

The body is the principal part of the vessel, being the base or foundation, as indicated by the term itself. The glaze is a thin transparent layer of glass which covers the body and fills up its pores, giving it a smooth surface with a polished and a finished appearance.

I. The substances principally employed to form the body of earthenware are, clays of different kinds, flint and Cornish stone.

Clay which constitutes the base of the body of earthenware is distinguished from siliceous earth by becoming plastic when mixed with water, and being very soft and not gritty to the feel; also when burned, it keeps its form, and becomes firm and solid; whereas siliceous earth crumbles into a powder when burned. Clay when intensely heated, as in porcelain manufactories, does not regain its plasticity, which it loses in the burning, although pounded very fine, in which state it is technically termed potsherd.

Clay is obtained naturally from Cornwall, Dorset, and Devonshire, and is the finer particles of decomposed felspar deprived of its alkali.

1. The finest clay (termed *China clay*) used in Britain is obtained artificially from Cornwall, by running a stream of water over decomposed granite, which carries with it the finer particles of felspar, and is then received into catchpools or ponds where it is allowed to subside. The water is then run off, leaving a fine sediment, which is removed and exposed to the atmosphere for four or five months, when it is ready for export. By analysis of this clay previously dried at  $212^{\circ}$ , I found it to consist of—

	I.	II.
Silica . . . . .	46.32	46.29
Alumina . . . . .	39.74	40.09
Protoxide of iron . .	.27	.27
Lime . . . . .	.36	.50
Magnesia . . . . .	.44	
Water and some alkali	12.67	12.67
	<hr/> 99.80	<hr/> 99.82

For the second analysis I am indebted to Mr. John Brown. The more common clays, which are found naturally depo-

\* R ad before the Philosophical Society of Glasgow, April 28, 1847, and communicated by Dr. R. D. Thomson.

sited, are supposed to have been produced in a similar manner to the china clay; the rains having washed from the hills the decomposed rock into a lake or estuary, where it has subsided and gradually displaced the water, and become in the course of time perfectly firm and solid, forming fields of clay. The clay is found in layers or strata lying over each other, each layer possessing some distinctive property from the other, which renders each clay fitted for a peculiar purpose.

2. *Sandy clay* (stiff or ball) is the upper layer of clay, and is used by itself for making salt glazed ware; it is well adapted for this kind of ware, in consequence of the considerable quantity of silica or sand which it contains. By analysis of this clay, I found it to be composed of—

Silica . . . . .	66.68
Alumina . . . . .	26.08
Protoxide of iron . . . . .	1.26
Lime . . . . .	.84
Magnesia . . . . .	trace
Water . . . . .	5.14
	<hr/>
	100.00

being previously dried at  $212^{\circ}$ , specific gravity = 2.558.

3. *Pipe clay* is the second layer, which is used in making tobacco pipes. This clay is not employed in manufacturing earthenware, owing to its possessing the property of contracting more than sandy clay. It was analysed by Mr. John Brown, who obtained—

Silica . . . . .	53.66
Alumina . . . . .	32.00
Protoxide of iron . . . . .	1.35
Lime . . . . .	.40
Magnesia . . . . .	trace
Water . . . . .	12.08
	<hr/>
	99.49

4. *Blue clay* is of a grayish colour, and is considered the best layer of clay in the whole series, owing to its burning perfectly white, and approaching in character nearest to the china clay. As analysed by Mr. John Higginbotham, it was found to consist of—

Silica . . . . .	46.38
Alumina . . . . .	38.04
Protoxide of iron . . . . .	1.04
Lime . . . . .	1.20
Magnesia . . . . .	trace
Water . . . . .	13.57
	<hr/>
	100.23

also previously dried at  $212^{\circ}$ . There is a variety of other clays obtained from these fields, which are of less value, and need not be enumerated here, as they are similar in appearance to those already noticed.

5. *Red or brown clay*, which is very abundant in the neighbourhood of Glasgow, is a surface clay, and contains a large quantity of peroxide of iron, which gives it a deep brown colour. It is of this clay that common black ware, flower-pots, and red bricks are made, which do not require a very high temperature, else they would fuse. The analysis gave—

Silica . . . . .	49.44
Alumina . . . . .	34.26
Protoxide of iron . . . . .	7.74
Lime . . . . .	1.48
Magnesia . . . . .	1.94
Water . . . . .	5.14
	<hr/>
	100.00

6. *Yellow clay* is obtained from various parts of the country, and is so called from possessing a yellow colour both before and after being burned, owing to the presence of iron.

By mixing sandy clay and red clay together, we gain an artificial yellow clay, which is often employed.

Yellow clay, as analysed by Mr. John Brown, was found to contain—

Silica . . . . .	58.07
Alumina . . . . .	27.38
Protoxide of iron . . . . .	3.30
Lime . . . . .	.50
Water . . . . .	10.30
Magnesia . . . . .	trace
	<hr/>
	99.55

7. *Fire-clay* is also very abundant in this country, and occurs both on the surface and several fathoms under ground. It is termed marl, and is used principally in potteries for making saggars or vessels for placing the ware previous to burning to protect them from the flame; and owing to its coarse particles, which cause the body to be very porous, is well adapted for strong heats: crucibles, or large pots for glass works, in which the glass is fused, are also made from fire-clay, as well as bricks known under the name of fire-brick. This clay was analysed by Mr. John Brown, who obtained—



Silica . . . . .	66.16
Alumina . . . . .	22.54
Protoxide of iron . . . . .	5.31
Lime . . . . .	1.42
Magnesia . . . . .	trace
Water . . . . .	3.14
	<hr/> 98.57

8. *Flint* as used in potteries is first calcined, then water-ground, in which state it is used for mixing with clays, and is called slop flint; but for glazes it is evaporated to dryness, and used in the dry state with other articles which constitute the glaze.

9. Cornish stone or granite is water-ground, then evaporated to dryness for mixing in glazes, and is used in the slop state for mixing with clays.

10. Plaster of Paris or gypsum, which is employed in forming the moulds in which certain kinds of pottery are cast, is a native sulphate of lime. It is a very important article to the manufacturer of earthenware, owing to its singular property of parting easily with the clay by the application of a slight heat. Plaster of Paris requires to be dried at a high temperature before using it; but if it is over-dried, it will not again set for making moulds; the drier the stucco the harder are the moulds that are made of it, and they will stand more readily a greater degree of wear. Plaster of Paris casts, as commonly prepared, cannot again be used for the same purpose.

II. The colours used for printing and painting on ware are similar to one another, excepting that the colours for painting may not be so expensive as for printing; both however form an important and extensive part of the materials of a pottery. The manufacturers of earthenware are much occupied with the improvement of the variety and beauty of the colours, as well as of the patterns or styles that are produced, and hence a great emulation exists among those employed in the trade.

1. The blue colour in printing is produced from cobalt, which is used with flint, ground glass, pearlash, white lead, barytes, china clay, and oxide of tin in reducing its strength.

2. The brown colour by ochre, manganese, and cobalt.

3. The black colour by chromate of iron, nickel, ironstone, and cobalt.

4. The green colour by chrome, oxide of copper, lead, flint, and ground glass.

5. The pink colour by chrome, oxide of tin, whiting, flint ground glass, and china clay, which are mixed in various proportions, fused together at a high temperature, then pounded and mixed with oil, when it is ready for the printer's use.

For the following analysis of a blue cobalt calx, I am indebted to Mr. John Adam:—

Silica . . . . .	17·84
Peroxide of cobalt . . . . .	19·42
Peroxide of iron . . . . .	25·50
Water . . . . .	8·41
Carbonate of lime and magnesia .	28·45
	<hr/> 99·62

The oil that is used for mixing with the colours, is made by boiling the following substances together; viz. linseed oil, rape oil, sweet oil, rosin, common tar, and balsam copaiba in various proportions.

III. It is but recently since a new method has been applied to cause the colours to flow or spread over the surface of the ware. This object is effected by washing the saggars in which the ware is placed previous to its being fired in the glost kiln, with a mixture of—

1. Lime, common salt, and clay slip. Dry flows are also used, which answer equally well, the mixture being sprinkled on the bottom of the saggar. The following are some of those flows:—

2. Lime, sal-ammoniac and red lead.

3. Lime, common salt, and soda.

4. Whiting, lead, salt and nitre.

5. But there is a wash made of lime, clay slip, nitre, salt, lead, in general use for washing all the saggars employed in the glost kiln, which fuses on the inner surface of the saggar, making it perfectly close and not porous, otherwise the gloss required on the surface of the ware would not be obtained.

IV. The colours used in producing the dipt or sponged ware are of a very cheap kind, as it is only for common purposes that they are employed. The colours when used for dipt ware are put on the ware before it is burned; and when used for sponged ware, are put on the ware in the biscuit state. The following are some of those colours:—

1. A black dip is made from manganese, ironstone and clay slip.

2. A drab dip by nickel and slip.

3. A sage or a greenish-blue dip by green chrome and slip.

4. A blue dip by cobalt and clay slip.

5. A yellow dip by yellow clay alone, or a compound of white and red clay, which produces the same results.

6. A red dip is produced from the red or brown clay; but it is not every quality of this clay that will answer, as it requires to burn red.

The first four of these dips are prepared by mixing a little

of the colouring agent with a quantity of clay slip; whilst the two last-mentioned dips are mixed with water to produce the slip state, in which state they are employed.

V. There are several kinds of bodies manufactured; but they may be all classed under two heads, viz. porcelain and earthenware.

1. *Porcelain or china* is a rich, very smooth and transparent ware, and is the finest quality that has yet been manufactured. It is a fused body, and owes its transparency to this circumstance; it also requires a very high temperature to burn it, and is manufactured in this country from flint, Cornish stone (granite), china clay, and bone-earth; the lime employed acting as a flux, partly fusing it. By analysis of two pieces of china from different manufactories in Staffordshire, I found them to be differently composed. The last of these pieces was also analysed by Mr. Crichton, the three analyses being as follows:—

	No. 1, by R. A. C.	No. 2, by R. A. C.	No. 2, by W. C.
Silica . . . . .	39·88	40·60	39·685
Alumina . . . . .	21·48	24·15	24·650
Lime . . . . .	10·06	14·22	14·176
Protoxide of iron } Phosphate of lime }	26·44	15·32	15·386
Magnesia . . . . .	.....	·43	·311
Alkali or difference	2·14	5·28	5·792
	<hr/> 100·00	<hr/> 100·00	<hr/> 100·000

2. Foreign manufacturers do not employ bone-earth; but instead of it they use felspar, the alkali of which supplies the place of the phosphate of lime. The Germans make the best porcelain for chemical purposes, as that body is more vitrified and less liable to be acted upon by acids, as well as being capable of standing a very strong heat; and hence it is extensively used by chemists. By the analysis of some specimens of foreign porcelain, I obtained the following results:—

	Berlin.	Chinese Porcelain, superior.	inferior.
Silica . . . . .	72·96	71·04	68·96
Alumina and protoxide of iron	24·78	22·46	29·24
Lime . . . . .	1·04	3·82	1·60
Alkali . . . . .	1·22	2·68	
	<hr/> 100·00	<hr/> 100·00	<hr/> 99·80
Specific gravity . . . . .	2·419	2·314	2·314

VI. Earthenware is a very porous and less compact body than china or porcelain, owing to its containing little or no alkali, which is the great difference between these bodies. I had a piece of ware manufactured, resembling in appearance porcelain, as regards the absence of porosity and its compactness,

slightly transparent, and capable of standing a very strong and sudden heat; it was produced by mixing soda to the extent of  $3\frac{1}{2}$  per cent. in a little clay prepared for the common white body, and was then fired in the biscuit kiln. The clay employed having been previously well dried, so as to weigh it without water, the proportional quantity of soda requisite was then calculated and weighed out; the clay was again mixed with water along with the soda; it was then formed into capsules, which after being fired and then broken, presented the appearance of a vitrified or fused body.

1. The common white ware or earthenware is made from flint, Cornish stone, china clay, and blue clay, and does not require such a high temperature in burning as the porcelain does. By analysis of a piece of white ware manufactured in this city, it was found to contain—

Silica . . . . .	68·55
Alumina and protoxide of iron . . . . .	29·13
Lime . . . . .	1·24
	<hr/>
	98·92
Specific gravity . . . . .	2·36

Coloured ware is also manufactured from the same substances, but mixed with a colouring agent which stains the body.

2. The toqua or blue-coloured ware is coloured by cobalt.
3. The sage or greenish-blue coloured ware, by nickel and cobalt.
4. The drab or buff-coloured ware by chromate of iron.
5. The body for the cane or yellow-coloured ware is produced by a mixture of sandy clay and common red clay, the same as used for red bricks, but is generally produced from the natural yellow clay found in particular localities.
6. The last-mentioned body is also employed for making Rockingham ware, which only varies from the cane ware by possessing a different glaze.
7. The common black ware body is made from the red clay alone.
8. The Egyptian ware body is made from ironstone, ball and red clay.

These four last-mentioned bodies are not nearly so expensive as the white ware, and do not require nearly such a high temperature to burn them; therefore they are, comparatively speaking, soft bodies.

9. Salt glazed ware is made from sandy clay and a little sand, to keep the body open, or make it less compact; but for large salt glazed ware, potsherd, which is ware that has



been fired and then ground, is employed to render the body still more open or porous, and also to give it a greater capability of standing sudden heats or colds. This ware is much used in public works for chemical purposes: it is exposed to the action of the flame during burning, whereas other kinds of ware are protected by saggars from the flames.

VII. The glaze vitrifies the surface of the body, rendering it generally capable of withstanding acids. It is a very important point with the manufacturer to obtain a glaze which will adhere to the body without crazing or peeling off, as he may discover a good body, but not find a glaze to answer it, since every glaze will not adhere to the same body, and hence every manufacturer has a glaze of his own composition.

1. The substances used in the preparation of the glaze for white ware, are borax, china clay, flint, Cornish stone, Paris white, and white lead.

In preparing the glaze, a substance technically termed *frett* is first made, consisting of borax, china clay, flint, Cornish stone, and Paris white, which are fused together in a kiln, and when ready allowed to flow into water, which shortens it, owing to the water being mechanically lodged in it, and keeps it from adhering to the bottom of the vessel, rendering it much easier to pound. *Frett* is a beautiful glass, coloured by a little iron, and is pounded and water-ground along with Cornish stone, flint, and white lead: this constitutes the glaze for white ware.

	Analysis of white glaze.	Analysis of <i>frett</i> .
Silica . . . . .	43.66	55.98
Lime . . . . .	.52	2.52
Alumina and protoxide of iron	9.56	10.38
Borax . . . . .	20.08	31.12
Carbonate of lime . . . . .	10.88	
Carbonate of lead . . . . .	15.19	
	<hr/> 99.89	<hr/> 100.00
Specific gravity . . . . .		2.345

A piece of earthenware was brought from America, having been discovered several feet under ground, the glaze of which was tested, and found to be composed of silica, iron, alumina, lime, sulphate of lime and antimony, which was a beautiful rich white glaze concealing a common red clay body.

2. The glaze of Rockingham ware possesses a beautiful brownish metallic lustre, and is made from Cornish stone, flint, manganese, red lead and clay slip, the latter substance being a little clay mixed with water until it becomes of the consistency of milk.

3. The glaze for common black ware is made from the same

materials in different proportions, and has a brilliant black appearance.

4. The glaze used for cane or yellow-coloured ware is made from flint, red lead, and Cornish stone.

5. The Egyptian ware owes its value to the beautiful and rich tinted black glaze, made from flint, Cornish stone, red lead, and manganese, with which it is covered.

These four last-mentioned glazes are made by stirring the substances together with a certain quantity of water, and passing it through a very fine sieve or search. Glazes do not require such a high temperature to fuse them on the surface of the ware, as the body does to be burned.

6. The glaze for salt glazed ware is common salt, which is thrown in at the top of the kiln through a number of small apertures in the crown of it, and diffuses itself through all parts of the kiln, giving the ware the required glaze. The action that is supposed to take place, when the salt is thrown into the kiln, is owing to its decomposition. The chlorine of the salt combines with the hydrogen of the water, which is mechanically lodged in the salt, forms muriatic acid gas, which passes off, while the sodium combining with the oxygen of the water then unites with the silica in the ware, forming a silicate of soda which fuses on its surface. The salt is not thrown in until the kiln has been raised to its greatest necessary temperature.

Table of the Composition of Clays and Porcelain when free from Water.

	Silica.	Alumina.	Protoxide of iron.	Lime.	Magnesia.	Phosphate of lime and protoxide of iron.	Alkali and loss.	Specific gravity.
Cornish china clay .....	53·16	45·61	·31	·41	·51			
Cornish china clay .....	53·12	46·00	·31	·57	·51			
Sandy clay .....	70·29	27·47	1·33	·90	Trace	.....	.....	2·558
Pipe clay .....	61·39	36·61	1·54	·46	Trace			
Blue clay .....	53·52	43·89	1·20	1·39	Trace			
Red clay .....	52·11	36·19	8·17	1·56	2·04			
Fire clay .....	69·33	23·62	5·56	1·49	Trace			
Yellow clay .....	65·06	30·68	3·70	·56	Trace			
English china ware, No. 1 ...	39·88	21·48	.....	10·06	Trace	26·44	2·14	
... .. No. 2 ...	40·60	24·15	.....	14·22	·43	15·32	5·28	
... .. No. 2 ...	39·68	24·65	.....	14·18	·31	15·39	5·79	
Berlin ware .....	72·96	24·78	...	1·04	Trace	.....	1·22	2·419
Superior Chinese ware .....	71·04	22·46	...	3·82	Trace	.....	2·68	2·314
Inferior Chinese ware.....	68·96	29·24	...	1·60	Trace	.....	.....	2·314
Common English white ware	68·55	29·13	...	1·24	Trace	.....	.....	2·360

LXVIII. *On the Polarization of the Atmosphere.* By Sir DAVID BREWSTER, K.H., D.C.L., F.R.S., and V.P.R.S. Edin.\*

WHEN the light of the sun or of any self-luminous body has been transmitted through certain crystallized substances, or has been reflected from, or refracted by, bodies not metallic, it suffers a physical change, to which the name of *plane polarization* has been given. This physical change consists in decomposing common light into two equal portions of polarized light, one of which is polarized in a plane at right angles to that in which the other is polarized. In doubly refracting crystals, the two pencils are polarized in opposite or rectangular planes; and when common light is reflected from any body not metallic, whether it is solid, or fluid, or gaseous, a portion of the incident light enters the body; and of the portions thus reflected and refracted, precisely the same quantity is polarized,—the light polarized by refraction being polarized in a plane at right angles to that which is polarized by reflexion.

If the earth had no atmosphere the sky would appear absolutely black; and when the sun sets we should be left in utter darkness. The existence of twilight, however, the blue colour of the sky, and the refraction of the rays which emanate from the stars and planets, place it beyond a doubt that the pure air in which we live and breathe is capable of acting upon light like all other bodies, and consequently of producing that physical change which constitutes *polarization*. The polarization of the blue sky, or of the atmosphere, was therefore observed and studied by different philosophers, both in France and England; and it was speedily ascertained, in conformity with the laws of polarization, that the polarization was a *minimum* in the vicinity of the sun, where his light is reflected at angles approaching to  $90^\circ$ , or where the incident and reflected rays form an angle approaching to  $180^\circ$ ; that it was also a *minimum* in the region opposite the sun, where his light is reflected at an angle approaching to  $0^\circ$ , or at a perpendicular incidence; and that it was a *maximum* in those intermediate parts of the sky, which are distant about  $90^\circ$  from the sun, and where his light is reflected at an angle of about  $45^\circ$ , the polarizing angle for *air*.

Such was the first view which was naturally taken of the

\* This paper is reprinted, with the permission of Dr. Berghaus and Mr. A. K. Johnston, from the Seventh Part of their valuable Physical Atlas now in the course of publication. A map representing the four neutral points, and the system of lines of equal polarization, will be found in that work.

polarization of the atmosphere, and a considerable time elapsed before its leading elements were determined, and its more important phænomena observed and measured. It is to M. Arago, to whom this branch of science owes such deep obligations, that we are indebted for the discovery of the first and leading fact on which the law of atmospheric polarization depends. In examining the region of the sky opposite to the sun, he discovered a *neutral point*, or a point in which there is *no polarization* whatever. This neutral point he found to be  $25^{\circ}$  or  $30^{\circ}$  *above* the point diametrically opposite to the sun, or what we may call the *antisolar point*; and we shall distinguish this pole of no-polarization by the name of *M. Arago's neutral point*, or the *antisolar neutral point*. It is best seen after sunset.

In the year 1840, M. Babinet discovered a second neutral point, situated about the same distance above the sun as the neutral point of M. Arago is situated above the antisolar point. This point is most distinctly seen immediately after sunset, but is generally much fainter than the other, owing to the discoloration of the blue sky by the yellow light of the setting sun.

Our readers are no doubt aware, that when light is reflected from the surfaces of transparent bodies, a certain portion of it, and at a particular angle the whole of it, is *polarized* in the plane of reflexion, or *positively*\*; while precisely the same quantity of the transmitted light is *polarized* in a plane at right angles to the plane of reflexion or refraction, or *negatively*. Now, in the part of the sky between the neutral point of M. Arago and that of M. Babinet, the light is polarized *positively*; while in the parts of the sky between the first of these neutral points and the antisolar point, or between the second and the sun, it is polarized *negatively*. Hence it became obvious that the two neutral points must be produced by a compensation, in which light polarized *negatively* neutralized light polarized *positively*, and that the *negative* light was either produced by *reflexion* in a plane at right angles to that passing through the sun, the neutral point, and the observer, or by *refraction* in a plane passing through these three points, or by both these causes combined. But in whatever way the *negative* polarization was produced, it was manifest that the same cause ought to produce a *neutral point beneath the sun*. After many fruitless attempts to discover this neutral point—owing chiefly to the predominance of the sun's light

\* These terms are used for the purpose of abbreviation. An account of the laws of the polarization of light by reflexion and refraction, will be found in my papers in the Phil. Trans., 1815, p. 129, and 1830, pp. 69, 133.



at the part of the sky where it should be found—I at last observed, under a very favourable state of the atmosphere, that the polarization of the sky was *negative* in the space between the risen sun and the horizon. This observation placed it beyond a doubt that there must be a neutral point below the sun, where that *negative* polarization passed into *positive* polarization; and by concealing the sun from view, and admitting no light to the eye but what came from the probable place of the neutral point, I succeeded in discovering it. After communicating this discovery to M. Babinet\*, early in 1845, he made several ineffectual attempts to confirm it; and it was not till the 23rd of July 1846, when the state of the sky was peculiarly favourable for the observation, that he succeeded in obtaining a distinct view of it†.

Before proceeding to explain the map of the lines of equal polarization in the pure blue sky, I shall give a brief account of my observations on the three neutral points to which I have referred:—

### I. On M. Arago's Neutral Point.

In the normal state of the lines of equal polarization, namely, when the sun is in the horizon, this neutral point is about  $18\frac{1}{2}^{\circ}$  above the horizon or above the antisolar point; but when the sun is about  $11^{\circ}$  or  $12^{\circ}$  above the horizon, and the antisolar point of course as much below it, the neutral point is in the horizon, and consequently only  $11^{\circ}$  or  $12^{\circ}$  above the antisolar point. As the sun descends to the horizon, and the antisolar point rises, the distance of the neutral point from the latter gradually increases; and when the sun reaches the horizon, the neutral point is  $18\frac{1}{2}^{\circ}$  above it, and therefore  $18\frac{1}{2}^{\circ}$  distant from the antisolar point. After the sun has set, the distance of the neutral point from the antisolar point increases; that is, it rises faster than the sun descends, and its maximum distance when the twilight is very faint, is about  $25^{\circ}$ .

In the latitude of St. Andrews, M. Arago's neutral point is above the horizon all the day between the middle of November and the end of January. In the other months of the year it

\* *Comptes Rendus des Séances de l'Acad. des Sciences*, tom. xxii. p. 801–803, 1845, 17th Mars.

† *Comptes Rendus, &c.*, Juillet 27, 1846, tom. xxiii. p. 195; and Aout 3, 1846, tom. xxiii. p. 233. “M. Brewster,” says M. Babinet, “a sans doute été guidé dans sa recherche par des vues théoriques; autrement il me paraît peu probable qu’il eut fait, par observation seul de la polarization atmosphérique, la découverte remarquable de ce point neutre si difficile à reconnaître, et que, depuis lui j’avais plusieurs fois tenté inutilement de retrouver.” *Ibid.* p. 235.

never rises till the sun is within  $11^\circ$  or  $12^\circ$  of the horizon, and never sets till the sun is  $11^\circ$  or  $12^\circ$  above the horizon.

## II. On a secondary Neutral Point accompanying M. Arago's Neutral Point.

I observed the first traces of this remarkable phænomenon on the 8th of June 1841, at  $5^h 50'$ , when the positive polarization was strongest close to the horizon, whether land or sea, and to about  $1\frac{1}{2}^\circ$  above it. Hence, when M. Arago's neutral point rose, it *did not appear first in the horizon*, but about  $1\frac{1}{2}^\circ$  above it, the compensation being effected where the positive polarization was weaker than in the horizon. When this took place, we had the singular phænomenon of a *neutral point with positive polarization on each side of it*. When this phænomenon was more fully developed, under a favourable state of the horizon, the positive polarization was overcome by the advancing negative polarization. The negative polarization was then immediately below the ascending neutral point; but at a certain distance, a few degrees below the neutral point, the negative polarization was compensated by the excess of positive polarization close to the horizon, and the beautiful phænomenon was seen of two neutral points separated by bands of negative polarization! This phænomenon was best seen on the sea horizon, which was marked by an obscure band a few degrees high, that indicated the existence of a distant haze. On the 21st of April 1842, I observed the secondary neutral point under favourable circumstances. At  $6^h 22'$  P.M., when the *primary* neutral point was  $15^\circ$  high, the *secondary* one was  $2^\circ 50'$  high. At  $7^h$  positive bands were still seen above the sea line, and were strongest upon the obscure band above the visible sea line.

## III. On M. Babinet's Neutral Point.

This neutral point is situated about  $18^\circ 30'$  above the sun, when he is rising or setting in a very clear sky. It is not so easily seen as that of M. Arago, and was therefore longer in being discovered. It is above the horizon during the greater part of the year in great latitudes, and being above the sun, it is of course always visible when the sun is above the horizon in a clear sky. When the sun is in the zenith, this neutral point coincides with the sun's centre. As the sun's altitude diminishes, it separates from the sun's centre, its distance gradually increasing till it becomes  $18^\circ 30'$ , when the sun's altitude is nothing, or at sunrise and sunset.

The neutral point of M. Babinet *must*, like that of M. Arago, be accompanied, in certain states of the horizontal

sky, with a secondary neutral point; but I have never had an opportunity of observing M. Babinet's neutral point when it either rose above or set beneath the horizon, which, though not essential, is the most favourable for observing a secondary neutral point.

#### IV. On the Neutral Point below the Sun.

This neutral point is, as we have previously noticed, much more difficult to be seen than that of M. Babinet. In November, December and January, it cannot be seen in our latitudes, unless when, early in November and late in January, a higher degree of polarization in the sky brings it above the horizon at noon.

As theory indicated the existence of this neutral point, I long sought for it in vain; but when I was assured of its existence by the discovery of negative polarization, which often extended from the sun to the horizon even when the sun's altitude was  $30^\circ$ , I took such precautions for excluding all unnecessary light from the eye that I at last observed it near the horizon, with a small portion of positively polarized light beneath it. I afterwards observed it repeatedly when the sun had higher altitudes, and was able to measure its varying distance from that luminary. On the 18th of February 1842, at noon, when the sun's altitude was about  $22^\circ$ , I observed this neutral point in the most distinct manner, the polarized bands being negative below the sun, and positive near the horizon. Its distance from the sun, therefore, was about  $15^\circ$  or  $16^\circ$ . I afterwards obtained the following measures of its distance from the sun:—

			Distance of neutral point from the sun.
1842, February 21,	<sup>h</sup> 12 <sup>'</sup> 39	<sup>°</sup> 15 <sup>'</sup> 0	
... April	3, 11 45	13 0	
... ..	6, 11 6	12 0	
... ..	8, 2 7	16 0 estimated.	

On the 20th of April, in a very fine day, the wind being west and the barometer 30.02, I obtained the following measures:—

		Distance from sun.
April 20,	<sup>h</sup> 12 <sup>'</sup> 10	<sup>°</sup> 11 <sup>'</sup> 20
	12 37	10 40
	2 21	12 0
	3 45	12 35

The maximum polarization of the sky at the time of these observations was equal to a rotation of  $25\frac{1}{2}^\circ$ , about  $4\frac{1}{2}^\circ$  below the greatest maximum.

On the 26th of April 1842, when the barometer was at 30·00, and not a cloud in the sky from morning till night\*, I obtained the following measures:—

		Distance from sun.	
April 26,	<sup>h</sup> 11 <sup>i</sup> 1	<sup>°</sup> 12 <sup>'</sup> 15	
	11 46	12 30	
	3 30	14 35	
	3 35	15 5	
	4 10	17 45	

At 10<sup>h</sup> 53', the maximum polarization of the sky, or the rotation, was  $28\frac{3}{4}^{\circ}$ , and at 11<sup>h</sup> 46', and 3<sup>h</sup> 42', it was  $28\frac{1}{2}^{\circ}$ .

On the 27th of April I observed a remarkable series of phænomena relative to this neutral point. The sky was singularly fine—the barometer at 30·04, and at 10<sup>h</sup> 41' the maximum polarization of the sky  $29\frac{1}{2}^{\circ}$ , the greatest that I have observed. At 10<sup>h</sup> 45', the distance of the neutral point from the sun was  $12^{\circ} 3'$ , and consequently about  $33\frac{1}{2}^{\circ}$  above the horizon. At 12<sup>h</sup> 12', a fog came rapidly from the sea. The neutral point below the sun was driven beneath the horizon, and Babinet's neutral point rose almost to the zenith. At 1<sup>h</sup> 20' the fog diminished. The neutral point below the sun reappeared near the horizon, oscillating up and down, through a space of  $5^{\circ}$  or  $6^{\circ}$ , as the fog became alternately denser or rarer!

When the sky is clear, the neutral point below the sun approaches to the sun as his altitude increases, and finally coincides with the sun's centre when he is in the zenith. Hence it follows, that when the sun is in the zenith, the two neutral points in his vicinity meet in the sun, and the system of polarization lines becomes uniaxal.

Were the sky sufficiently clear, we should doubtless find a secondary neutral point accompanying the primary one below the sun; but in our climate there is little chance of this phænomenon being distinctly observed.

In his observations on the antisolar *neutral point*, M. Arago observed that it sometimes deviated from the plane passing through the antisolar point and the eye of the observer; and he justly ascribed this deviation to the influence of luminous clouds situated out of this plane. The same phænomenon takes place in reference to the other neutral points, though the deviation is in these cases less distinctly seen, from the interference of the sun's light. But it is not merely the position of the neutral point that is influenced by the intrusion of

\* The lines in the spectrum were ill-defined, from unequal refraction in the air.



light different from that of the sky ; the degree of polarization is always affected whenever we measure it in parts of the sky which have luminous clouds or illuminated terrestrial objects in their vicinity, or any luminosity in the field of view of the polarimeter. If the neutral point happens to be above or below any such object, its *distance* from the antisolar point or from the sun is increased or diminished\*.

#### V. On the Maximum Polarization of the Sky.

After having ascertained the position of the neutral points, or *poles of no-polarization* as we may call them, the next most important element to be determined is the *maximum polarization of the atmosphere*.

When a ray of common light is reflected from any transparent body, at an angle whose tangent is equal to the index of refraction, it is completely polarized ; or when a ray of light, completely polarized in a plane inclined  $45^\circ$  to the plane of reflexion, is reflected from any such body, its plane of polarization is brought into the plane of reflexion ; that is, *its plane is turned round  $45^\circ$* . Hence complete polarization is measured by a rotation of  $45^\circ$ . When the polarized ray is reflected at angles *above* or *below* the angle of maximum polarization, its plane is less turned round, and its rotation is more or less than  $45^\circ$ , according as the angle of reflexion is more or less distant from the angle of maximum or complete polarization†.

Different degrees of rotation below  $45^\circ$  may also be produced by the refraction of the polarized ray at one or more surfaces of glass‡, the rotation increasing with the angle of incidence. Hence we may measure the *degree of polarization* wherever it exists, by observing at what angle of incidence it is compensated or neutralized, by reflexion from a transparent surface, or by refraction at one or more such surfaces. I have found the last method the most convenient, and have therefore constructed a *polarimeter* which measures the polarization of the sky, by observing with it either the varying angle at which

\* On the 16th of May 1842, barometer 30.3, the sun was faintly seen through a thick haze. At 8<sup>h</sup> 49' A.M. the polarization was *positive* all the way from the sun to the horizon, so that the neutral point below the sun was below the horizon. Immediately afterwards the sun was quite hid—a great glare supervened, and a *quaquaversus* polarization was observed, in which the polariscope gave no coloured bands.

On the 17th of May, at 6<sup>h</sup> 30', the sun's disc was quite white through a thick haze, and there was *no neutral point either above or opposite the sun*, the polarization being everywhere *positive*. When the haze is thicker on one side of the plane passing through the sun's spectrum, the neutral point deviates from that plane.

† See Phil. Trans., 1830, p. 69.

‡ Ibid. p. 133.

it is compensated or neutralized by a fixed number of thin glass plates, or the varying number of refracting surfaces, by which the same effect may be produced at a fixed angle, capable also of being changed\*.

With a *polarimeter* thus constructed, I have determined that the maximum polarization of a clear blue sky is equivalent to a rotation in the plane of a polarized ray of  $30^\circ$ ; and that this maximum takes place at a distance of from  $88^\circ$  to  $92^\circ$  from the sun, and in the plane passing through the sun and the zenith. This maximum is of course dependent on the state of the atmosphere, both with respect to its magnitude and position; but we shall assume  $30^\circ$  as its amount, and  $90^\circ$  from the sun as its position in a normal state of the atmosphere, and when the sun is in the horizon.

# VI. On the Form of the Lines of equal Polarization in the Atmosphere.

It is obvious, from the phænomena already described, that the polarization of the atmosphere, produced by the reflexion of the sun's light from the matter which composes the atmosphere, in planes passing through the sun, the point of reflexion, and the eye of the observer, would have been equal in circles of which the sun and the antisolar point are the centre, had there been no disturbing causes, or had the atmosphere been a perfectly transparent medium. In this case the polarization would have been complete, or  $45^\circ$ ; and this maximum would have occurred at a distance from the sun, the half of which was the polarizing angle of the medium. There is obviously, however, a cause depending on the zenith distance of the polarizing point of the sky, which acts in opposition to the polarization produced by reflexion, and compensates it at the neutral point already described. When the sun, therefore, is in the horizon, these two actions are rectangular, as in biaxal crystals; and we must therefore determine the form of the lines of equal polarization when the sun is in the horizon, and when the atmosphere is perfectly pure. When viewed, consequently, in their general aspect, the phænomena of atmospheric polarization may be represented by the formula

$$R = 30^\circ (\sin D \sin D'),$$

where  $R$  = rotation, or degree of polarization, and  $D$  and  $D'$  = the distances of the point whose polarization is required from the *two* neutral points.

This formula would make the lines of equal polarization

\* See the Transactions of the Royal Irish Academy, vol. xix. part 2.

*Lemniscates*, as in biaxal crystals, and consequently the polarization in the horizon greater than in the zenith, which is contrary to observation. I have therefore added a correction, depending on the zenith distance and azimuth, which makes the formula coincide better with observation, namely,

$$R = 33\frac{1}{2}^{\circ}(\sin D \sin D') - 6^{\circ} 34' (\sin Z \sin A);$$

$Z$  being the zenith distance, and  $A$  the angle of azimuth.

Assuming, therefore, that the distance of the neutral points from the sun and from the antisolar point is  $18^{\circ} 30'$ , when the sun is in the horizon, and that the atmosphere is perfectly pure and uniformly transparent, the lines of equal polarization will have the forms and the degrees of polarization represented by the formula. The direction of the polarization follows the same law as in biaxal crystals, the lines without bands or colour corresponding with the black hyperbolic branches in the polarized rings produced by these crystals, being distinctly seen with the polariscope.

#### VII. *On the Construction of the Map of the Lines of equal Polarization.*

Had the map been on a greater or a less scale than it is, it might have been desirable to appropriate a single curve to every single degree, or to every two degrees of rotation or polarization. On the present scale, the curves would have been too numerous and close had there been one to each degree; and with only one to each two degrees, they would have been too distant, in so far as that the form of the curves round the neutral points would not have been sufficiently seen. I therefore adopted such a number of curves, viz.  $18\frac{1}{2}$ , as enabled me to get the curves, No. 2, continuous round each neutral point. Hence the formula became

$$N = 20.5 (\sin D \sin D') - 3.9 \sin Z \sin A,$$

or in the plane passing through the sun and the zenith, in which  $Z$  and  $A$  become zero,

$$N = 25.5 (\sin D \sin D').$$

In the zenith itself we have  $N = 18.45$ , and at  $P, P'$  we have  $N = 0$ .

The curves thus obtained do not represent values of  $N$  in degrees of rotation, but in numbers, each of which is equal to  $1^{\circ}.626$ . Hence  $R = N 1^{\circ}.626$ , and the distance between each curve is  $1^{\circ}.626$ . The following table contains the rotations or degrees of polarization, indicated by each of the curves numbered from  $\frac{1}{2}$  to  $18.45$  in the map:—

Values of N.	Corresponding degrees of rotation or polarization, or values of R.
$\frac{1}{2}$	0.813
1	1.626
$1\frac{1}{2}$	2.439
2	3.252
$2\frac{1}{2}$	4.065
3	4.878
4	6.504
5	8.130
6	9.756
7	11.382
8	13.008
9	14.634
10	16.260
11	17.886
12	19.511
13	21.137
14	22.764
15	24.396
16	26.016
17	27.642
18	29.268
18.45	30.000

Hence the maximum polarization of the atmosphere, as measured by a rotation of  $30^\circ$ , is equal to that produced by reflexion from a plate of glass at an angle of  $65\frac{1}{2}^\circ$ , and with a refractive index of 1.4826, or to that produced by a surface of diamond at an angle of  $75\frac{1}{2}^\circ$ . The number of refractions at a given angle, or the angle, with a given number of plates of glass, at which a rotation of  $30^\circ$  is produced, will be found from the formula in my paper on the Compensations of Polarized Light\*.

As the sun *rises* above the horizon, the lines of equal polarization change their form, and the degree of polarization varies at points of the sky whose distance from the sun is invariable. The neutral points *above* and *below* the sun approach his disc till he reaches the meridian, when the distance of each from the sun is a *minimum*; they then separate again, and attain their maximum distance, when he reaches the horizon. In countries where the sun passes across the zenith, these two neutral points coincide with the sun, when he reaches the zenith, and again separate.

\* Transactions of the Royal Irish Academy, vol. xix. part 2. p. 13.



As the sun descends beneath the horizon, the neutral point of M. Arago separates from the antisolar point, and when this point is first seen in the morning before sunrise, its distance from the antisolar point is a maximum; it gradually approaches that point till the sun rises, and also till the neutral point itself reaches the horizon, when its distance from the antisolar point is a *minimum*.

When the altitude of the sun is  $45^\circ$ , the distance  $x$  of the neutral point above the sun is about  $13^\circ 5'$ , and the distance  $x'$  of the neutral point below the sun  $6^\circ 42'$ ; at other altitudes we have

$$\begin{aligned} x &= \Delta \cos A, \\ \text{and} \quad x' &= \Delta \cos A, \\ &\quad \tan Z, \end{aligned}$$

$\Delta$  being  $18\frac{1}{2}^\circ$ ,  $A$  the sun's altitude, and  $Z$  the zenith distance of  $P'$ , the neutral point below the sun.

An interesting paper, entitled *Delle Leggi della Polarizzazione della Luce Solare nella Atmosfera Serena*, communicato con lettera al David Brewster, LL.D., F.R.S., Lond. et Edin., membro delle Principali Accademie di Europa, del Prof. A. B. Francesco Zantedeschi, will be found in the *Raccolta Fisico-chimica Italiana*, tom. i. fascic. 10. 1846. The details in this paper are chiefly historical. The results obtained by M. Zantedeschi himself, which are of a general nature, differ in several respects from mine; but whether this difference arises from a difference in the methods of observation, or from the different states of the atmosphere under which the observations were made, I am not able to determine.

In a Memoir on the Polarization of the Atmosphere, which, I trust, will soon be published in the Transactions of the Royal Irish Academy, I shall give a full account of my observations, and enter more deeply into the subject than would have been proper in the preceding popular explanation of a Map of the Lines of Equal Polarization.

LXIX. *On the Hydrates of Nitric Acid.* By Mr. ARTHUR SMITH, Assistant in the Laboratory of University College, London\*.

SOME doubt still hanging over the composition of the hydrates of nitric acid, especially of the first hydrate, I was induced to try some experiments with a view of diminishing this uncertainty. For this purpose a quantity of the red fuming acid was procured, which I examined before com-

\* Communicated by the Chemical Society; having been read June 7, 1847.

mencing my experiments very carefully for chlorine, and found to be perfectly free from that impurity, and to have a specific gravity of 1.500.

Fourteen ounces of acid of the above-named strength were mixed with 7 ounces of commercial oil of vitriol, and distilled in a sand-bath over a gas flame; the first 2 ounces that came over were rejected, and the receiver changed directly the red fumes of nitrous acid were observed to fill the interior of the retort. The acid collected was almost as dark in colour as the acid before distillation. Its specific gravity was 1.522, and it turned out to be perfectly free from the smallest trace of sulphuric acid.

I also examined the first two ounces of acid that came over very carefully for chlorine, and found it to contain scarcely a trace, nitrate of silver producing only a slight opalescence, and that which came over afterwards, being the portion that I selected for my experiments, contained none at all. This last acid, when diluted with water, gave off nitric oxide gas with a burst of effervescence, which was the principal reason why it could not be employed to ascertain the exact amount of real acid by saturation in its present dark-coloured condition.

The apparatus employed in decolorizing the nitric acid consisted of a capacious retort, capable of holding about a pint, to the beak of which was attached a large tubulated receiver, which was kept surrounded with water, to condense any little acid that might come over during the process; to the tubulure of this receiver was adapted a glass tube, bent at right angles, fitting tightly with a cork, the other extremity being in connexion with a large gas-holder, which was kept constantly filled with water, to be used as an aspirator. To the tubulure of the retort was also fitted a long glass tube bent at right angles, the one end of which terminated within an inch of its bottom, whilst the other was in connexion with a couple of tubes, each 2 feet 11 inches long, arranged side by side, and connected by means of a tube of a smaller diameter bent like the letter U.

These long tubes, through which the air was to be aspired, were filled, the one with dried chloride of calcium, and the other with pumice-stone moistened with oil of vitriol, and by these means the absence of all moisture from the air was ensured.

In decolorizing the acid a quantity amounting to 6 or 7 ounces was introduced into the retort, and after having ascertained that the whole apparatus was perfectly tight, heat was applied to the bottom of a small sand-bath in which the retort was immersed, and the temperature kept up carefully to 170° F. Then, by removing the plug at the bottom of the

gas-holder, and turning the stop-cock at the top, which was in connexion with the apparatus, a constant flow of perfectly dry air was caused to bubble through the nitric acid in the retort, the level of which was kept 2 or 3 inches above the orifice of the tube in the interior, the only passage for the air being through the long desiccating tubes. Aspiration kept up for two or three hours was found to be generally sufficient to decolorize completely 6 or 7 ounces of nitric acid.

The acid before decolorization had a specific gravity of 1.522, and after the process fell to 1.503. Fifty grs. of the colourless nitric acid were accurately weighed out in a stoppered specific gravity bottle, to which was cautiously added, whilst in the bottle, with a view to prevent any loss from splashing, a known weight of perfectly pure carbonate of soda, recently ignited in a porcelain crucible, until the solution was perfectly neutral to test-paper. The absence of any sulphate or chloride in the carbonate had been previously ascertained.

I. Carbonate of soda required 40.23 grs.

II. Carbonate of soda required 40.23 grs.

The quantity of carbonate of soda that 50 grs. of acid required for saturation, then, was 40.23 grs., which corresponds to 40.78 grs. of nitric acid, or 81.56 per cent.

An acid containing  $1\frac{1}{2}$  equiv. of water would contain in 100 parts—

Real nitric acid	. . . . .	80
Water	. . . . .	20
		<hr/> 100

A portion of the prepared acid, amounting to about 5 ounces, was introduced into a small retort, through the tubulure of which was fitted tightly, by means of a stopping of moist clay, a delicate thermometer, which was kept immersed in the liquid. The acid began to boil at  $190^{\circ}$ , and before the distillation had come to an end it had risen to  $250^{\circ}$ . The acid coming over between  $190^{\circ}$  and  $200^{\circ}$  was collected apart to be examined by saturation.

50 grs. of the acid which remained in the retort boiling at  $250^{\circ}$  were then examined, and found to require 31.20 grs. of carbonate of soda in the first experiment, and 31.07 in the second, for saturation; the mean of the two experiments would correspond to 63.11 per cent. of nitric acid.

50 grs. of the most volatile portion, namely, that which came over between the temperatures of  $190^{\circ}$  and  $200^{\circ}$ , were then weighed out exactly; this quantity was found to require no less than 41.92 grs. in the first experiment, and 41.91 in the second, corresponding to 84.96 per cent. nitric acid; but then it must be remembered that this acid had a very dark red colour.

A quantity of this red acid was introduced into the decolorizing apparatus, and a constant rapid stream of dry air made to bubble through it for two hours; at the expiration of that time it was found to be perfectly limpid, and colourless as water, and to have a specific gravity of 1.516 at 60°.

50 grs. of the last acid were weighed out and neutralized with pure carbonate of soda as before. The numbers below will show the amount required for saturation:—

Exp.	Carb. of soda.	Mean.
1. . .	41.77	41.70, corresponding to 42.27, or 84.54 per cent. nitric acid.
2. . .	41.69	
3. . .	41.64	

This acid began to boil at about 184°, the greater part distilling over between the temperatures of 184° and 188°; it afterwards rose when near the end to 200°.

The first portion that came over was collected apart, introduced into the decolorizing apparatus, and dry air again drawn through it until it was quite colourless. This was found to be necessary after each distillation, on account of the decomposition that it suffered upon boiling, which rendered it as dark in colour as the original acid. 50 grs. of the colourless acid, of the specific gravity of 1.517 at 60°, were weighed out, and carbonate of soda very carefully added until neutral to test-paper. The increase in the specific gravity this time only amounted to .001.

Exp.	Carb. of soda.	Acid.	Mean.
1. . .	41.79	= 42.36	42.37.
2. . .	41.81	= 42.38	

Hence in 100 parts—

				According to theory with 1 eq. water.			
Real acid	.	.	84.74	Real acid	.	.	85.71
Water	.	.	15.26	Water	.	.	14.28
			<u>100.00</u>				<u>99.99</u>

This would give, when compared with the theoretical composition of nitric acid with 1 equiv. of water, a deficiency of .97 in the acid, and an excess of .98 in the water.

This hydrate, when pure, was a perfectly limpid and colourless liquid, like so much water; it boiled at 184°, and had a specific gravity of 1.517 at 60°. It was found not to have the slightest action on tin or iron even when boiled. A portion of this acid placed in a freezing mixture composed of ice and salt suffered no change.

These experiments leave little doubt concerning the composition of the first hydrate of nitric acid, namely, that it is the true mono-hydrate, consisting of 1 equiv. of nitric acid and 1 of water, HO, NO<sub>5</sub>.

*Deuto-Hydrate.*—In preparing this hydrate, I set out by obtaining a quantity of colourless strong nitric acid, the exact



amount of real acid in which was ascertained by saturation to be 79.79 per cent. To reduce this acid to the proper strength, so as to contain exactly 2 equivs. of water, it was found by calculation that it would require 63.86 grs. of water to every 1000 grs. of acid.

The proper proportions of acid and water were weighed out carefully in a stoppered specific gravity bottle, and the two mixed. This mixture was cooled down to 60° and found to have the sp. gr. 1.486.

50 grs. of this hydrate were weighed out and saturated in the usual way with recently-ignited carbonate of soda. The quantities of carbonate of soda required were as follows:—

Exp. Carb. of soda. Mean.

1. . . 37.45 }  
2. . . 37.53 } 37.49, or 74.98 per cent. acid.

An acid containing 2 equivs. of water will contain 75 per cent. real acid.

A portion of this acid was introduced into a small retort and distilled. It began to boil, as nearly as could be judged, at about 200°, it being difficult to come at the exact temperature on account of the very rapid rise of the thermometer, which continued to take place until it had gained the temperature of 218°; it afterwards rose when near the end to 250°.

It appears, then, from these experiments, that no such thing as a deuto-hydrate exists, but that when a mixture is made in the proportions to form such a hydrate and subjected to distillation, it divides spontaneously into the first and another, at the same time suffering considerable decomposition; and the acid which is found remaining in the retort has the exact boiling-point of the tetra-hydrate, namely, 250°; and moreover, the first portion that came over had the exact density of the first.

A portion of this acid placed in a freezing mixture of ice and salt, suffered not the least solidification.

*Tetra-Hydrate.*—This hydrate was prepared in the same way as the first, namely, by preparing a quantity of colourless acid, ascertaining its saturating power, and mixing it with the proper quantity of distilled water, ascertained by calculation. It was then tried afterwards by saturation to see if it was correct; the numbers below will show the difference:—

Exp. Carb. of soda. Acid. Mean.

1. . . 29.96 = 30.37 }  
2. . . 29.87 = 30.27 } 30.32, or 60.64 per cent. real acid.

Real acid	. . .	60.64
Water	. . .	39.36
		<u>100.00</u>

According to theory with 4 eqs. water.

Real acid	. . .	60
Water	. . .	40
		<u>100</u>

The acid had a density of 1.424 at 60°; it began to boil at 250°, and distilled over perfectly colourless and unchanged; towards the end, when slight decomposition commenced, the temperature rose to 260°.

Five or six ounces of very weak acid, of the density of 1.180, were introduced into a retort and kept heated just below its boiling-point for two or three hours; the heat was increased from time to time so as to make it boil briskly, and a thermometer introduced through the tubulure; when that which remained in the retort boiled uniformly at 250°, the heat was withdrawn and it was allowed to cool.

When the specific gravity of this acid was taken, it was found to be close upon that of the tetra-hydrate, but not exactly; probably if I had operated upon a large quantity, and carried it on for a longer time, it would have been more so; as found, its density was 1.412 instead of 1.424, which would make a difference of rather less than 1½ per cent. deficiency in the acid.

This is, I have no doubt, the proper hydrate of nitric acid,  $\text{HO}, \text{NO}_5 + 3\text{HO}$ , as it is generally considered; and as Dr. Dalton correctly observed, acids which are either stronger or weaker than this acid, are brought to this strength by continued ebullition, the former losing acid and the latter water.

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LXX. *On the Products of the Decomposition of Cuminate of Ammonia by Heat.* By Mr. FREDERICK FIELD\*.

THE peculiar mode of decomposition which the ammonia salts of inorganic acids exhibit when exposed to the action of heat, occurs likewise in the ammonia compounds of organic acids, although the results in the latter instances are usually of a more complicated nature. In most of these cases a formation of water takes place, the hydrogen of which is derived from the volatile alkali, while the acid furnishes the oxygen, the residue of which combines in a more intimate manner with the nitrogen of the ammonia. In decompositions, however, of *inorganic* compounds this reduction seems to be carried at once as far as it can go, the whole of the hydrogen contained in the ammonia being converted into water; while in *organic* salts this hydrogen is eliminated only by degrees, an intermediate body being produced between the original ammonia salt and the final product of the decomposition. Thus we find that nitrite and nitrate of ammonia, when exposed to heat, are at once converted into water, and respectively into nitrogen and nitrous oxide. Oxalate of ammonia,

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on the other hand, if submitted to a gentle heat, loses only two equivalents of water, the residue of both base and acid combining to form oxamide, and only by a strong and brisk application of heat Dœbereiner converted it into cyanogen, the rest of the hydrogen being eliminated in the form of water.

The dry distillation of oxalate of ammonia thus affords the prototypes of two series of compounds, which may arise from ammoniacal salts by the elimination of two or four equivalents of water respectively. There are few cases, however, in which the decomposition of ammoniacal salts have been carefully studied, and the instances in which we are acquainted with the representative of the two types are exceedingly scarce. We are indeed intimate with a very great number of amidogen compounds analogous to oxamide (fumaramide, salicylamide, succinamide, anisylamide, &c.), but only few of these have been obtained from ammoniacal salts by the action of heat. The greatest number of these bodies were produced by the change most compound æthers suffer under the influence of ammonia, a beautiful mode of decomposition pointed out first by Professor Liebig in the transformation of oxalate of ethyl into oxamide, or by the action of gaseous ammonia on other substances related in some manner with the acid: thus was chloride of benzoyle converted into benzamide by Wöhler and Liebig, and lately lactide into lactimide by Pelouze.

As yet, however, the members of the second class, those compounds standing to other acids in the same relation as cyanogen to oxalic acid, are very rare. From a beautiful experiment of Pelouze, we know that the vapour of formiate of ammonia, when passed through a red-hot tube, is converted into water and hydrocyanic acid. In their investigation on the radical of benzoic acid, Wöhler and Liebig obtained a peculiar oil by the action of heat on benzamide, which at that time they did not study more closely. The same body was at a later period obtained in the dry distillation of benzoate of ammonia, and fully examined by Fehling, who found that this interesting substance, to which he gave the name *benzonitrile*, has the composition  $C_{14}H_5N$ , and is produced from benzoate of oxide of ammonium, exactly in the same manner as cyanogen and prussic acid are formed respectively from oxalate or formiate of ammonia. These facts did not long remain isolated. Schlieper, in an excellent examination he has lately published on the products of oxidation of gelatine by chromic acid, discovered that in these reactions, among other products, the body  $C_{10}H_9N$  is formed, valerionitrile or valerianate of ammonia—4 equivs. of water.

The members of this class acquire every day a greater degree of importance. A remarkable paper, read before the

Chemical Society a short time since by Dr. Kolbe and Mr. Frankland, has indeed opened a most interesting connexion between these bodies and another class of substances, which hitherto have been obtained by very different processes. The conversion of cyanide of ethyl into metacetic acid by means of alkalies and acids, seems to indicate that cyanide of ethyl is nothing else than metacetonitrile. This experiment is likely to be of great importance, for it is exceedingly probable that the whole class of substances alluded to must be considered as a class of cyanogen compounds. It is evident that similar considerations may be applied to cyanide of methyl and cyanide of amyl, lately described by Balard; and the conversion of these cyanides respectively into acetic and caproic acids, which we have a right to anticipate on treating them with alkalies or acids, will prove that these compounds are the nitriles of acetic and caproic acids—acetonitrile and capronitrile—which as yet have not been obtained by the action of heat on the ammoniacal salts of these acids.

The following experiments on the action of heat on cuminate of ammonia have been made with the hope of contributing to the history of the nitryles, or organic cyanides, as they perhaps should be more correctly designated.

The cuminic acid employed in my experiments was prepared by the action of solid hydrate of potash on oil of cumin, and the product perfectly freed from the least traces of cymol which it might possibly contain by precipitating the potash salt by hydrochloric acid, dissolving the precipitated cuminic acid in ammonia, reprecipitating by hydrochloric acid, and crystallizing from water. The acid was then dissolved in strong ammonia, and the solution subjected to heat. The first portions which passed over, although consisting chiefly of water and ammonia, together with cuminate of ammonia, which is always carried over with the steam, presented more or less an opalescent appearance, indicative of traces of the oil. On evaporating the solution in the retort to dryness, a portion of the salt is decomposed, ammonia is evolved, and cuminic acid condenses in beautiful plates upon the sides and neck of the retort, separation going on even on raising the temperature; but simultaneously another decomposition takes place, water is eliminated, in consequence of which there are produced a peculiar white crystalline body, difficultly soluble in water, and subsequently a colourless oil of a most fragrant odour; although the operation may seem very simple, experience alone teaches the proper regulation of temperature necessary to obtain these two bodies.

*Cuminamide.*—Observing in my first experiments evolution of ammonia and sublimation of cuminic acid on heating cu-



minate of ammonia, I thought that by heating it under pressure, the ammonia then not being able to escape, the desired change might be effected. Accordingly a portion of the salt was placed in a strong glass tube, and after sealing the other end, gradually heated in an oil-bath to nearly the boiling-point of the oil, and allowed to cool. On cooling the mass appeared to have been completely fused, but perfectly solid and of a highly-crystallized texture. On examination it was found to be insoluble in cold water and ammonia, but very soluble in hot water, from which it solidified into a crystalline mass as the temperature cooled; this alone sufficiently indicated that a complete change had been effected, the cuminate of ammonia being readily soluble in cold water. In order to ascertain the nature of the change it was dissolved in hot water, and weak ammonia added to dissolve any cuminic acid that might be mixed with it, and crystallized; the crystals were separated by filtration, and once more dissolved in a hot weak solution of ammonia, from which they separated on cooling in brilliant white crystalline plates, similar in appearance to benzamide. These were dried at  $212^{\circ}$  in a water-bath, and analysed in the usual manner.

I. 0.174 grm. of substance burnt with oxide of copper yielded 0.470 of carbonic acid and 0.128 of water.

II. 0.248 grm. yielded 0.670 of carbonic acid and 0.181 of water.

III. To estimate the nitrogen, 0.287 grm. ignited with soda-lime yielded 0.390 of ammonio-chloride of platinum\*.

From these analytic results the following per-centages are obtained:—

	I.	II.	III.
Carbon . .	73.66	73.67	
Hydrogen .	8.17	8.10	
Nitrogen .			8.50

leading to the formula  $C_{20}H_{13}NO_2$ , as may be seen from the following comparison of the theoretical and experimental numbers:—

			Theory.	Mean of exp.
20 equivs. of Carbon . .	120		73.68	73.66
13 ... Hydrogen .	13		7.99	8.13
1 ... Nitrogen .	14		8.52	8.50
2 ... Oxygen . .	16		9.81	9.71
	163		100.00	100.00

This body therefore is cuminamide,  $NH_2 C_{20}H_{11}O_2$ , having

\* In this operation a large quantity of an oily body is produced, which floats on the surface of the hydrochloric acid. It is evidently *cumol*.

precisely the same relation to cuminate of ammonia as oxamide to oxalate of ammonia.

In preparing large quantities of this substance the employment of close tubes would be very inconvenient, and I soon found that it could be obtained in a retort by the continued application of a heat sufficient to keep the salt in a state of semi-fluidity. The analyses II. and III. were made with the product obtained in this manner.

Cuminamide crystallizes like benzamide, in two forms, according to the state of the solution; if crystallized immediately, or from a strong solution, it separates in the form of crystalline tables of great brilliancy, but if the solution be dilute, it crystallizes after the lapse of some hours in long opaque needles, both forms having exactly the same composition. It is soluble in hot and cold alcohol in any proportion, as also in æther. This new amide differs from most others that have been described in remaining intact on the addition of strong solution of potash, or mineral acids; from the former it crystallizes in large plates after some days. Long boiling with alkalies or acids is scarcely sufficient to produce the characteristic conversion of amides either into ammoniacal salts or combinations of the base with the acid and evolution of ammonia.

*Cumonitrile.*—On heating cuminate of ammonia until it is perfectly fused, and keeping the fused mass in a state of brisk ebullition, large globules of a light yellowish oil pass over with water, evidently derived from the decomposition of the salt; when the globules began to diminish the process was stopped, the oil was separated from the water in the receiver by means of a pipette, the remaining distillate added to the mass in the retort, and the process again repeated as before; in this manner, after some half-dozen distillations, nearly an ounce of oil was obtained; it was well-washed with ammonia to remove cuminic acid, which seemed to be soluble in the oil, then treated with hydrochloric acid to remove ammonia, thoroughly washed with water, and digested with chloride of calcium; after standing some days to separate chloride of calcium, it was distilled and carefully rectified, the first portions being rejected, as possibly containing traces of water; the middle portion was reserved and placed in a retort with a coil of platinum; the liquid entered into ebullition at  $239^{\circ}$  C., at which point it remained stationary while at least a quarter of an ounce was passing over. This portion was employed in the following analyses:—

I. 0.212 gm. burnt with oxide of copper yielded 0.644 of carbonic acid and 0.145 of water.

II. 0.225 grm. yielded 0.6835 of carbonic acid and 0.161 of water.

III. 0.244 grm. ignited with soda-lime yielded 0.364 of ammonio-chloride of platinum\*.

From these analytical results the following per-centages are obtained :—

	I.	II.	III.
Carbon . .	82.82	82.84	
Hydrogen .	7.59	7.96	
Nitrogen .			9.34

leading to the formula  $C_{20}H_{11}N$ , as may be seen from the following comparison of the theoretical and experimental numbers :—

		Theory.	Mean of exp.
20 Carbon . .	120	82.76	82.83
11 Hydrogen .	11	7.58	7.77
1 Nitrogen .	14	9.66	9.34
	<u>145</u>		

This body is therefore cumonitrile,  $C_{20}H_{11}N$ , standing in the same relation to cuminate of ammonia as cyanogen does to oxalate of ammonia.

Cumonitrile is a perfectly clear and colourless liquid, possessing a high refractive power; it has a most powerful and agreeable odour and a burning taste; it is somewhat soluble in water, causing turbidity in that liquid; it is soluble in all proportions of alcohol and æther; it is lighter than water, having a specific gravity 0.765 at 14° C. (57° Fahr.). The boiling-point, when in contact with metal, is constant at 239° C. (462.2° Fahr.), at the barometric pressure 0.7585 m. = (29.85 inches). The equivalent of cuminic acid containing  $3C_2H_2$  more than the equivalent of benzoic acid, it was interesting to compare the boiling-points of benzonitrile and cumonitrile. According to Fehling's experiments, the boiling-point of benzonitrile is 191° C.; on calculating from this observation the boiling-point of cumonitrile according to the rules first pointed out by Kopp, the boiling-point should be  $191 + 3.19 = 248$ .

Dr. Fehling does not however mention that he had this substance in contact with metal, and it is not improbable that the true boiling-point of benzonitrile is somewhat lower; the vapour of cumonitrile is very inflammable and burns with a bright flame, which deposits much carbon.

\* Professor Fehling found it difficult to estimate the nitrogen in benzonitrile in the form of ammonia, drops of oil passing over into the hydrochloric acid. In the case of cumonitrile, this method gave very accurate results; oil drops also passed over, but they were evidently *cumol*.

The strongest nitric acid has but little action upon this substance; after boiling, however, and setting aside for some days, crystals of cuminic acid are formed. On being heated with potassium it darkened, and apparently another oil was produced; the mass on being washed and tested for cyanogen in the usual manner gave a copious precipitate of prussian blue, which seems to be strongly in favour of the view which Kolbe and Frankland have recently promulgated. A strong alcoholic solution of potash has no immediate action on cumonitrile, but after a day or two, on pouring the liquid into a watch-glass, it partially solidified into a yellow crystalline mass, a mixture of the original substance with white crystals. These crystals after purification had all the appearance of cuminamide, and in order to be satisfied of their composition—

I. 0.174 grm. burnt with oxide of copper yielded 0.472 of carbonic acid and 0.124 of water; the calculated per-centage of carbon and hydrogen from these numbers being—

Carbon	. .	73.62
Hydrogen	. .	7.91

These numbers correspond to those of cuminamide, as may be seen by a comparison with the former analyses.

It appears then that cumonitrile, on the addition of potash, is not, as might have been expected, converted into cuminate of ammonia, but into cuminamide, taking 2 instead of 4 atoms of water— $C_{20}H_{11}N + 2HO = C_{20}H_{13}NO_2$ , the latter body being, as before remarked, in such a remarkable degree unaffected by alkalies or acids.

Having obtained one amide with comparative ease, many other ammoniacal salts were heated for the purpose of obtaining analogous amidogen compounds. Benzoate of ammonia was tried unsuccessfully, and it appears from the account published by Fehling of his investigation of benzonitrile, that the residue in the retort consisted entirely of benzoate of ammonia, that salt appearing to have lost directly 4 equivs. of water without undergoing an intermediate conversion into an amide by the loss of 2 equivs. Nitrobenzoic acid was dissolved in ammonia, evaporated, and cautiously fused for a considerable time; when cold it was found to be insoluble in water and ammonia at the ordinary temperature, but dissolved by hot water, from which it crystallized in beautiful yellow needles. On analysis, the following results were obtained:—

I. 0.222 grm. of substance burnt with oxide of copper yielded 0.410 of carbonic acid and 0.080 of water.

II. 0.255 grm. yielded 0.472 of carbonic acid and 0.087 of water.



466 *On the Decomposition of Cuminate of Ammonia by Heat.*

From these results the following per-centages were obtained:—

	I.	II.
Carbon . . .	50·36	50·43
Hydrogen . . .	4·00	3·78

corresponding to the formula  $C_{14}H_6N_2O_6$ , as may be seen from the comparison of the theoretical and experimental numbers:—

		Theory.	Mean of exp.
14 equivs. of Carbon . .	84	50·60	50·39
6 ... Hydrogen . .	6	3·62	3·98
2 ... Nitrogen . .	28	16·87	
6 ... Oxygen . .	48	28·91	
	<u>166</u>	<u>100·00</u>	

This body is therefore nitrobenzamide, having the same relation to nitrobenzoate of ammonia as cuminate has to cuminate of ammonia.

This beautiful substance can only be obtained with difficulty, as the nitrobenzoate of ammonia explodes violently unless very great caution is employed.

A specimen of chlorobenzoic acid, made in the laboratory for some other investigation, was dissolved in ammonia and heated; it fused readily, became perfectly insoluble in cold water and ammonia, but soluble in hot water, crystallizing as the solution cooled in long needles of great beauty. The specimen of acid afforded me, being all that could be spared, was insufficient for the manufacture of an amide; I prepared a portion of chlorobenzoic acid by acting upon benzoic acid for some days with hydrochloric acid and chlorate of potash; after purification it was burnt with chromate of lead and gave the following results:—

I. 0·394 grm. = 0·769 of carbonic acid and 0·114 of water.

From this result the following per-centage was obtained:—

	Experiment.	Theory.
Carbon . . .	53·22	53·61
Hydrogen . . .	3·22	3·25

leading to the formula  $HO, C_{14} \left\{ \begin{smallmatrix} H_4 \\ Cl \end{smallmatrix} \right\} O_3$ , 1 equiv. of the hydrogen of benzoic acid replaced by an equivalent of chlorine.

This acid, however, on being subjected to the usual treatment by solution in ammonia and subsequent heat, did not fuse but blackened, charcoal being separated. Unfortunately the specimen of ammoniacal salt from which I had made the former compound was not analysed, probably it would have

proved to be  $C_{14} \left\{ \begin{smallmatrix} H_3 \\ Cl_2 \end{smallmatrix} \right\} O_3, HO$ , or  $C_{14} \left\{ \begin{smallmatrix} H_2 \\ Cl_3 \end{smallmatrix} \right\} O_3, HO$ , a dichlorobenzoic or a trichlorobenzoic acid, such existing.

These experiments were conducted in the laboratories of the Royal College of Chemistry under the direction of Dr. Hofmann, to whom I beg to offer my best thanks for his advice and assistance during their progress.

LXXI. On the General Solution (in certain cases) of the equation  $x^3 + y^3 + Az^3 = Mxyz$ , &c. By J. J. SYLVESTER, A.M., F.R.S., late Professor of Natural Philosophy in University College, London\*.

I SHALL restrict the enunciation of the proposition I am about to advance to much narrower limits than I believe are necessary to the truth, with a view to avoid making any statement which I may hereafter have occasion to modify. Let us then suppose in the equation

$$x^3 + y^3 + Az^3 = Mxyz$$

that  $A$  is a *prime* number, and that  $27A - M^3$  is *positive*, but exempt from positive prime factors of the form  $6i + 1$ . Then I say, and have succeeded in demonstrating, that all the possible solutions in integer numbers of the given equation may be obtained by explicit processes from one particular solution or system of values of  $x, y, z$ , which may be called the *Primitive system*.

This system of roots or of values of  $x, y, z$  is that system in which the value of the greatest of the three terms  $x, y, A^{\frac{1}{3}}z$  (which may be called the *Dominant*) is the least possible of all such dominants. I believe that in general the system of the least *Dominant* is identical with the system of the least *Content*, meaning by the latter term the product of the three terms out of which the *Dominant* is elected. I proceed to show the law of derivation.

To express this simply, I must premise that I shall have to employ such an expression as  $S' = \wp(S)$  to indicate, not that a certain quantity,  $S'$ , is a function of  $S$ , but that a certain system of quantities disconnected from one another, denoted by  $S'$ , are severally functions of a certain other system of quantities denoted by  $S$ ; and, as usual, I shall denote  $\wp\wp S$  by  $\wp^2 S$ ,  $\wp\wp^2 S$  by  $\wp^3 S$ , and so forth.

Let now  $P$  be the *Primitive system* of solution of the equation

$$x^3 + y^3 + Az^3 = Mxyz,$$

\* Communicated by the Author.  
2 H 2

$P$  denoting a certain system of values of and written in the order of the letters  $x, y, z$ , which may always be found by a limited number of trials (provided that the equation admits of any solution). That this is the case is obvious, since we have only to give the Dominant every possible value from the integer next greatest to  $A^{\frac{1}{3}}$  upwards, and combine the values of  $x^3, y^3, Az^3$  so that none shall ever exceed at each step the cube of such dominant, and we must at last, if there *exist any solution*, arrive at the System of the Least Dominant.

Now, every system of solution is of one or the other of two characters. Either  $x$  and  $y$  must be odd and  $z$  even, or  $x$  and  $y$  must be one odd and the other even and  $z$  odd. That all three should be odd is inconsistent with the given conditions as to  $A$  being odd and  $M$  even; and if all three were even, by driving out the common factor we should revert to one or the other of the foregoing cases.

The systems of solution where  $z$  is even may be termed Reducible, those where  $z$  is odd Irreducible. Let  $\phi$  denote a certain symbol of transformation hereafter to be explained.

Then the Reducible systems of the first order may be expressed by

$$\phi P, \phi^2 P, \phi^3 P, \text{ ad infinitum;}$$

or in general by  $\phi^{n_1} P$ ,  $n_1$  being absolutely arbitrary. I will anticipate by stating that the function  $\phi$  involves no *variable* constants; that is to say,  $\phi(S)$  may be found explicitly from  $S$  without any reference to the particular equation to which  $S$  belongs. Let now  $\psi$  denote another symbol of transformation, also hereafter to be defined, and differing from  $\phi$  insofar as it does involve as *constants* the three values of  $x, y, z$  contained in  $P$ : then the general representations of Irreducible systems of the first order will be denoted by  $\psi \phi^{n_1} P$ .

It is proper to state here that the symbol  $\psi$  is ambiguous; and  $\psi \phi^{n_1} P$ , when  $P$  and  $n_1$  are given, will have two values, according to the way in which the terms represented by  $P$  are compared with  $x, y, z$  in the given equation

$$x^3 + y^3 + Az^3 = Mxyz;$$

for it is obvious that if  $x=a, y=b, z=c$  satisfies the equation, so likewise will

$$x=b, y=a, z=c.$$

Each however of these values of  $\psi \phi^{n_1} P$  gives a solution of the kind above designated.

Proceeding in like manner as before, the Reducible system of the second order may be designated by  $\phi^{n_2} \cdot \psi \phi^{n_1} \cdot P$ , the Irreducible by  $\psi \phi^{n_2} \cdot \psi \phi^{n_1} \cdot P$ ; and in general *every possible*

system of values of  $x, y, z$  satisfying the proposed equation, in which  $z$  is even, is comprised under the form

$$\phi^{n_r} \cdot \psi \phi^{n_{r-1}} \cdot \psi \dots \phi^{n_2} \cdot \psi \phi^{n_1} \cdot P;$$

and every possible system of such values, in which  $z$  is odd, is comprised under the form

$$\psi \phi^{n_r} \cdot \psi \phi^{n_{r-1}} \cdot \psi \dots \psi \phi^{n_1} \cdot P:$$

the quantities  $n_1, n_2, \dots, n_r$  being of course all independent of one another, and unlimited in number and value.

Thus then we may be said to have the general solution of the given equation in the same sense as an arbitrary sum of terms, each of a certain form, is in certain cases accepted as the complete solution of a partial differential equation.

As regards the value of the symbols  $\psi$  and  $\phi$ ,  $\phi$  indicates the process by which  $a, b, c$  becomes transformed into  $\alpha, \beta, \gamma$ , the relations between the two sets of elements being contained in the following equations:

$$a' = a^3 \quad b' = b^3 \quad c' = Ac^3$$

$$\alpha = a'^2 \quad b' + b'^2 \quad c' + c'^2 a' - 3a'b'c'$$

$$\beta = a' \quad b'^2 + b' \quad c'^2 + c'a'^2 - 3a'b'c'$$

$$\gamma = abc\{a'^2 + b'^2 + c'^2 - a'b' - a'c' - b'c'\}.$$

Next, as to the effect of the Duplex symbol  $\psi$ . Let  $e, g, i$  be the elements of the Primitive system  $P$ :  $i$  being the value of  $z$  and  $e, g$  of  $x$  and  $y$  taken in either mode of combination, each with each, which satisfy the proposed equation

$$x^3 + y^3 + Az^3 = Mxyz.$$

Let  $l, m, n$  represent any system  $S$ ,

$\lambda, \mu, \nu$  represent any system  $\psi(S)$ ,

$\psi S$  has two values, which we may denote by  $\psi' S, \psi'' S$  respectively, and accentuating the elements  $\lambda, \mu, \nu$  accordingly to correspond, we shall have

$$\lambda' = 3gm(gl - em) + 3\Lambda in(il - en) - M(gl^2 - e^2lm)$$

$$\mu' = 3\Lambda in(im - gl) + 3el(em - gl) - M(eim^2 - g^2lm)$$

$$\nu' = 3el(en - il) + 3gm(gn - im) - M(egn^2 - i^2lm):$$

we have then

$$\psi' S \equiv \lambda', \mu', \nu',$$

and in like manner

$$\psi'' S \equiv \lambda'', \mu'', \nu'',$$

$\psi'' S$  being derived from  $\psi' S$  by the mere interchange of  $e$  and  $g$  one with the other.

I have stated that every possible solution of the proposed equation comes under one or the other of the orders, infinite



in number and infinite to the power of infinity in variety of degree, above given: this is not strictly true, unless we understand that all systems of solution are considered to be equivalent which differ only in a multiplier common to all three terms of each; that is to say, which may be rendered identical by the expulsion of a common factor. So that  $m\alpha$ ,  $m\beta$ ,  $m\gamma$  as a system is treated as identical with  $\alpha$ ,  $\beta$ ,  $\gamma$ , which of course substantially it is; and it should be remarked that there is nothing to prevent the operations denoted by  $\phi$  and  $\psi$  introducing a common factor into the systems which they serve to generate, and the latter in particular will have a strong tendency so to do.

I believe that this theorem may be extended with scarcely any modification to the case where  $A$ , instead of being a prime, is any power of the same, and to suppositions still more general. I believe also that, subject to certain very limited restrictions, the theorem *may* prove to apply to the case where the determinant  $27A - M^3$  becomes negative.

The peculiarity of this case which distinguishes it from the former, is that it admits of all the three variables  $x$ ,  $y$ ,  $z$  in the equation

$$x^3 + y^3 + Az^3 = Mxyz$$

having the same sign, which is impossible when the determinant is positive; or in other words, the curve of the third degree represented by the equation  $Y^3 + X^3 + 1 = \frac{M}{A^{\frac{1}{3}}} XY$  (in which I call the coefficient of  $XY$  the characteristic), which, as long as the quantity last named is less than 3, is a single continuous curve extending on both sides to infinity, as soon as the characteristic becomes equal to 3 assumes to itself an isolated point, the germ of an oval or closed branch, which continues to swell out (always lying apart from the infinite branch) as the characteristic continues indefinitely to increase.

I ought not to omit to call attention to the fact that the theorem above detailed is always applicable to the case of the equation

$$x^3 + y^3 + Az^3 = 0,$$

when  $A$  is *any* power of a prime number *not* of the form  $6i + 1$ ; in other words, the above always belongs to the class of equations having Monogenous solutions, which for the sake of brevity may be termed themselves Monogenous Equations\*.

\* Thus the equation  $x^3 + y^3 + Az^3 = 0$  alluded to by Legendre is Monogenous, and the Primitive system of solution is  $x=1$   $y=2$   $z=-1$ , from which every other possible solution in Integers may be deduced.

On the probable existence of such a class of equations I hazarded a conjecture at the conclusion of my last communication to this Magazine. As I hope shortly to bring out a paper on this subject in a more complete form, I shall content myself at this time with merely stating a theorem of much importance to the completion of the theory of insoluble and of Monogenous equations of the third degree; to wit, that the equation in integers

$$a(x^3 + y^3 + z^3) + c(x^2y + y^2z + z^2x + xy^2 + yz^2 + zx^2) + exyz = 0$$

may always be transformed so as to depend upon the equation

$$f u^3 + g v^3 + h w^3 = (6a - e)uvw,$$

wherein  $fgh = ae^2 - (c^2 + 3a^2)e + 9a^2 - 3ac^2 - 2c^3$ .

By means of the above theorem, among other and more remarkable consequences, we are enabled to give a theory of the irresoluble and monogenous cases of the equation

$$x^3 + y^3 + m^3 z^3 = Mxyz,$$

when  $m$  is some power of 2, or of certain other numbers.

26 Lincoln's Inn Fields,  
Nov. 17, 1847.

J. J. S.

ERRATUM.—In the October Number, at page 295, a little below the middle, for  $y=117949000$  read the same with the number 1 added at the end. At page 293, last line, omit the words after "provided" as far as "divisible by 9" on the following page, and read in lieu, "provided in the second case that ABC is of the form  $9m \pm 1$  and that D is divisible by 9."

## LXXII. On *Cochineal* (*Coccus Cacti*). *First Memoir.*

By WARREN DE LA RUE, Esq.\*

THE beautiful theoretical results which have been lately obtained by a closer examination of indigo blue and its products of decomposition, made it desirable to undertake similar investigations with other colouring matters. I made choice of the colouring principle of cochineal (*Coccus Cacti*), hoping that a detailed research might not only prove of interest in a scientific point of view, but also throw some light on its practical applications, and the more so, as the recent investigations of Preisser had seemed to point out a very close

\* Communicated by the Chemical Society; having been read June 21, 1847.

analogy in the chemical properties of a variety of colouring matters with indigo blue.

Before entering into the detail of my experiments, I think it desirable to give a brief outline of the results obtained by the chemists who have hitherto worked on this subject.

Dr. Jean Frédéric John, in a quarto volume translated from the German and entitled *Tableaux Chimiques du Règne Animal*, appears to have published the first analysis of cochineal: he does not describe his method, but merely states that it contains the following per-centage:—

Colouring principle (semi-solid, soluble in water and alcohol) . . . . .	50·00
Gelatine . . . . .	10·50
Waxy fat . . . . .	10·00
Modified mucus . . . . .	14·00
Membrane . . . . .	14·00
Alkaline phosphates and chlorides, phos- phate of lime, phosphate of iron, and phosphate of ammonia . . . . .	1·50
	<hr/> 100·00

Pelletier\* and Caventou, in a very long memoir read before the Institut de France in 1818, have gone very elaborately into the examination of cochineal and obtained many interesting results. In analysing this substance they employed the following process:—They removed the fatty bodies by boiling æther, in which they found the colouring matter but slightly soluble; these fatty substances, recovered by distilling off the æther, were considered to consist of stearine, oleine, and an aromatic acid, from which latter substance it was difficult to remove the adhering colouring matter.

The cochineal, exhausted with æther, was treated with alcohol of 40° Beaumé, which dissolved the colouring matter, together with a small quantity of fatty and nitrogenous substances.

On cooling, and by spontaneous evaporation, they obtained a granular red residue of a semi-crystalline appearance, and which they considered to be the colouring matter contaminated still with nitrogenous matter (*matière animalisée*) and some fatty bodies, the greater part of which remained undissolved in strong cold alcohol; by repeating the operation once or twice they considered that the substance was obtained almost in a state of purity. To remove the last traces

\* *Annales de Chimie et de Physique*, sér. 2, tome viii. p. 250. *Journal de Pharmacie*, sér. 2, tome iv. p. 193.

of foreign matter it was dissolved in strong alcohol, and an equal volume of æther added, which precipitated the colouring matter and retained the fat, which was still adhering to it. The colouring matter thus purified they named carmine (*carminium*), and described as being very soluble in water, from which it did not crystallize, more or less soluble in alcohol, according to its strength, and quite insoluble in æther and the fixed and volatile oils. Acids did not precipitate it from its aqueous solution if free from animal matter. They found hydrochloric and sulphuric acid to decompose it; the latter with elimination of carbon. By the action of nitric acid they obtained an acid in prismatic crystals resembling oxalic acid, but differing in some of its properties.

On heating the "carmine" it intumescenced and gave off carburated hydrogen, a considerable quantity of oily substances, a little acid water, but no trace of ammonia. Chlorine and iodine decomposed it; the alkalies in the commencement produced merely a change in colour, turning it violet, but by the assistance of time or heat they effected a complete alteration. They found an aqueous solution of "carmine" to exhibit the following comportment with reagents.

Of the alkaline earths, lime only produced a precipitate; hydrate of alumina showed a marked affinity, absorbing the whole of the colouring matter from an aqueous as well as an alcoholic solution; the presence of alum prevented this reaction: iron, copper, and silver salts were without reactions; terchloride of gold destroyed the colour; neutral salts of lead merely changed it to violet, except the neutral acetate, which precipitated it, the free acetic acid retaining a little of the compound in solution; the colouring matter could be recovered by decomposing the lead compound with hydrosulphuric acid. The nitrate of mercury gave a purple, and the pernitate a scarlet-red precipitate; the bichloride no precipitate; chloride of tin gave a violet precipitate; the bichloride changed the colour to scarlet without causing a precipitate. Albumen and gelatine had no marked action, but if precipitated by reagents the colouring matter was carried down.

In a later communication (1832), Pelletier\* gave the composition of the colouring matter as prepared by himself and Caventou. In a previous qualitative examination they had failed to exhibit the presence of nitrogen which M. Pelletier now detected. The substance was dried *in vacuo* at a gentle heat to remove every trace of alcohol and æther, and burnt with oxide of copper it yielded—

\* *Annales de Chimie et de Physique*, sér. 2, tome li. p. 194.



Carbon . . . . .	49·33
Hydrogen . . . . .	6·66
Nitrogen . . . . .	3·56
Oxygen . . . . .	40·45
	<hr/> 100·00

M. Pelletier stated, however, that he did not greatly rely on the correctness of this analysis.

After alcohol had dissolved out all the colouring matter removable by it, they extracted the last traces, by repeatedly washing the residue with boiling water, and along with it a little fatty and some nitrogenous matter; the residue was a brownish transparent mass. The later decoctions, containing no red colouring matter, left likewise on evaporation a brownish transparent mass, which they considered identical with the organic residue of the insect. This animal matter had, according to them, some analogy with gelatine, but differed in many of its properties, as it did also from albumen and fibrine, they therefore considered it as peculiar to the cochineal insect; the alkalies and ammonia dissolved it readily; chlorine precipitated it; all acids and acid salts precipitated it, as also acetate of lead, salts of tin and copper, and nitrate of silver; and they considered the latter reagent as a good test of the purity of the colouring matter, as it did not precipitate the latter if free from nitrogenous substances. If the colouring matter were contaminated with nitrogenous substances, all the salts which precipitated the latter carried down likewise some of the colouring matter.

An examination of the ashes showed the presence of phosphate of lime, carbonate of lime, chloride of potassium, and phosphate of potash, to the extent of 0·7 per cent.

In the second part of the memoir they went into the theory of the technical applications of the colouring matter: this having no reference to the present subject, I do not think it necessary to reproduce it here.

M. Lassaigne, in 1819\*, examined Kermes (*Coccus ilicis*), an insect common in the South of Europe, and employed as a red dye before the discovery of America, and obtained by following the methods of Pelletier and Caventou, substances agreeing in their properties with the analogous ones found in cochineal.

M. F. Preissert†, in an elaborate paper on the origin and nature of colouring matters, has again drawn the attention of chemists to the subject. This gentleman, from a study of a variety of colouring substances, comes to the conclusion that all resemble indigo in its behaviour with reducing agents.

\* *Journal de Pharmacie*, sér. 2, tome v. p. 435.

† *Ibid.* p. 191.

He affirms that he obtained by the action of hydrosulphuric acid on the lead compounds of a great number of organic colouring matters, substances bearing the same relation to the original colouring matters as white indigo does to blue indigo. In order to obtain the colourless modification of the colouring matter of cochineal, he adds what he terms "hydrate of oxide of lead" to an aqueous decoction of cochineal, the fats being previously removed by æther. The colouring matter is entirely removed by the so-called oxide of lead, which is nothing but a basic nitrate of lead,  $2(3\text{PbO}, \text{NO}_3) + 3\text{HO}$ . The lead compound suspended in water (hot?) was decomposed by a stream of hydrosulphuric acid; the nearly colourless filtrate deposited on cooling needles of a pale yellow colour, which became perfectly white by washing with æther and pressing between bibulous paper; these crystals, which, according to his statement, are soluble in water and alcohol, but less so in æther, assume in contact with the atmosphere the purple-red of the colouring matter of cochineal. He moreover asserts that his colourless modification gives a white lead salt on adding acetate of lead to its aqueous solution, and that this assumes a purple colour in contact with the air.

He proposes to give the name carmine, hitherto applied to the red colouring matter, to the white crystals, and to designate the red substance by the name "*Carméine*."

The statements of Preisser, generalizing most beautifully under one head the chemical character of all colouring matters, making indigo as it were the prototype of them all, could but induce other chemists to work out more in detail the relations cursorily pointed out in the memoir of this chemist. Unfortunately a careful repetition of these experiments has not confirmed the basis on which his theory reposes.

M. A. E. Arppe repeated Preisser's experiments on the colouring matter of cochineal\*. He found that by proceeding in the manner described by Preisser that he could only obtain a red solution, which on evaporation was converted into white crystals of oxalic acid by the nitric acid derived from the basic lead salt.

Arppe now prepared a pure oxide of lead by precipitating acetate of lead with potash. He found that this would not take down the colouring matter in the cold, but by boiling it is carried down as a blue lake, which he decomposed by hydrosulphuric acid; the supernatant liquor was nearly colourless, the colouring matter he found adhered with great obstinacy to the sulphuret of lead, from which water, alcohol and ammonia failed to separate it; but sulphuret of ammo-

\* Liebig's *Annalen*, vol. lv. p. 101.

nium and acids rendered it perceptible. He likewise tried to obtain the colouring matter in a state of purity by precipitating with hydrated oxide of lead an aqueous decoction of cochineal (previously purified from nitrogenous substances by nitrate of silver). On treating the precipitate by hydrosulphuric acid, he obtained a red liquor of *strongly acid reaction*, the acid of which was not derived from the lead salt: on evaporation it left a dark red mass, emitting the smell of burnt sugar. Wishing to separate the acid, which he thought contaminated the colouring matter, he prepared a strong aqueous decoction of cochineal, and after separating the nitrogenous matter by means of nitrate of silver, filtering, and then saturating by ammonia, and afterwards adding the hydrated oxide of lead, he found that the supernatant ammoniacal liquor, which was nearly colourless, yielded by evaporation an acid liquid; and on decomposing the lead lake with hydrosulphuric acid, he obtained a liquid slightly coloured (the colouring matter adhering to the sulphuret of lead), which was likewise acid. From this he concluded that the colouring matter had not up to that period been obtained in a state of purity.

#### *Microscopic Examination of Living Cochineal.*

By the kindness of Sir James Clark, who furnished me with specimens of the living insect, I have been enabled to examine the physical characters of the colouring matter as it exists in the organism of this little insect before it is subjected to the process of drying for commerce. On examination by the microscope, the white dust which covers the insect and the adjacent parts of the cactus leaves, on which it feeds, has all the characters of an excrement; it has a curled cylindrical form, is of very uniform diameter and of a white colour. On removing the powder with a little æther and piercing the side of the little creature, a quantity of a purplish red fluid exudes, which contains the colouring matter in minute granules assembled round a colourless and larger nucleus, and these groups float in a colourless fluid. It is evident from this, that, whatever may be the function of the colouring matter, it has a distinct and marked form, and does not pervade as a mere tint the fluid portion of the insect.

#### *Separation of the Colouring Matter.*

It became evident from a few preliminary experiments that the investigation would be greatly facilitated by the employment of a large quantity of material; and as in the course of the inquiry different methods were adopted for the prepara-

tion of the colouring matter, capital letters will be used to designate the various preparations.

A. About 3 lbs. of ground cochineal (technically known as shelly black) was introduced into 15 gallons of boiling distilled water, and the mixture maintained at that temperature for twenty minutes; the decoction, strained through a sieve, was allowed to subside for a quarter of an hour and then decanted off; whilst still hot the transparent liquid was mixed with basic nitrate of lead, added with caution to avoid excess; a fine purple lake was thus obtained, the supernatant liquor retaining only a pale buff tinge. After decantation of the supernatant liquor, the lake was thrown on a cloth filter and washed with distilled water until the filtrate gave but a slight opalescence with chloride of mercury, which was found to be a test for the presence of nitrogenous matter. The lead lake was then suspended in distilled water and treated with a copious stream of hydrosulphuric acid, when a precipitate of sulphuret of lead and a deep red supernatant liquid was obtained; on stirring the liquid this colour almost disappeared, the colouring matter being evidently absorbed by the sulphuret, agreeing perfectly with Arppe's observation. A fresh stream of gas reproduced the colour, which was again absorbed on stirring; after continuous treatment with hydrosulphuric acid, the lead lake being completely decomposed, the filtered liquid was evaporated in a water-bath to a syrupy consistence, and the evaporation finished as far as possible at a temperature of  $38^{\circ}\text{C}$ . The semi-solid substance thus obtained was of a deep purple colour, had a strongly acid reaction, and evolved the smell of burnt sugar, as noticed by Arppe. The weight of this substance, which I call crude carminic acid, was  $3\frac{1}{2}$  ozs., and 1 oz. more was obtained from the residue by similar treatment.

B. On repeating the same process the whole product was lost. An excess of the basic nitrate having been employed, the nitric acid set free by the hydrosulphuric acid caused a violent decomposition, with evolution of nitrous fumes, as soon as the carminic acid arrived at a pasty consistence; this agrees also with Arppe's experience.

C. In this operation a decoction of cochineal, made in the described manner, was precipitated with a solution of acetate of lead acidulated with acetic acid (six parts by weight of crystallized acetate, and one part of strong acetic acid). The resulting lead lake, being very bulky, was washed by decantation with boiling distilled water, collected on a filter, dried in a current of warm air, and finely powdered; 17 ozs. of *crude carminate of lead* were thus obtained.



D. Half a pound of cochineal was boiled with five pints of alcohol, spec. grav. .830. The filtered tincture deposited on cooling a granular precipitate, consisting chiefly of fatty matter retaining a portion of colouring matter; on concentrating the tincture by distillation a further quantity was deposited, which was filtered off; the filtrate was evaporated to dryness *in vacuo*, when after eight weeks a gummy residue was obtained. This mass dissolved with great difficulty in a large quantity of absolute alcohol, a red flocculent substance consisting chiefly of nitrogenous matter remaining undissolved. The alcoholic solution filtered off from this deposit, concentrated by distillation and finally evaporated *in vacuo* over sulphuric acid, dried to a tenacious semi-solid mass, covered with a colourless oily fluid, and containing crystalline particles of a solid fat. After removal of the fats by means of æther, this mass was digested in water at 38° C., which partly dissolved it with a fine red colour, leaving a brown mass of resinous aspect behind, more of which deposited on the cooling of the coloured liquid; the decoction was now evaporated to the consistence of a syrup, and finally dried *in vacuo* over sulphuric acid.

These are all the processes employed to *extract* the colouring matter from the cochineal; I may here remark, before entering on the details of its further purification, that I obtained other substances on evaporating the mother-liquors from which the colouring matter had been separated by lead salts, which will be hereafter described.

*Purification of the Carminic Acid.*—In my first attempts to purify the colouring matter I proceeded in the following way:—An aqueous solution of the crude carminic acid (A) was precipitated with acetate of lead, the precipitate of carminate of lead well-washed and decomposed by hydrosulphuric acid; the red supernatant liquid was first concentrated on the water-bath and finally dried *in vacuo*; a highly hygroscopic purple residue was thus obtained.

I could not, by whatever means I adopted, effect the decolorization of the colouring principle. In several attempts I heated the solution for some hours to 100° C., keeping up a continuous current of hydrosulphuric acid, and in other experiments a stream was made to pass for several days through the disengaged colouring matter, but without the slightest change in its aspect. From these experiments, made with the greatest care and at several periods, I am led to the same conclusion as Arppe, that Preisser must have been mistaken in his results, and I regret that I cannot throw any light on the probable cause of his error.

Several combustions of the carminic acid thus purified were made, the resulting numbers however became useless by the subsequent observation that this acid was by no means pure. A sufficient quantity being incinerated left a residue of acid reaction, which was suspected to contain phosphoric acid. Carminic acid burning only with great difficulty, it was reconverted into carminate of lead, the oxide of lead dissolved out of the residue obtained after fuming by acetic acid, which left a white residue of metaphosphate of lead, together with a little lead. The white residue was soluble in dilute nitric acid, and exhibited, when treated before the blowpipe, the characters of metaphosphate of lead; other tests likewise confirmed the presence of phosphoric acid. It will hereafter be seen that the process of extracting the colouring matter by alcohol (D) does not exclude the phosphoric acid, which in all probability existed in the colouring matter analysed by Pelletier. It is further evident that the presence of phosphoric acid explains most satisfactorily the facts observed by Arppe.

a. In order to separate the phosphoric acid, another portion of crude carminic acid (A) was precipitated with acetate of lead. Three-fourths of the carminate of lead produced were decomposed by hydrosulphuric acid and evaporated to dryness in the way already mentioned. The dry mass being dissolved in cold absolute alcohol, and filtered from a slight flocculent brownish residue, was heated to ebullition in a water-bath and mixed with the remaining fourth of the carminate of lead, which had been previously reduced to a fine powder; the ebullition was continued for a few hours. In this method the free phosphoric acid combined with the lead, liberating an equivalent proportion of carminic acid, which was taken up by the alcohol. The alcoholic solution was filtered whilst hot, concentrated by distillation, and then evaporated *in vacuo* in the presence of sulphuric acid; it dried into a granular mass of a deep purple-brown colour, detaching itself spontaneously from the sides of the vessel, and on examination by the microscope was found to be a beautiful transparent crimson substance, exhibiting only slight, if any, signs of crystalline structure; by pulverization it became of a fine scarlet colour; it left a mere trace of ash, and was found to be perfectly free from phosphoric acid. It was highly hygrometric\*.

\* In consequence of this it was found convenient to dry the carminic acid intended for analysis in little stoppered tubes in the air-pump, as the stopper could be rapidly inserted after desiccation, and access of air effectually prevented.

Burnt with chromate of lead,—

I. ·4647 grm. gave ·9096 grm. carbonic acid and ·2175 grm. water.

II. ·4630 grm. gave ·9105 grm. carbonic acid and ·2140 grm. water.

For the latter analysis I am indebted to my friend Mr. Nicholson.

*b.* A second preparation of carminic acid was made by operating on the crude carminate of lead (C) and treating the resulting crude carminic acid in the manner just described for the preparation *a*. It left on incineration 0·2 per cent. of ash (·1609 grm. giving ·0003 grm. ash), which was neglected in the following analyses:—

III. ·3710 grm. gave ·7316 grm. carbonic acid and ·1710 grm. water.

IV. ·3685 grm. gave ·7235 grm. carbonic acid and ·1722 grm. water.

*c.* To effect the purification of the carminic acid (D) obtained by digesting cochineal in alcohol, it was dissolved in water and precipitated by acetate of lead; the filtrate was found to contain nitrogenous matter, and the carminate of lead to be contaminated with phosphate of lead; it was therefore treated in the manner already detailed.

V. ·3925 grm. of this substance gave ·7658 grm. carbonic acid and ·1780 grm. water.

*d.* A fourth preparation of carminic acid was obtained by substituting phosphoric acid for hydrosulphuric in the decomposition of the crude carminate of lead (C), and evaporating the carminic acid to dryness in contact with a fresh portion of carminate of lead; this did not however separate entirely the phosphoric acid, it was therefore redissolved in boiling absolute alcohol, and maintained some time at that temperature with more carminate of lead. On analysis—

VI. ·3805 grm. gave ·7530 grm. carbonic acid and ·1848 grm. water.

Pelletier having obtained in his analysis of “carmine” (carminic acid) as much as 3·56 per cent. of nitrogen, all the before-cited preparations of carminic acid were carefully examined qualitatively for nitrogen by heating with soda-lime, and without exception gave indications of its presence; in most cases but a mere trace was found, but I thought it necessary notwithstanding to make a few quantitative determinations, especially as M. Berzelius\* had pointed out the improbability of it being an essential constituent.

\* *Traité de Chim.* t. iii. p. 808. Brussels, edit. 1839.

The last preparation (*d*) appearing to contain more than any of the others, it was chosen and burnt with soda-lime.

It was indispensable in experiments of this nature to test the purity of the soda-lime as regarded the absence of ammonia. A tube having 9 inches of its length filled with soda-lime was heated to redness, just as in a nitrogen determination; the hydrochloric acid, being treated with bichloride of platinum in the usual manner, gave 7.5 milligrammes of ammonio-chloride of platinum; and a repetition of the experiment gave a similar result. This allowance has been made on all the nitrogen determinations by soda-lime.

·5938 grm. carminic acid (*d*) gave ·0717 grm. ammonio-chloride of platinum = 0.76 per cent. of nitrogen.

This quantity of nitrogen could not be supposed to belong to the composition of the colouring matter, but was evidently due to some foreign substance, and not improbably to ammonia. In order to purify the carminic acid still more, the same specimen (*d*) was dissolved in a small quantity of boiling absolute alcohol and the filtered solution mixed with three times its bulk of anhydrous æther; a splendid scarlet precipitate was immediately produced, which absorbed water rapidly from the atmosphere, and agglutinated into a dark purple mass; when dried it weighed 0.3 grm. (*e*). The filtrate, which was of a pale orange-red colour, left on evaporation 0.5 grm. of carminic acid (*f*).

·2635 grm. (*e*) burnt with soda-lime gave ·0637 grm. ammonio-chloride of platinum = 1.52 per cent. of nitrogen.

·4732 grm. (*f*) gave ·0150 grm. ammonio-chloride of platinum = 0.2 per cent. of nitrogen.

We have therefore (*e*) 0.3 grm. found to contain by analysis 1.5 per cent. nitrogen, and (*f*) 0.5 grm. 0.2 per cent.,

$$\frac{(3 \times 1.5) + (5 \times 0.2)}{8} = .69 \text{ average per cent., which agrees}$$

closely with .76, found previous to treatment with æther.

*g.* Another preparation of carminic acid was obtained by precipitating crude carminic acid with acidulated acetate of copper, which salt was found to carry down the carminic acid, and to leave in solution by far the greater portion of the phosphoric acid. The carminate of copper, which occupied a long time in washing, was collected and decomposed by hydrosulphuric acid. The filtrate was evaporated to dryness, dissolved in boiling absolute alcohol, filtered, concentrated by distillation, and again evaporated to dryness *in vacuo*. It still contained a trace of phosphoric acid. On evaporating the mother-liquor and filtering, from an impure carminate of copper which deposited as the acetic acid was driven off, and



again concentrating to dryness, a brown mass was obtained, which on incineration left a greenish-white very hygrometric ash, in which phosphoric acid, soda and copper were found. Burnt with chromate of lead—

VII. .4020 grm. carminic acid (*g*) gave .7842 grm. carbonic acid and .1662 grm. water.

This acid however still retained some impurities: on incineration it left 0.4 per cent. of ash (.5489 grm. giving .0022 grm. ash), and examined for nitrogen it gave the following numbers:—

.4731 grm. burnt with soda-lime gave .0150 grm. ammonio-chloride of platinum = 0.2 per cent. of nitrogen.

*h.* In order to separate these impurities the greater portion was dissolved in boiling absolute alcohol, and filtered from a slight residue; the remainder, about an eighth, was converted into carminate of lead and digested with the boiling alcoholic solution for some hours; the alcoholic tincture filtered off whilst hot and mixed with about six times its volume of anhydrous æther; this threw down a bulky precipitate of a fine red colour, which was separated by filtration and the filtrate concentrated in a retort, and finally evaporated to dryness *in vacuo* (*h*).

*i.* The precipitate retained on the filter was dried *in vacuo*, then dissolved in as small a quantity of alcohol as possible, and again mixed with a large quantity of æther; this determined a precipitate which was no longer of a fine red but of a brown colour, and on re-solution and similar treatment it diminished in quantity and became darker in colour, leaving the colouring matter in solution. From the filtrates a quantity of carminic acid (*i*) was obtained on evaporating to dryness *in vacuo*. It therefore appears that the æther precipitates a nitrogenous body which carries down with it variable quantities of carminic acid, according as a larger or smaller relative proportion is present. The carminic acid (*h*) was found to be free from phosphoric acid as well as nitrogen.

.3003 grm. burnt with soda-lime gave .0015 grm. ammonio-chloride of platinum = 0.03 per cent. of nitrogen.

From this analysis I venture to assert that the colouring principle of cochineal contains no nitrogen, thus fully confirming the anticipation of Berzelius. We can now understand from the preparation of the specimen of carminic acid (*e*), that the method employed by Pelletier for the preparation of the substance he analysed was calculated to accumulate all the nitrogenous matter contained originally in his alcoholic decoction; a fact which fully explains the large amount of nitrogen he obtained in his analysis.

An analysis of the carminic (*h*) acid by chromate of lead gave from—

VIII. .3167 grm. .6203 grm. carbonic acid and .1402 water.

The following table exhibits the per-centage results deduced from the foregoing analyses: the specimens were all dried over sulphuric acid *in vacuo*, with the exception of analysis VII., in which the carminic acid was dried at 100° C.

	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	Mean.
Carbon ...	53.38	53.63	53.78	53.55	53.21	53.97	53.20	53.42	53.51
Hydrogen	5.20	5.14	5.12	5.19	5.04	5.39	4.59	4.92	5.07

By the analysis of a copper salt of carminic acid hereafter to be mentioned, it became probable that carminic acid might still retain, when only dried *in vacuo*, a portion of the solvents employed; a presumption which was supported by the analysis VII., in which the substance analysed had been dried at 100° C., and which gave a smaller per-centage of hydrogen. A portion of carminic acid (*i*), being first dried *in vacuo*, and then heated to a temperature of 121° C., was found to yield a small quantity of acetic acid, and was not altered in its properties, which were not in fact changed even at a temperature of 136° C.

In the following analyses the carminic acid, previously dried *in vacuo*\* and then at a temperature of 120° C., gave, on burning with chromate of lead, the following results:—

IX. .3347 grm. (*h*) gave .6648 grm. carbonic acid and .1381 grm. water.

X. .3583 grm. (*i*) gave .7108 grm. carbonic acid and .1504 grm. water.

These analyses give the following per-centage quantities:—

	IX.	X.
Carbon . . .	54.17	54.10
Hydrogen . .	4.58	4.66

The analysis IX. being of the same preparation as had served for analysis VIII., it is fair to presume that all the other specimens of carminic acid would have given the same per-centage quantities as the specimen (*h*) if dried at 120° C., as this particular specimen, dried *in vacuo*, yielded numbers in close accordance with the mean of the other analyses.

These numbers converted into the most simple expression lead to the following formula,  $C_{14} H_7 O_8$ ; but an analysis of a copper salt renders it probable that this formula has to be

\* The carminic acid fuses if exposed to a temperature of 120° C. without having been previously dried.

doubled, and that the composition of carminic acid is expressed by the formula,



as may be seen from the following table containing the comparison of the theoretical per-centages with the mean of analyses IX. and X.

		Theory.	Experiment.
$\text{C}_{28}$	. 168	54.19	54.13
$\text{H}_{14}$	. 14	4.52	4.62
$\text{O}_{16}$	. 128	41.29	41.25
	<u>310</u>	<u>100.00</u>	<u>100.00</u>

From the foregoing experiments, it seems that the best method of obtaining pure carminic acid is to precipitate the aqueous decoction by acetate of lead; to decompose the washed carminate of lead by hydrosulphuric acid, and to throw down the carminic acid once more by acetate of lead, previously mixed with acetic acid; to decompose the carminate of lead by hydrosulphuric acid; to evaporate to dryness and redissolve the carminic acid in absolute alcohol; then to digest the alcoholic tincture with carminate of lead; and lastly, to precipitate the trace of nitrogenous matter by æther, the pure carminic acid is obtained from the filtrate.

As thus prepared, carminic acid has the following properties. It is a purple brown friable mass, transparent when viewed by the microscope, and pulverizing to a fine red powder; soluble in water and alcohol in all proportions, very slightly soluble in æther, which does not however precipitate it from its alcoholic solution if free from nitrogenous matter. It is soluble without decomposition in concentrated hydrochloric and sulphuric acids. It is decomposed by chlorine, iodine and bromine, which change its colour to yellow, and the latter on warming or by standing gives a yellow precipitate soluble in alcohol. Nitric acid decomposes it even if highly diluted: I shall have occasion to refer to this decomposition presently. It bears a temperature of  $136^{\circ} \text{C}$ . without decomposition; on gradually increasing the temperature a quantity of an acid liquor is produced, and at a red heat it intumesces and gives off a small quantity of red fumes, which condense: it gives no trace of oily matter.

The aqueous solution has a feeble acid reaction; it does not absorb oxygen. A volume of this gas contained in a tube with carminic acid over mercury did not change by absorption after exposure for several months. The fixed alkalies and ammonia give no precipitate in the aqueous solution, merely changing its colour to purple; in the alcoholic tincture they

produce purple precipitates; all the alkaline earths produce purple precipitates; sulphate of alumina gives no precipitate, but on addition of a drop of ammonia the carminic acid is immediately taken down as a beautiful crimson lake; acetates of lead, copper, zinc and silver give purple precipitates; the latter is immediately decomposed, and silver deposited; the nitrates of lead, mercury and silver reddish precipitates; protochloride and bichloride of tin no precipitates, but change the colour to a deep crimson.

The acid character of carminic acid being so very little pronounced, I met with considerable difficulties in determining its atomic weight; it is only with great reserve that I bring forward the formula before cited. Several attempts were made to produce soda, baryta, lead and copper compounds, but it was only with the copper salt that I obtained results agreeing in different preparations.

It seems that carminic acid attaches itself to salts, for it was found that the precipitants could be removed from the precipitates only with the greatest difficulty. I omit several soda, baryta and lead determinations which have not led to any satisfactory result, and confine myself to the statement of the result of the analysis of the copper compound. It was obtained by acidulating an aqueous solution of pure carminic acid with acetic acid, and then precipitating by the cautious addition of acetate of copper, so as to leave an excess of carminic acid in the liquid. The precipitate was well-washed by decantation (by which a great loss was sustained) and dried. It formed into masses of a bronze colour, very hard and difficult to powder. Two specimens were prepared at different times (*a* and *b*).

I. .2800 grm. (*a*) dried at 100° C. left, after ignition and treatment with nitric acid and re-ignition, .0330 grm. oxide of copper.

II. .3782 grm. (*b*) dried at 100° C. gave .0426 grm. oxide of copper.

III. .4702 grm. (*b*) dried at 100° C. gave on burning with chromate of lead .8210 grm. carbonic acid and .1743 grm. water.

These numbers lead to the following per-centage results:—

	I.	II.	III.
Carbon . . .	...	...	47.62
Hydrogen . .	...	...	4.12
Oxide of copper	11.78	11.27	

agreeing closely with the formula,  $C_{28}H_{14}O_{16}, CuO$ , as will be seen from a comparison of the theoretical and experimental numbers.



		Calculated.	Found.
Carbon . . . 28	168	48·05	47·62
Hydrogen . . . 14	14	4·01	4·12
Oxygen . . . 16	128	36·61	36·74
Oxide of copper 1	39·6	11·33	11·52
	<u>349·6</u>	<u>100·00</u>	<u>100·00</u>

*Action of Nitric Acid on Carminic Acid.*

*Nitrococcusic Acid*.—When acting with nitric acid on “carmine” (carminic acid), MM. Pelletier and Caventou obtained white acid crystals resembling oxalic acid, but differing from this acid in several of its properties. M. Arppe found that the acid produced was oxalic acid. In my experiments I obtained the following results:—One pound and a half of crude carminic acid was gradually introduced into ten pounds of nitric acid, spec. grav. 1·4, and digested at a moderate heat; a violent evolution of nitrous fumes succeeded each addition of the carminic acid; after the whole quantity had been introduced and the action had somewhat subsided, the mixture was transferred into a smaller vessel and the action continued at the boiling-point for about two hours; by this time the greater part of the nitric acid had evaporated, and on withdrawing the vessel from the fire and allowing the mixture to cool, a crystalline cake was obtained, which on examination was found to consist partly of a new acid and partly of oxalic acid. To separate the oxalic acid, it was dissolved in a large quantity of boiling water and treated with nitrate of lead as long as any precipitate formed; this was collected and decomposed by boiling with dilute sulphuric acid; the filtrate from the sulphate of lead yielded a large quantity of prismatic crystals of oxalic acid, which were obtained perfectly white and pure after two or three crystallizations with the aid of a little animal charcoal.

The yellow liquid filtered from the oxalate of lead was concentrated and separated from a fresh portion of oxalate which deposited on concentration, the evaporation was then continued until a large quantity of crystals formed; the solution on cooling deposited a very bulky mass of yellow rhombic prisms, which were drained and dried, and re-dissolved in a sufficient quantity of boiling water, which on cooling deposited the acid (for which I propose the name of nitrococcusic acid) in beautiful crystals free from any lead salt; it was recrystallized twice more, by which means it was obtained perfectly pure.

Several preparations were made, sometimes using pure car-

minic acid, at other times carminate of lead, with similar results.

The analyses of four different preparations dried at 100° C. gave, on burning with chromate of lead (unless otherwise stated), the following numbers:—

I. .3152 grm. (a) gave .3892 grm. carbonic acid and .0561 grm. water.

II. .2500 grm. (a) gave .3080 grm. carbonic acid and .0445 grm. water.

(For this analysis I am indebted to Mr. Nicholson.)

III. .3068 grm. (a) gave .3820 grm. carbonic acid and .0502 grm. water.

IV. .4498 grm. (b) gave .5626 grm. carbonic acid and .0757 grm. water.

V. .4461 grm. (c) gave .5515 grm. carbonic acid and .0777 grm. water.

VI. .4503 grm. (d) gave, on being burnt with oxide of copper, .5585 grm. carbonic acid and .0757 grm. water.

VII. .4796 grm. (c) gave, on being burnt with oxide of copper, and a layer of copper twelve inches long used so as to completely decompose the binoxide of nitrogen, .5882 grm. carbonic acid and .0815 grm. water.

The foregoing analyses lead to the following per-centage quantities:—

	I.	II.	III.	IV.	V.	VI.	VII.
Carbon .	33.67	33.60	33.95	34.11	33.72	33.82	33.44
Hydrogen	1.98	1.98	1.82	1.87	1.93	1.87	1.89

In the following experiments the nitrogen of the nitrococcus acid was ascertained by burning with oxide of copper in an atmosphere of carbonic acid.

VIII. .6808 grm. (b) dried at 100° C. gave 84 cub. cent. of moist nitrogen at 6°·5 C. and 0.7585 m., barometer corrected.

IX. .7162 grm. (c) dried at 100° C. gave 91.5 cub. cent. of moist nitrogen at 17°·5 C. and 0.7641 m., barometer corrected.

These numbers correspond to the following per-centage quantities:—

	VIII.	IX.	Mean.
Nitrogen . .	15.03	14.92	14.97

X. In this experiment the nitrogen was determined according to Bunsen's\* method, which consists in burning the substance mixed with oxide of copper in the presence of copper turnings in a hard glass tube. The tube being freed from air by a stream of hydrogen, is then exhausted, sealed hermetically, and placed in an iron mould filled with plaster of Paris; it is then heated to redness and allowed to cool. After the com-

\* Liebig's *Annalen*, vol. xxxvii. p. 27.

bustion, the gas is transferred into a graduated jar over mercury and its volume noted; the carbonic acid being absorbed by a potash ball, the volume is again read off. This analysis gave the following numbers:—

	Vol.	Temp.	Diff. of level.	Barom.
Carbonic acid + nitrogen (moist)	123	20°·7 C.	0 <sup>m</sup> ·0640	0 <sup>m</sup> ·7543
Nitrogen . . . . .	22·2	20°·0	0 <sup>m</sup> ·1650	0 <sup>m</sup> ·7529

The height of the column of mercury in the eudiometer above the level in the trough and the barometric column are corrected for temperature.

Carbonic acid + nitrogen corrected to 0° C. and barom.	1 <sup>m</sup> = 76·84
Nitrogen . . . . .	0°      ...      1 <sup>m</sup> = 12·16
Carbonic acid . . . . .	0°      ...      1 <sup>m</sup> = 64·68

$\frac{64·68}{12·16} = 5·32$ , which is the ratio of carbon equivalents to one nitrogen equivalent.

The preceding analyses of nitrococcusic acid agree with the following formula, confirmed by the analyses of several of its compounds,



as will be seen on referring to the table.

	Theory.		Experiment.
			Mean.
Carbon . 16	96	33·45	33·75
Hydrogen 5	5	1·74	1·91
Nitrogen. 3	42	14·63	14·97
Oxygen . 18	144	50·18	49·37
	287	100·00	100·00

By analysis VII., in which the precaution was taken of using a very long layer of copper turnings, there was obtained, carbon 33·44, hydrogen 1·89; these numbers agree as closely as possible with the theoretical quantities, as does likewise the nitrogen determination (X.) by Bunsen's method; in this experiment the ratio of carbon equivalents to nitrogen equivalents was found to be as 5·32 to 1, or as 16 equivs. of carbon to 3·007 equivs. of nitrogen; taking analysis VII. as the basis of calculation, it gives 14·67 per cent. of nitrogen, the theoretical number being 14·63.

The acid, as it separates from its aqueous solution, contains water of crystallization, which it loses at 100° C.; four experiments gave the following results:—

·4800	gram.	lost	·0289	gram.	= 6·02	per cent.
·6613	...	...	·0395	...	= 5·97	...
·6586	...	...	·0385	...	= 5·84	...
·4804	...	...	·0289	...	= 6·01	...
Mean		.	.	.	= 5·96	...

This mean corresponds perfectly with the formula



as may be seen by a comparison of the theoretical and experimental numbers.

		Theory.		Experiment.
				Mean.
1 equiv. dry acid	. . .	287	94.10	94.04
2 ... water	. . .	18	5.90	5.96
1 ... crystallized ni- trococcusic acid	. . } . . }	305	100.00	100.00

*Properties of Nitrococcusic Acid.*—It is of a yellow colour, crystallizing in rhombic plates, and presenting very different aspects, according to the circumstances under which it is crystallized. Its solution stains the skin yellow, it is soluble in cold, but considerably more so in hot water; soluble in alcohol, and very soluble in æther. All its salts dissolve readily in water, and most of them in alcohol; it deflagrates violently on being heated; it dissolves iron and zinc, becoming dark-coloured. It is decomposed by sulphuret of ammonium with separation of sulphur and the formation of the ammonia salt of a new acid, which I have not yet examined.

#### *Compounds of Nitrococcusic Acid.*

*Nitrococcusate of Potash.*—I have prepared this salt by two different methods.

*a.* A solution of nitrococcusic acid in boiling water was accurately saturated with carbonate of potash; by evaporation to a small bulk and cooling, the salt was obtained in small yellow crystals; it was purified by draining and recrystallizing.

*b.* An ætherial solution of the acid was precipitated by the cautious addition of an alcoholic solution of potash; the pale yellow precipitate washed with æther and dried, then dissolved in as small a quantity of cold water as possible, and the solution poured into about five times its bulk of absolute alcohol; after standing some time the salt crystallized in well-formed crystals; it was washed with æther and dried. The ætherial washings being added to the mother-liquor, a further portion was obtained and washed with æther. The latter process is less troublesome than the process *a*.

I. .5469 grm. (*a*) dried at 100° C. were dissolved in a small quantity of boiling water and decomposed by sulphuric acid; dried in a water-bath, the nitrococcusic acid, removed by æther and the residue ignited, gave .2606 grm. sulphate of potash.

II. .4383 grm. (*b*) dried at 132° C. gave .2103 grm. sulphate of potash.

III. .6251 grm. (*b*) dried at 100° C. and burnt with chro-



mate of lead, gave ·6064 grm. carbonic acid and ·0662 grm. water.

These numbers give the following per-centage quantities :—

	I.	II.	III.
Carbon . .	...	...	26·46
Hydrogen .	...	...	1·18
Potash . .	25·74	25·92	

corresponding with the formula



as may be seen by comparing the theoretical and experimental numbers.

	Theory.		Experiment.
			Mean.
Carbon . 16	96	26·45	26·46
Hydrogen 3	3	·83	1·18
Nitrogen . 3	42	11·57	
Oxygen . 16	128	35·26	
Potash . 2	94	25·89	25·83
	363	100·00	

I was not successful in preparing a nitrococcusate of potash with one equivalent of fixed base ; the method I adopted was saturating a given weight of acid with carbonate of potash, and then adding the same amount of acid to the bibasic potash salt ; on washing with æther the greater part of the excess of acid was removed, leaving the bibasic salt behind.

*Nitrococcusate of Ammonia*.—This salt was prepared by passing an excess of dry ammoniacal gas through an ætherial solution of the acid dried in the atmosphere ; the solution became turbid, and by standing for a short time deposited the salt in clusters of needles adhering firmly to the sides of the vessel ; these were removed, washed with æther, and dried on bibulous paper. It is volatile, and sublimes on being heated, most probably with decomposition.

I. ·6011 grm. of the salt dried *in vacuo* was dissolved in a small quantity of boiling water and decomposed by strong hydrochloric acid, which immediately separated the acid in crystals ; the mixture was dried in a water-bath, and the nitrococcusic acid removed by æther, a little bichloride of platinum and alcohol being added to the ætherial washings to precipitate a trace of chloride of ammonium. The residue, precipitated as ammonio-chloride of platinum, gave ·8208 grm. of the double chloride.

II. ·6126 grm. dried *in vacuo* and burnt with oxide of copper, the mixture being made in the combustion-tube, gave ·6525 grm. carbonic acid and ·2191 grm. water.

These numbers correspond with the following per-centage quantities:—

	I.	II.
Carbon . . . . .	...	29.05
Hydrogen . . . . .	...	3.97
Oxide of ammonium . .	15.91	

agreeing closely with the following formula,



as may be seen by a comparison of the theoretical and experimental numbers.

	Theory.		Experiment.
Carbon . 16	96	29.09	29.05
Hydrogen 12	12	3.64	3.97
Nitrogen . 5	70	21.21	
Oxygen . 19	152	46.06	
	<u>330</u>	<u>100.00</u>	

Or

	Theory.		Experiment.
Acid . . . . . 1	269		
Water . . . . . 1	9		
Oxide of ammonium 2	52	15.76	15.91
	<u>330</u>		

*Nitrococcusate of Baryta* was prepared by adding an excess of a solution of baryta to an aqueous solution of nitrococcusic acid, a stream of carbonic acid gas being passed through the solution to separate the excess of baryta. The solution was warmed, filtered and evaporated in a water-bath, and again filtered from a small quantity of carbonate of baryta. The evaporation being continued until a pellicle formed on the surface, the solution on cooling deposited this salt in minute yellow crystals. It is insoluble in alcohol, which precipitates it in the form of a jelly from the aqueous solution.

I. .6750 grm. of substance dried at 100° C. and decomposed by sulphuric acid, gave .3602 grm. of sulphate of baryta.

II. .6439 grm. of nitrococcusate of baryta dried at 100° C. and burnt with chromate of lead, gave .5185 grm. of carbonic acid and .0800 grm. of water.

These numbers correspond to the following per-centage quantities:—

	I.	II.
Carbon . . . . .	...	21.96
Hydrogen . . . . .	...	1.38
Baryta . . . . .	35.06	

agreeing with the formula  $C_{16}H_3N_3O_{16} + 2BaO + 2HO$ , as may be seen from the following table:—

	Theory.		Experiment.
Carbon . 16	96·00	21·80	21·96
Hydrogen 5	5·00	1·14	1·38
Nitrogen . 3	42·00	9·54	
Oxygen . 18	144·00	32·71	
Baryta . . 2	153·28	34·81	35·06
	<u>440·28</u>	<u>100·00</u>	

*Nitrococcusate of Silver*.—I attempted to make this salt by boiling oxide of silver with an aqueous solution of nitrococcusic acid, but there was an evident decomposition of the acid, a large quantity of carbonic acid being evolved; after warming the filtered liquor a brown deposit was formed. On filtering off this brown deposit a silver salt was obtained by evaporation, which yielded on analysis—

Carbon . . . .	23·64
Hydrogen . . .	1·26
Oxide of silver . .	38·10

per-centage numbers not reconcilable with those of nitrococcusate of silver.

On decomposing a hot solution of this salt with hydrochloric acid a new acid was obtained, perfectly distinct from nitrococcusic acid; it crystallized in long needles; very insoluble in water, but soluble in æther and alcohol. I refrain from giving any further account of this acid until the study is completed.

In order to avoid decomposition the nitrococcusate of silver was prepared without the aid of heat, by dissolving carbonate of silver in a cold aqueous solution of nitrococcusic acid and evaporating the filtered solution *in vacuo* over sulphuric acid. The salt crystallized in long bulky needle-like crystals of a yellow colour; when dried at 100° C. the powdered salt becomes deep orange.

It is soluble in alcohol and water, and is highly explosive when heated; in small quantities it may be decomposed by a progressive heat without any violent action; but on attempting to decompose a quantity amounting to ·45 grm. in a porcelain crucible, heated in an air-bath, the salt exploded with great violence, shattering the copper air-bath and driving fragments of the crucible through the copper; the temperature was noted just before the explosion, the thermometer standing at 200° C.; the silver was therefore determined as chloride.

I. .4698 grm. of substance (*a*) dried at 100° C. and decomposed by nitric acid and the silver precipitated by the addition of hydrochloric acid, gave .2675 grm. chloride of silver.

II. .5085 grm. of substance (*b*) dried at 100° C. gave .2892 grm. chloride of silver.

III. .8184 grm. of substance (*a*) dried at 100° C. and burnt with oxide of copper, gave .5700 grm. carbonic acid and .0554 grm. water.

Corresponding to the following per-centage quantities:—

	I.	II.	III.
Carbon . . . . .	...	...	18.99
Hydrogen . . . . .	...	...	0.75
Oxide of silver . . .	46.03	45.97	

and agreeing closely with the following formula,



as may be seen by the following table:—

	Theory.		Experiment. Mean.
Carbon . . . . 16	96	19.162	18.99
Hydrogen . . . . 3	3	.599	.75
Nitrogen . . . . 3	42	8.383	
Oxygen . . . . 16	128	25.549	
Oxide of silver . 2	232	46.307	46.00
	501	100.000	

*Nitrococcusate of Copper*.—This was made by dissolving carbonate of copper in nitrococcusic acid and deposited on evaporation in pale apple-green needles. I made no analysis of this salt.

The following is a synoptical table of the analyses of nitrococcusic acid and its compounds:—

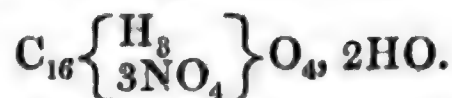
Hydrate of nitrococcusic acid .	$\text{C}_{16} \text{H}_3 \text{N}_3 \text{O}_{16} + 2\text{HO}.$
Hydrate of nitrococcusic acid } as crystallized from water }	... .. + 2HO + 2Aq.
Nitrococcusate of potash . . .	... .. + 2KO.
Nitrococcusate of ammonia . .	... .. + 2NH <sub>4</sub> O + Aq.
Nitrococcusate of baryta . . . .	... .. + 2BaO + 2Aq.
Nitrococcusate of silver . . . .	... .. + 2AgO.

The properties of nitrococcusic acid and its salts exhibit a great analogy with those of a number of acids obtained by the action of nitric acid on organic bodies, more especially nitropicric and styphnic acids, from which it differs by the greater solubility of its salts.

If we assume with many chemists the nitrogen of these



acids to exist in the form of hyponitric acid, the formula of nitrococcusic acid will be represented by



This acid would consequently derive from a non-nitrogenous acid, having the composition expressed by the formula



When I first began this investigation I imagined that a similar relation might exist between nitrococcusic acid and carminic acid; the analysis of these acids, however, as well as the simultaneous production of a large quantity of oxalic acid in its oxidation, showed that this view was erroneous, and that nitrococcusic acid was derived from carminic acid in a more complex manner. Some attempts were made to produce the non-nitrogenous acid, the coccusic acid, but unsuccessfully.

The experiments of MM. Cahours and Laurent on the oxidation of the oils of anise and of tarragon (*Oleum dracunculi*) have made us acquainted with anisic acid, the composition of which is  $\text{C}_{16} \text{H}_8 \text{O}_6$ . The formula agrees with the composition of the hypothetical hydrated coccusic acid.

Anisic acid, however, as well as nitranisic acid, being monobasic, it was not probable that the further introduction of the elements of hyponitric acid would convert it into a bibasic one; nevertheless it was my intention to have studied the further action of nitric acid on the acids mentioned, in order to obtain if possible trinitroanisic acid, and to compare this substance with the acid obtained from carminic acid, when an account of some new experiments of M. Cahours came under my notice, of the action of a mixture of concentrated sulphuric and nitric acids on anisic acid, by which he has succeeded in preparing trinitroanisic acid. The experiments of M. Cahours have not yet been published in detail, and from his short account it was not possible to decide on the identity or non-identity of nitrococcusic and trinitroanisic acids. A small specimen of anisic acid at my disposal was treated in the manner described by him; after acting for some time water threw down an acid, from the insolubility of which I conclude that these acids are only isomeric.

*Investigation of the Mother-liquor from which the Carminic Acid had been separated.*

On evaporating the mother-liquors of carminic acid and separating the lead held in solution by means of hydrosul-

phuric acid, they all gave the following results: on acquiring a syrupy consistence, a white chalky-like matter subsided; this was separated by filtration, and proved to be a new crystalline body. The liquor filtered off from this substance deposited a small quantity more on further concentration, and could only be dried to a soft tenacious mass, partly soluble in alcohol, the rest being soluble in water. From three pounds of cochineal five ounces of this soft matter were obtained, showing that the precipitation by a lead salt had effected the separation of carminic acid from a large quantity of foreign matters. This gelatinous matter appears to be of a complex character, but I have not yet examined it fully.

To purify the chalky-like matter, it was well-washed with cold water and crystallized twice by solution in boiling water and evaporation; it was then dissolved by boiling it in a just sufficient quantity of water; animal charcoal was now added, and the ebullition continued for a few minutes; the solution filtered whilst hot deposited on cooling a mass of silky crystalline tufts, completely filling the liquid, and when collected and dried they aggregated into paper-like masses of a silky aspect. I obtained in three experiments 4 parts of the new body from 1000 of cochineal.

I. .4918 grm. of substance, preparation (a), dried *in vacuo* and burnt with oxide of copper, gave 1.0705 grm. carbonic acid and 0.2838 grm. water.

II. .5680 grm. of substance (b) gave 1.2416 grm. carbonic acid and .3160 grm. water.

III. .4700 grm. of substance (b) gave 1.0210 grm. carbonic acid and .2660 grm. water.

For the latter analysis I am indebted to the kindness of Mr. Nicholson.

A qualitative examination having pointed out the presence of nitrogen, it was determined by Varrentrapp and Will's method.

IV. .5046 grm. of substance (a) dried *in vacuo* and burnt with soda-lime, gave .6131 grm. ammonio-chloride of platinum.

V. .5076 grm. of substance (b) gave .6239 grm. ammonio-chloride of platinum.

From these numbers the following per-centages are calculated:—

	I.	II.	III.	IV.	V.
Carbon .	59.36	59.62	59.25		
Hydrogen	6.41	6.18	6.29		
Nitrogen .	... .	...	...	7.62	7.71

These per-centages, translated into the most simple expression, lead to the formula,  $C_{81} H_{11} NO_6$  as may be seen from the following table:—

	Theory.		Experiment.
			Mean.
Carbon . . . 18	108	59.668	59.41
Hydrogen . . 11	11	6.077	6.29
Nitrogen . . . 1	14	7.735	7.66
Oxygen . . . . 6	48	26.520	
	181	100.000	

Careful and repeated examinations for sulphur proved the absence of this element as a component of the new white substance. I have been unable to produce a compound to control the proposed formula, though several methods were adopted; amongst others, I attempted to form a lead compound by adding acetate of lead to an ammoniacal solution of the substance; I obtained merely a bulky precipitate, consisting of little else than oxide of lead.

This substance is sparingly soluble in cold water, much more so in boiling water; insoluble in alcohol and æther; soluble in hydrochloric acid, which appears to be driven off by evaporation, leaving the substance in large crystals. In a large quantity of nitric acid it dissolves with a slight evolution of gas; the solution evaporated spontaneously furnishes long crystals, which are in all probability a new acid; if dissolved in a small quantity of nitric acid, the mixture becomes spontaneously heated, violent action takes place, and the product is lost; frequently the substance becomes blackened into charred masses. It is soluble in ammonia, from which it is again recovered by the evaporation of the ammonia. It is soluble in the fixed alkalies, and is precipitated from these solutions by saturating with an acid.

In a short paper, entitled "*Valerianic Acid and a new body from Casein*," Baron Liebig\* describes a new substance obtained by fusing casein with hydrate of potash until an evolution of hydrogen takes place along with ammonia. On saturating with acetic acid the aqueous solution of the fused mass an aggregate of fine needles was produced, which were purified by repeated solution in carbonate of potash and reprecipitation by acetic acid. A preliminary analysis led to the formula  $C_{16} H_9 NO_5$ , differing from the result I obtained in the analysis of the white substance from cochineal by two carbon, two hydrogen, and one oxygen. The properties of the two bodies being however so analogous, it is extremely probable that they are identical, a presumption I am sup-

\* Liebig's *Annalen*, vol. lvii. p. 127.

Fig. 1.

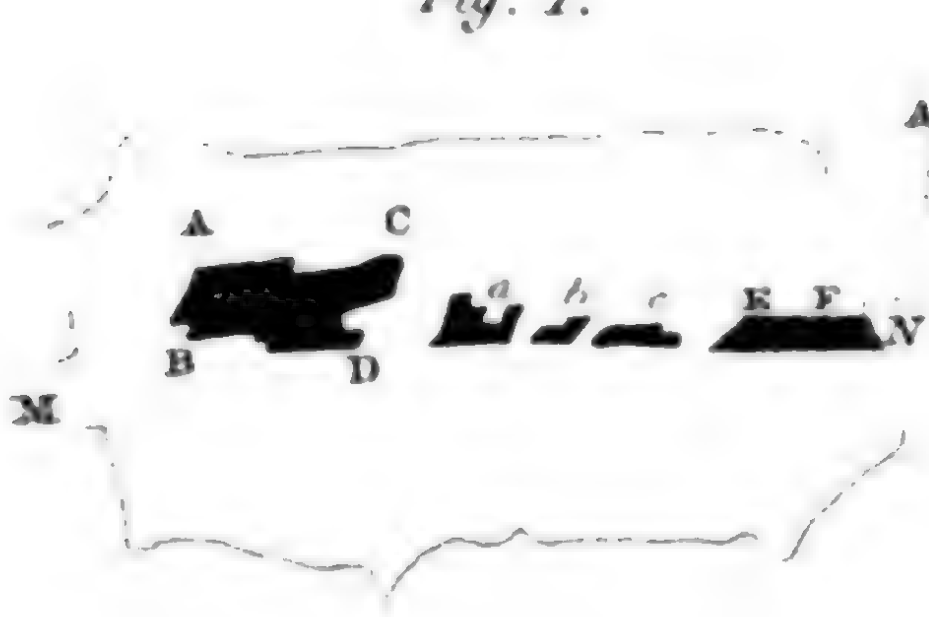


Fig. 2.

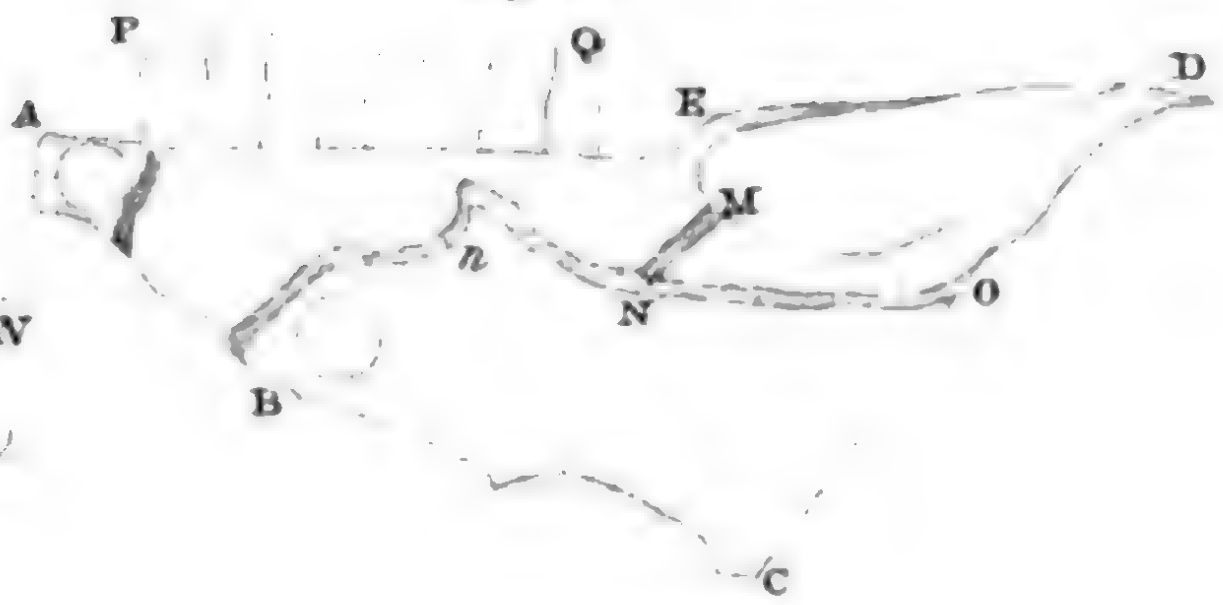


Fig. 3.



Fig. 4.



Fig. 5.



Fig. 6.

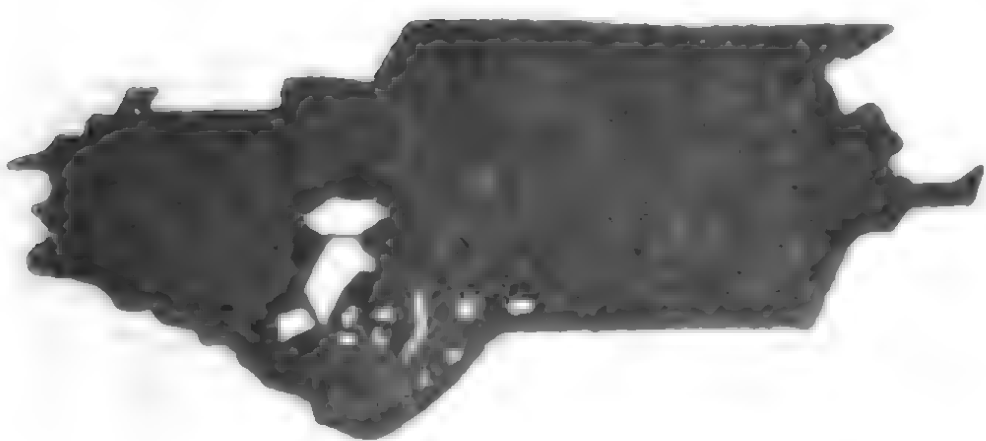
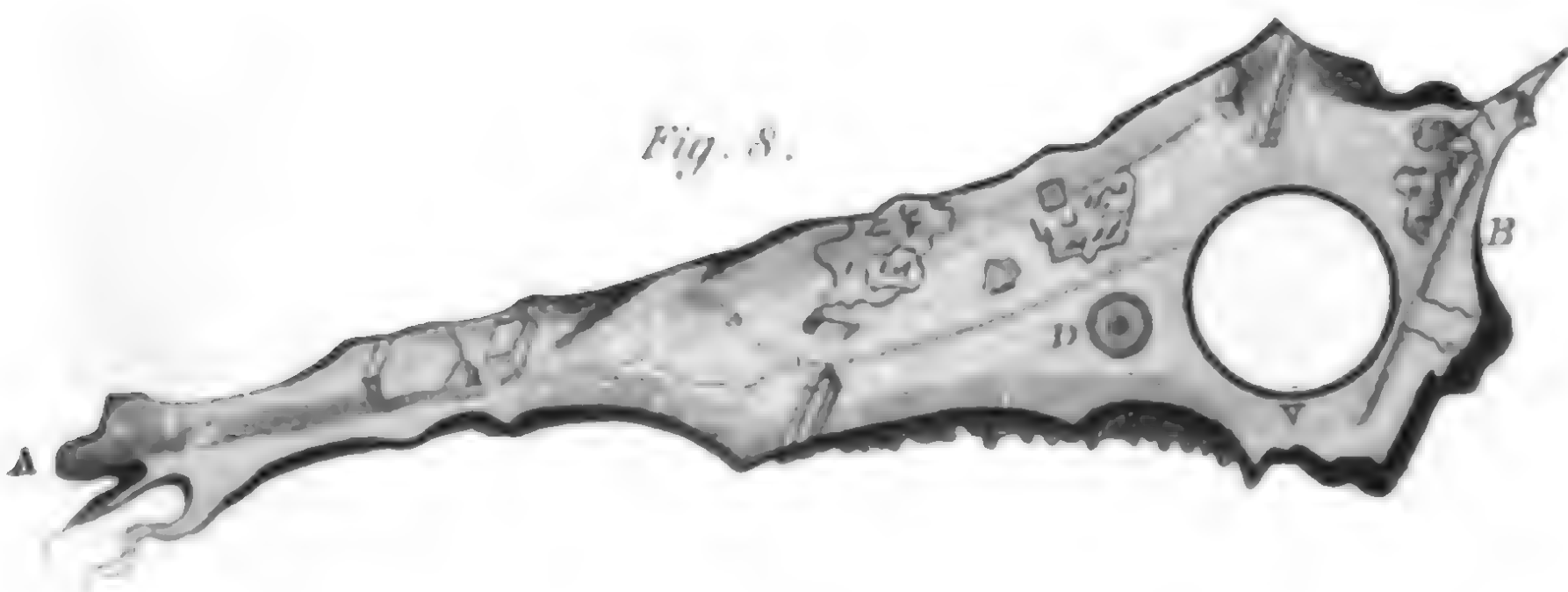


Fig. 7.



Fig. 8.





ported in by a comparison of a specimen kindly furnished me by Dr. Hofmann \*; further investigations will clear up this point: in the meantime I refrain from proposing a name, as Liebig † has lately proposed the name Tyrosine for the substance prepared from casein. As the latter body arises evidently from a process of oxidation, and as I had obtained the first crop of crystals from a liquid from which the colouring matter had been precipitated by the basic nitrate of lead, I thought that this body might owe its formation to the action of the nitric acid liberated by the sulphuretted hydrogen; but this supposition proved to be erroneous, for in later experiments in which acetate of lead had been used, the same body, and in exactly the same quantity, was obtained. From this we may assume that this substance is contained ready-formed in the cochineal insect.

My engagements for the present preventing me from continuing these researches, I must defer for a future period their completion, but hope to be enabled to communicate to the Society a second paper. In conclusion I may be allowed to express my thanks to my friend Dr. Hofmann for his valuable instruction in the methods of organic research, and his kind advice during the progress of this investigation.

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**LXXIII.** *On the Existence of Crystals with different primitive forms and physical properties in the Cavities of Minerals; with additional Observations on the New Fluids in which they occur.* By Sir DAVID BREWSTER, K.H., LL.D., F.R.S., and V.P.R.S. Edin.‡

[With a Plate.]

**I**N 1823 and 1826 I communicated to the Society two papers on the nature and properties of two immiscible fluids, which I discovered, in contact with each other, in the cavities of topaz and other minerals§. Although the facts contained in these papers were of so extraordinary a nature as to be received with scepticism by some, and with ridicule by others, yet I am not aware that, during the *twenty* years which have elapsed since their publication, any person has either repeated my observations, or advanced a single step in the same path of inquiry. In showing to strangers some of the leading phænomena of the two new fluids, my attention has been frequently recalled to the subject; but it was not till

\* This specimen had been prepared by Baron Liebig himself.—A.W.H.

† Researches on the Chemistry of Food, p. 16.

‡ Read before the Royal Society of Edinburgh on the 17th of February 1845, and published in their Transactions, vol. xvi. part 1. p. 11.

§ Edinburgh Transactions, vol. x. p. 1 and 407.

*Phil. Mag. S. 3. No. 211. Suppl. Vol. 31.*

last spring, when I discovered cavities in topaz filled with the most beautiful crystals of various form, that I was induced to undertake a new investigation of their nature and properties. In this investigation I have examined, with various magnifying powers, and both in common and polarized light, more than 900 specimens of topaz from Scotland, New Holland, and the Brazils; and I have had the good fortune to observe many new phænomena connected with mineralogy, chemistry, and physics, which, in addition to the interest which they may possess as scientific facts, promise to throw a strong light upon the existing theories of crystallization, and to bring before us some of those recondite operations which had been going on in the primitive rocks of our globe, before the commencement of vegetable or animal life.

1. *On the Form and Position of the Strata in which the Cavities lie.*

The cavities which contain the two new fluids, and their accompanying crystals, sometimes occur single, and in groups more or less numerous; but, in general, they exist in millions, occupying extensive strata, which affect the transparency of the mineral, and render it unfit for the use of the jeweller, or even for the cabinet of the collector, who has not learned that it is in the deviations from her ordinary laws that Nature often discloses her deepest mysteries.

Although the strata of cavities sometimes occur, as in artificial salts, in planes parallel to the primary or secondary forms of the crystal, yet they occupy every *possible position* in reference to these planes; and we therefore cannot account for them by supposing that certain spaces have been left in the crystal, without the primitive molecules which ought to have been there deposited. The strata of cavities, too, have every possible curvature. From a plane surface they pass into a curved one, sometimes of variable curvature, and sometimes of contrary flexure, cutting and intersecting each other in the most capricious manner.

In the shape of the strata the same irregularity presents itself; their outline is sometimes rectilineal, sometimes curved, and sometimes singularly irregular. In some specimens the whole crystal is intersected with the strata; and it is extremely probable, though it is impossible to determine the fact, that in every specimen some edge or angle of the stratum touches the surface.

The succession of the cavities in composing the stratum, and their form in relation to the character of the stratum, present interesting phænomena. I have found specimens in

which the cavities lie in concentric arches, and have their sides concentric, and, as it were, a portion of the same arches, as if they had been formed under the influence of a rotatory force. In other cases they occupy parallel lines, and are sometimes so equidistant that they might be advantageously used as micrometers for microscopes. In one remarkable specimen they radiate from a centre, each radiation having a character of its own. One radiation will sometimes throw off a diverging branch, while two or more radiations will converge and then diverge again, subsequently uniting themselves into a single radiation.

When different strata of cavities lie parallel to each other in the specimen, which they sometimes do, to the number of *four* or *five*, each stratum has generally a distinct character; flat and exceedingly thin cavities occupying one stratum, very deep cavities occupying another, minute cavities which the highest magnifying powers can scarcely resolve occupying a third, while a fourth consists of the most irregular and indescribable forms.

When the forms of individual cavities are related to that of the stratum which contains them, they, of course, cut at all angles the primary and secondary planes of crystallization; and the same is true of insulated cavities of great length, which are sometimes turned, and twisted, and bent in the most capricious manner. It is impossible to read these details, and still more so to study the phænomena themselves, without being driven to the conclusion, that the strata of cavities must have been formed under the influence of forces propagated through a soft and plastic mass, and carrying along with them gases and vapours which came to a position of rest previous to the regular crystallization of the topaz. This conclusion, which I have been led to draw, in another paper, from a series of entirely different facts, will be still further confirmed by the phænomena of imbedded crystals, to which I shall have to refer in another section.

## *2. Additional Observations on the Nature and Properties of the two New Fluids.*

In re-examining the phænomena exhibited by the two new fluids, I have found no occasion to modify or to correct any of the results contained in my former papers. In the cavities which appear to contain only one fluid, namely, the dense fluid, I have sometimes found a very small quantity of the volatile fluid, which, with a slight rise of temperature, passes into vapour, and prevents the apparent vacuity from disappearing by the application of a strong heat. When there is

no volatile fluid present in such cavities, the vacuity is a real one, and disappears entirely by the application of such a heat. If the heat is not instantly withdrawn on the disappearance of the vacuity, the crystal never fails to burst with great violence.

In some specimens of Brazil topaz I have found cavities with two fluids, and without any vacuity in the volatile fluid at the ordinary temperature of an apartment. In such cases I have generally produced a vacuity by the application of ice. Had heat been applied, the crystals would have burst, as there were no empty spaces into which the fluids could expand.

When the cavities are flat, and have their faces perpendicular to the axis of the crystal, or parallel to the planes of *easy cleavage*, the application of heat does not burst the crystal, but produces a very remarkable phænomenon. The cavity opens at its weakest point, and the fluid passes by starts, through a succession of resting places, to another part of the crystal where it finds the readiest exit. The fluid penetrates, as it were, the solid gem, and the laminæ which it has forced asunder in its passage, again close into optical if not into mechanical contact. If the heat is withdrawn when the first minute drop has passed, the laminæ unite, and we can discharge the rest of the fluid whenever we please till the cavity is exhausted. This phænomenon is represented in Plate III. fig. 1, where ABCD is a shallow cavity in a plate of topaz MN, and EF another cavity, which has been emptied of its fluid contents by reaching the surface at N, where it had been broken through. Upon looking at the cavity ABCD when slightly heated, I observed dark portions of fluid rushing from its sharp termination at D through the cavity at *a*, and then reappearing at *b* and *c*, and then passing into the empty cavity EF. The small lakes, as we may call them, at *a*, *b* and *c*, disappeared entirely when the discharged portions of fluid had passed, and reappeared with a change of form and size when the operation was repeated.

In a specimen of topaz possessed by Major Playfair, and seen by many individuals, a white ball passed from one cavity to the edge of the specimen, as if projected from a mortar; but by the application of too strong a heat it was shattered in pieces.

In my first paper of 1823\*, I have described and figured a phænomenon of an analogous kind; but as it appeared unexpectedly, and was instantly followed by the explosion of the crystal, I could neither observe it accurately, nor confirm what I did observe, by a repetition of the experiment. I have, therefore, some satisfaction in describing a similar phæno-

\* Edinburgh Transactions, vol. x. p. 11, plate 1. fig. 5, 6.



menon, seen frequently, and under more favourable circumstances, not only from its intrinsic interest, but because a distinguished philosopher had treated with an air of incredulity an observation which I had made of a similar kind. There can be no higher testimony to the novelty and importance of a scientific fact, than when a competent judge raises it to the supernatural.

I come now to describe a property of the dense fluid, so new and remarkable that it cannot fail to excite the attention of chemists. This fluid occupies the whole of a large cavity ABCDE, fig. 2, with the exception of a bubble at A, which must be either a vacuum, as it is in all cavities containing only this fluid, or a bubble of the expansible fluid, or the vapour of the dense fluid, or some gaseous body. It cannot be a vacuum, because it expands with heat, in place of being filled up by the expansion of the fluid. It cannot be the expansible fluid, because cold would contract it, and produce a vacuity. It cannot be the vapour of the expansible fluid, because there is no expansible fluid to throw it off, and it has not the optical properties of its vapour. It cannot be the vapour of the fluid in the cavity, for it does not disappear by the application of cold, and does not become a vacuity, which fills up by the expansion of the fluid. It is therefore an independent gas, which exhibits the following phænomena.

When heat is applied, the bubble A expands, not by the degradation of its circular margin passing into vapour, as in the vapour cavities described in a former paper, but by the rapid enlargement of its area. When it attains a certain size, it throws off a secondary bubble B, which passes over a sort of ridge or weir *mno*, in the bottom of the cavity, and settles at B. If the heat is continued, these two bubbles increase in size; but it was instantly withdrawn when B had begun to swell. As the topaz began to cool, both the bubbles A and B quickly contracted. The primary bubble A returned gradually to its original condition, and B, when reduced to a single speck, would have disappeared, had the cooling not been stopped. This speck swelled again by the application of heat, and so did the bubble A. When the speck at B was allowed to vanish, which it did on the spot which the bubble occupied, the fresh application of heat did not revive it at that spot, but merely expanded the primary bubble A, which again threw off a secondary bubble B, which exhibited by heat and cold the same phænomena as before. These experiments I repeated many times with the same result. It will naturally be asked, what was the condition of the fluid itself which has the property of expanding by heat; and what be-

came of it while a part of the space which it occupied was appropriated by the bubble B, and the addition to the bubble A? An accidental circumstance enables me to answer this question, which would have been otherwise a very perplexing one. Having applied too strong a heat to the specimen, the bubble A threw off beside B two or three smaller ones, which moved along the upper edge AE. My attention having been thus directed to this part of the specimen, I was surprised to observe a great number of capillary lines or pipes PQ, rising from the edge AE of the cavity, and into which the fluid was forcing itself, oscillating in these minute tubes like the mercury in a barometer, and sometimes splitting the laminæ between them. The force of cohesion, thus overcome by the expansive efforts of the fluid, predominated over the capillary attraction of the tubes and surfaces, and pressed back all the fluid into the cavity, when the body of fluid had contracted in cooling.

If we now consider the body which occupies the vacuity A as a gas, and, consequently, the other bubble B as the same, it follows that the whole of the gas in B was absorbed by the fluid while cooling, and again given out by an increase of temperature. The gas, when in the act of being discharged, took its course to the locality of the speck at B, and to the bubble A; but to the bubble A alone when the speck had disappeared.

Upon repeating these observations the cavity burst; and I have now before me its two halves, forming its upper and its under surface. The portion of the cavity at A has the same depth as the portion below *mno*, all the rest of the cavity being much shallower. There was a fine doubly refracting crystal at MN, which polarized the blue of the second order; and its outline is still left on the cavity. There was a sort of crystalline powder disseminated round MN to a considerable distance, and the roof of the bubble B, when the roof of the cavity was entire, was always mottled with this powder.

In a former paper, I have distinguished vapour cavities from common cavities, by the manner in which the vacuity in the expansible fluid disappears. In the one case, the vacuity gradually enlarges by the degradation, as it were, of its margin, as the fluid passes into vapour; in the other, the vacuity gradually diminishes till it disappears. I have since found cavities of an intermediate character, in which the vacuity, on the first application of heat, diminishes, and then, when it has contracted to a certain size, it begins to expand; and its margin becoming thinner and thinner, it finally passes into vapour.

### 3. On the Form and Position of Crystals in the Cavities of Topaz.

In a former paper I have described a moveable group of crystals of carbonate of lime, which I discovered in a cavity in quartz from Quebec, containing a fluid with the properties of water. The crystals to which I am about to call attention are of a very different kind, and possess a very different kind of interest.

The crystals which occupy the fluid cavities of topaz are either fixed or moveable. Some of the fixed crystals are often beautifully crystallized. They have their axes of double refraction coincident with those of the crystal, and, as I have ascertained by the examination of exploded cavities, they actually form part of the solid topaz, though they exist in the fluid cavity. One or two of these are shown in fig. 4, plate 19, of my paper of 1826\*, and they may be distinguished by their attachment to the sides of the cavity. In the same figure, as well as in figs. 10, 13, 20, and 21 of my paper of 1823†, I have drawn others which I then believed to be fixed, but which I have no doubt are moveable, and produced from one or other of the new fluids.

In re-examining my specimens of *topaz*, I have been surprised at the great number of cavities which contain crystals. In some there are only one; in very many there are two, three, and four; and in a great number of specimens the cavity is so crammed with them, like a purse full of money, that the circular vacuity has not room to take its natural shape, and often can scarcely be recognised, in its broken-down condition, among the jostling crystals.

The crystals of which I am treating are sometimes found in the volatile, and sometimes in the dense fluid, but chiefly in the latter. They are often found in an amorphous state in the narrow necks and narrow extremities of cavities, positions in which they remain fixed while they continue solid; and sometimes regularly formed crystals remain fixed between the prismatic edges of cavities, in consequence of having either fallen into that position, or of having been formed there.

The crystals in topaz cavities are, in general, beautifully crystallized, and have a great variety of forms. I have observed the following:—

1. The tetrahedron.
2. The cube.
3. The cube, truncated on its edges and angles.
4. The rhombohedron.

\* Edinburgh Transactions, vol. x.

† Ibid, plates 1 and 2.

5. The prism, with plain and pyramidal summits.
6. The flat octohedron, truncated on its edges and angles.
7. Rhomboidal plates.
8. Hexagonal plates.
9. Long rectangular plates.

Besides these, there are amorphous crystals and crystallized masses of various characters.

#### 4. *On the Physical Properties of the Crystals in Topaz Cavities.*

Although it would be desirable to submit these crystals, as well as the fluids which contain them, to chemical analysis, yet the task is too difficult to be accomplished in the present state of chemical science. I must therefore limit my observations to such of the physical properties of these crystals as can be rendered visible to the eye.

When I first applied heat to the crystals under consideration, I employed a very fine specimen, with large and numerous crystallized cavities, of a prismatical form, containing both the new fluids. In this specimen there were seven cavities unlike all the rest, and each of them containing a single crystal, and apparently but one fluid, namely, the dense one. The cavities were exceedingly flat, and irregular in their shape, and very unlike one another. Upon applying the heat of only a lighted paper match beneath the plate of glass on which the specimen lay, I was surprised to see the crystals gradually lose their angles, and then slowly melt, till not a trace of them was visible. In this state one of the cavities had the appearance shown in fig. 3, where V was the vacuity, and  $v$ ,  $v'$  other two bubbles, one of which  $v$  soon joined the principal one V. In all the other six cavities the crystals were speedily reproduced, always at the point where they disappeared, provided a small speck remained unmelted; but otherwise in different parts of the cavity. In the cavity AB, however, fig. 3, the crystal was very long in appearing. In the course of an hour, however, a fasciculus of minute crystals appeared in the centre of the vacuity, as in fig. 4, and to them the principal crystal attached itself, as in fig. 5, which exhibits a perfect rhomboidal plate, truncated on its obtuse angles. The elliptical vacuity was pressed into the shape of a heart; and, by the application of ice, I succeeded in precipitating the vapour of the expansible fluid, which existed in a very minute quantity in all the seven cavities. The expansible fluid is shown between the two heart-shaped outlines in the figure, and I repeatedly threw it into vapour, and reduced that vapour to a fluid state. The phænomenon now described, of the melting of the crystals, and their subsequent recrystallization, I have shown to various



persons; and it is very remarkable that they generally reappear in this specimen of the same form, though with considerable modifications.

Upon applying heat to other cavities containing several crystals, I obtained very different results. Some of them melted easily, others with greater difficulty; and some were not in the slightest degree affected by the most powerful heat I could apply. When the crystals melted easily, they were as quickly reproduced; sometimes reappearing more perfectly formed than before, but frequently running into amorphous and granular crystallizations.

In some specimens of topaz all the crystals in the cavities refuse to melt with heat, and seem not to suffer the slightest change in their form. Hence we are entitled to conclude, that the crystals possessing such different properties must be different substances; and this conclusion is amply confirmed by an examination of their optical properties.

In making this examination, I used a polarizing microscope, so constructed that the plane, passing through the optical axis of the topaz, could be readily placed either parallel or perpendicular to the plane of primitive polarization. In this case the field of the microscope is wholly obscure, in so far as the depolarizing action of the plate of topaz is concerned; but if there is any crystal in the topaz, either imbedded in its mass, or included in its cavities, that crystal will exhibit its doubly refracting structure, if it has any, by its depolarizing action. It may, indeed, happen,—and it does happen,—that the plane passing through their optical axes coincides, either accurately, or so nearly, with that of the topaz, that its depolarizing action is a minimum; but an experienced observer will have no difficulty in distinguishing this want of depolarization by position, from the want of it by structure.

When the specimen of topaz is rich in cavities full of crystals, the display of luminous and coloured crystalline forms in the dark field of the microscope, indicating, too, the imprisonment of fluids, and the condensation of gases before vegetable or animal life had visited our primæval globe, was as interesting to the imagination and the judgement as it was beautiful to the eye. Having had the privilege of being the first to see it, I felt the full influence of the sight; and I have again and again contemplated it with renewed wonder and delight. When the cavities are so numerous as to mock calculation, and so infinitely small as to yield no visible outline to the highest powers, the bright twinkle of a crystalline atom within them reveals to us their nature as well as their contents.

In the examination of the individual crystals, many interest-

ing facts present themselves to our notice. The crystals of the tessular class, which are modifications of the cube, are very numerous, and have no action upon polarized light. Many of them melt easily, while others refuse to yield to the action of heat; and hence there must be two different substances in the cavities which assume the same shape. In like manner, some of the doubly refracting crystals melt readily, others with very great difficulty, and others not at all; so that there must be *three* different substances, which belong to the classes of forms that give double refraction; a conclusion which is confirmed by the different secondary forms which I have already enumerated.

I have seldom found any crystals in these cavities which depolarize white light, or the highest order of colours. I have found some that depolarize *four* orders of colours; and when the crystal which does this is a flat hexagonal plate, it is highly interesting to see it pass through all the tints which these orders include, while slowly melting, and again reproducing them during its recrystallization.

In a cavity which was so placed as to be entirely black from the total reflexion of the light which fell upon it, I observed three *white* openings, *a*, *b*, *c*, of a crystalline form (see fig. 6). These appeared to be fixed crystals, or rather parts of the topaz, surrounded by a cavity. I found, however, that the hexagonal one *C* depolarized white light, while the rest had no action upon polarized light. Upon applying heat, the crystal *c* melted, and took up a position at *c*, fig. 15, in a narrower part of the cavity, where it remains of an irregular form, having been repeatedly melted and recrystallized. Upon turning the cavity into a position where it became transparent, I found that there was no fluid whatever in the cavity; so that we have here an example of a crystal melting and recrystallizing without having been dissolved in one of the fluids. From the irregular state of the laminæ close to this cavity, there is every appearance of the fluids having escaped from one of its extremities.

In the course of these observations, I observed a phenomenon, produced by heat, of the most novel and surprising kind, and one which I feel myself utterly unable to explain. It presented itself when I was studying the very interesting collection of crystals in the cavity AB, fig. 8. This cavity is filled with the dense fluid, in which there is a vacuity *V*: the fluid swells to such a degree with heat as to diminish very perceptibly the size of this vacuity; and as I can find no trace of any portion of the volatile fluid, I have no doubt that this vacuity would disappear by an increased degree of heat. The

fear, however, of bursting so rare and interesting a cavity, has prevented me from making this experiment. The cavity contains a great number of crystals of different forms, not one of which melts with heat, and almost all of which possess double refraction. When I first submitted this cavity to the microscope, there were *five* small crystals lying between D and the vacuity V; one a flat prism, another a hexagonal plate, a third amorphous, and a fourth and fifth two irregular halves of a hexagon. Upon the first application of heat, one or two of these crystals leapt from their resting place, and darted to the opposite side of the cavity. In a few seconds the others quitted their places one after another, performing the most rapid and extraordinary rotations. One crystal joined another, and, at last, four of them thus united revolved with such rapidity as completely to efface their respective shapes. They then separated on the withdrawal of the heat, and took the position which their gravity assigned them. On another occasion, a long flat prism performed the same rotation round its middle point; and I have repeated the experiment so often, in showing it to others, that the small crystals have been driven between the inclined edges of the cavity, from which I cannot extricate them. I have succeeded, however, in conducting a fine octohedral crystal, truncated on its edges and angles, into the arena at D, where I have just seen it perform its rotation, as indicated by the concentric circles on the right-hand of D.

In seeking for the cause of so extraordinary a phænomenon, we are reminded of the rotations of camphor and other volatile substances; but in this case no gas or matter of any kind could be thrown off without becoming visible in the fluid. The pyro-electricity of topaz next suggests itself as a moving power; but though it might produce attractions and repulsions, we cannot see how it could turn a crystal upon its axis. The experiments of Libri and Fresnel, on the repulsions which heated bodies exert upon each other at sensible distances, afford us as little aid. They may enable us to account for the mere displacement of the crystals by the application of heat, or for their sudden start from their places of rest, but they do not supply us with a force fitted to give and to sustain a rapid rotatory movement.

I have already had occasion to state, that the cavities often burst when too much heat is applied to the specimen. This generally takes place by a separation of the laminæ, which fly off in splinters; but when the burst cavity is large and insulated, a piece of the solid crystal is scooped out on its weakest side. Sometimes a great number of cavities explode at the same time, and when they are small, or exist in a part

of the crystal where there are no large ones, the explosive force is not strong enough to separate the laminæ. The fluid is merely driven between the laminæ to a small distance around the cavity, and shows itself as a dark brown powdery matter, encircling the cavity as the burr of a comet does its nucleus. When the cohesion of the laminæ is great, it resists the explosive force over a large cavity, and the contents of the cavity are thrown to a considerable distance around it, and remains between the laminæ, either as a sort of powder, or as a congeries of minute crystals, which are sometimes large enough to show their depolarizing action. When the laminæ separate, we find this crystalline matter either fluid or indurated; exhibiting, when fluid, the extraordinary properties described in my former papers. If we breathe upon the indurated matter it becomes fluid, recrystallizes in new spiculæ and crystals; and, on several occasions, I have found fine examples of circular crystallization.

After the explosion of cavities containing only the dense fluid, I have been surprised to find, and that in large cavities, that no trace of matter was left upon the sides of the cavity or around it. Whether this arose, as the fact seems to indicate, from the dense fluid being a condensed gas, or from some other cause, it will require new experiments to determine.

In a very remarkable specimen, in which the cleavage plane passed through a great number of large flat cavities, the brown matter has been lodged near to the edges of each cavity, and marks them out even to the unassisted eye. These cavities were filled almost solely with the volatile fluid; and since the faces of the cavities are corroded as if by the action of a solvent, developing crystalline forms, there is reason to think that the fluid has exercised this action, and that the phænomenon is analogous to that external action, on the faces of hundreds of Brazil topazes in my possession, which I have described in the *Cambridge Transactions*\*, and the singular optical figure formed by which, I have represented in a late volume of the *Transactions of this Society*†.

The only chemical experiment on the contents of these cavities, which I have had occasion recently to make, is perhaps worth reporting. One angle of a cavity was blown off by its explosion, and though the fluids escaped, a pretty large prismatic crystal remained within the cavity. I introduced *water* and *alcohol* successively into the cavity, and raised them to a considerable heat; but they had no effect in dissolving the crystal.

\* Vol. ii. plate 1. fig. 15.

† *Edinburgh Transactions*, vol. xiv. plate 10. figs. 1, 2.



### 5. On Solid Crystals and Crystalline Masses imbedded in Topaz.

Among the new phænomena which this section embraces, there is at least one intimately connected with the subject of the fluid cavities. How far the other phænomena may have any such connexion, it remains to be seen.

The imbedded crystals to which I refer, presented themselves to me while the specimens which contain them were exposed to polarized light. Mineralogists have been long familiar with the beautiful crystals of titanium, imbedded in quartz, and I have found the same mineral imbedded under still more interesting circumstances in the Brazilian amethysts.

In topaz, however, the imbedded crystals have never been noticed, and I have fortunately obtained specimens, in which they are displayed with singular beauty. Their axes of double refraction are not coincident with those of the topaz; and hence they are seen in the obscure field of the microscope splendent with all the colours of polarized light. These crystals are equally transparent with the topaz, with a few slight exceptions. They sometimes polarize five or six orders of colours; and, in general, they have very beautiful crystalline forms, which can be seen by the microscope in common light. In some cases they are mere crystalline masses, often of a reniform shape, but still with regular axes of double refraction.

In some specimens of Brazil topaz, the crystals occur in branches or groups of singular beauty, consisting of prisms and hexagonal plates, connected apparently by filaments of some opaque matter.

I have occasionally met with another interesting variety of them, which *have no visible outline* by common light, and which could never have been detected but by the polarizing microscope. In one of these cases, the crystalline mass, which is nearly spherical, lies in a crowded group of small fluid cavities, none of which enters its mass; a complete proof that the cavities were formed in the soft mass of topaz, when it encircled the indurated crystal.

Along with these interesting phænomena, another occasionally occurs, which may still require a further examination. I have observed *apparent* doubly refracting crystals, which differ in some essential points from those which have been described. They depolarize a uniform, or nearly a uniform tint, notwithstanding the different thicknesses through which the polarized light passes; and that tint is less brilliant than in the real imbedded crystals. I conceive, therefore, that they are crystallized cavities, having their inner surfaces coated with a doubly

refracting crust. This is, in itself, a very natural supposition, seeing that the fluid may have discharged its gaseous portion, and left behind it the matters which it held in solution. The cavities however, of this kind, which I have described in a former paper, have no depolarizing action; and I find that those now under consideration have regular axes of double refraction. Hence the matter which covers them must be a regular crystalline shell, with optical and crystallographic axes—a phænomenon which has no parallel in mineralogy.

St. Leonard's College, St. Andrew's,  
February 15, 1845.

#### LXXIV. *Observations on Chloric Acid and the Chlorates.*

By LEWIS THOMPSON.

*To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,

**A**N easy and æconomical mode of preparing chloric acid and some of the chlorates has not been described in any chemical work that I am aware of: the following will be found to answer extremely well.

Dissolve in two separate portions of boiling water one atom (122·81) of chlorate of potash, and one atom (168·34) of bitartrate of ammonia; mix the two solutions together, and set the whole aside in order that the bitartrate of potash may crystallize; then mix the clear solution with an equal bulk of alcohol, and filter or pour off the alcoholic solution of chlorate of ammonia, which must now be boiled in a flask or other narrow-necked vessel, with an excess of recently-precipitated carbonate of baryta, until the ammonia is expelled, water being occasionally added; then filter the fluid, evaporate, and crystallize. In dissolving the chlorate of potash and bitartrate of ammonia, as little water must be used as possible.

The chlorates of strontia and lime may be prepared in a similar manner; and the metallic chlorates are easily prepared by decomposing the chlorate of baryta by means of a sulphate of the base required.

Chloric acid is best obtained by dissolving a given weight of chlorate of baryta, and adding no more sulphuric acid than is sufficient to combine with the base; several hours or even days, however, appear necessary to effect this decomposition in the cold; after which the whole may be filtered and carefully evaporated at a low heat. When sulphuric acid is added to a solution of the chlorate of baryta, as long as it gives a precipitate, I have always found an excess of it in the chloric acid.

The bitartrate of ammonia may be readily made by dissolving tartaric acid in water, saturating one-half of the solution with ammonia or its carbonate, and adding to this the remaining half of the liquid tartaric acid; the bitartrate of ammonia immediately precipitates.

For pyrotechnical purposes, the chlorates of baryta, strontia, lead, &c. may be made without alcohol. With combustibles containing hydrogen, the chlorate of baryta produces a green flame of surpassing brilliancy; and the chlorate of strontia, although somewhat deliquescent, is much superior as a crimson to the nitrate of that earth.

I am, Gentlemen,

Your most obedient Servant,

Byker Bar, Newcastle-on-Tyne,  
October 14, 1847.

LEWIS THOMPSON.

LXXV. *On Quaternions; or on a New System of Imaginaries in Algebra.* By Sir WILLIAM ROWAN HAMILTON, LL.D., V.P.R.I.A., F.R.A.S., Corresponding Member of the Institute of France, &c., Andrews' Professor of Astronomy in the University of Dublin, and Royal Astronomer of Ireland.

[Continued from p. 293.]

51. **I**T has been shown\* that if the two symbols  $\iota, \kappa$  denote certain constant vectors, perpendicular to the two cyclic planes of an ellipsoid, and if  $\nu, \tau$  denote two other and variable vectors, of which the former is normal to the ellipsoid at any proposed point upon its surface, while the latter is tangential to a line of curvature at that point, then the *directions* of these four vectors  $\iota, \kappa, \nu, \tau$  are so related to each other as to satisfy the condition†

$$S.\nu\tau\iota\kappa=0 \text{ (49.), article 47;}$$

$S$  being the characteristic of the operation of taking the scalar part of a quaternion. And because the two latter of these four directions, namely the directions of the normal and tangential vectors  $\nu$  and  $\tau$ , are always perpendicular to each other, this additional equation has been seen to hold good:

$$S.\nu\tau=0 \text{ (36.), article 45.}$$

Retaining the same significations of the symbols, and carrying forward for convenience the recent numbering of the formulæ,

\* See the Philosophical Magazine for October 1847; or Proceedings of the Royal Irish Academy for July 1846.

† Inadvertently transcribed as  $S.\nu\tau\iota\kappa\tau=0$ , towards the end of the last communication to this Magazine: but correctly printed in the formula (49.) here referred to.

it is now proposed to point out some of the modes of combining, transforming, and interpreting the system of these two equations, consistently with the principles and rules of the Calculus of Quaternions, from which the equations themselves have been derived.

52. Whatever two vectors may be denoted by  $\iota$  and  $\tau$ , the ternary product  $\tau\iota\tau$  is always a *vector form*, because (by article 20) its scalar part is zero; and on the other hand the square  $\tau^2$  is a pure scalar: therefore we may always write

$$\tau\iota\tau = \mu\tau^2, \quad \tau\iota = \mu\tau, \quad . \quad . \quad . \quad . \quad . \quad (52.)$$

where  $\mu$  is a new vector, expressible in terms of  $\iota$  and  $\tau$  as follows:

$$\mu = \tau\iota\tau^{-1}; \quad . \quad . \quad . \quad . \quad . \quad . \quad (53.)$$

so that it is, in general, by the principles of articles 40, 41, 42, 43, the *reflexion* of the vector  $\iota$  with respect to the vector  $\tau$ , and that thus the direction of  $\tau$  is exactly intermediate between the directions of  $\iota$  and  $\mu$ . In the present question, this new vector  $\mu$ , defined by the equation (53.), may therefore represent the reflexion of the first cyclic normal  $\iota$ , with respect to any reflecting line which is parallel to the vector  $\tau$ , which latter vector is tangential to one of the curves of curvature on the ellipsoid. Substituting for  $\tau\iota\tau$  its value (52.), in the lately cited equation (49.), and suppressing the scalar factor  $\tau^2$ , we find this new equation:

$$S.\nu\mu x = 0; \quad . \quad . \quad . \quad . \quad . \quad . \quad (54.)$$

which, in virtue of the general *equation of coplanarity* assigned in the 21st article (Phil. Mag. for July 1846), expresses that the reflected vector  $\mu$ , the normal vector  $\nu$ , and the second cyclic normal  $x$ , are parallel to one common plane. This result gives already a characteristical geometric property of the lines of curvature on an ellipsoid, from which the directions of those curved lines, or of their tangents ( $\tau$ ), can generally be assigned, at any given point upon the surface, when the direction of the normal ( $\nu$ ) at that point, and those of the two cyclic normals ( $\iota$  and  $x$ ), are known. For it shows that if a straight line  $\mu$  be found, in any plane parallel to the given lines  $\nu$  and  $x$ , such that the bisector  $\tau$  of the angle between this line  $\mu$  and a line parallel to the other given line  $\iota$  shall be perpendicular to the given line  $\nu$ , then this bisecting line  $\tau$  will have the sought direction of a tangent to a line of curvature. But it is possible to deduce a geometrical determination, or construction, more simple and direct than this, by carrying the calculation a little further.



53. The equation (52.) gives

$$(\mu + \iota)\tau = \tau\iota + \iota\tau = V^{-1}0, \quad . \quad . \quad . \quad . \quad (55.)$$

this last symbol  $V^{-1}0$  denoting generally any quaternion of which the vector part vanishes; that is any pure scalar, or in other words any real number, whether positive or negative or null. Hence  $\mu + \iota$  and  $\tau$  denote, in the present question, two coincident or parallel vectors, of which the directions are either exactly similar or else exactly opposite to each other; since if they were inclined at any actual angle, whether acute or right or obtuse, their product would be a quaternion, of which the vector part would not be equal to zero. Accordingly the expression (53.) gives this equation between tensors,

$$T\mu = T\iota; \quad . \quad . \quad . \quad . \quad . \quad . \quad (56.)$$

so that the symbols  $\mu$  and  $\iota$  denote here two equally long straight lines; and therefore one diagonal of the equilateral parallelogram (or rhombus) which is constructed with those lines for two adjacent sides bisects the angle between them. But by the last article, this bisector has the direction of  $\tau$  (or of  $-\tau$ ); and by one of those fundamental principles of the geometrical interpretation of symbols, which are *common* to the calculus of quaternions and to several earlier and some later systems, the symbol  $\mu + \iota$  denotes generally the intermediate diagonal of a parallelogram constructed with the lines denoted by  $\mu$  and  $\iota$  for two adjacent sides: we might therefore in this way also have seen that the vector  $\mu + \iota$  has, in the present question, the direction of  $\pm\tau$ . This vector  $\mu + \iota$  is therefore perpendicular to  $\nu$ , and we have the equation

$$0 = S.\nu(\mu + \iota), \text{ or } S.\nu\mu = -S.\nu\iota. \quad . \quad . \quad . \quad (57.)$$

But by (56.), and by the general rule for the tensor of a product (see art. 20), we have also

$$T.\nu\mu = T.\nu\iota; \quad . \quad . \quad . \quad . \quad . \quad (58.)$$

and in general (by art. 19), the square of the tensor of a quaternion is equal to the square of the scalar part, minus the square of the vector part of that quaternion; or in symbols (Phil. Mag., July 1846),

$$(TQ)^2 = (SQ)^2 - (VQ)^2.$$

Hence the two quaternions  $\nu\mu$  and  $\nu\iota$ , since they have equal tensors and opposite scalar parts, must have the squares of their vector parts equal, and those vector parts themselves must have their tensors equal to each other; that is, we may write

$$(V.\nu\mu)^2 = (V.\nu\iota)^2, \quad TV.\nu\mu = TV.\nu\iota: \quad . \quad . \quad (59.)$$

and may regard these two vector parts of these two quaternions, or of the products  $\nu\mu$  and  $\nu i$ , as denoting two equally long straight lines. Consequently the vector  $\pm \nu\tau$ , which has the direction of the line represented by the pure vector product  $\nu(\mu + i)$ , or by the sum  $V.\nu\mu + V.\nu i$  of two equally long vectors, has at the same time the direction of the sum of the two corresponding versors of those vectors, or that of the sum of their *vector-units*; so that we may write the equation

$$t\nu\tau = UV.\nu\mu + UV.\nu i, \quad . \quad . \quad . \quad . \quad . \quad (60.)$$

where  $U$  is (as in art. 19) the characteristic of the operation of taking the versor of a quaternion, or of a vector; and  $t$  is a scalar coefficient. Again, the equation  $0 = S.\nu\mu x$ , (54.), which expresses that the three vectors  $\nu$ ,  $\mu$ ,  $x$  are coplanar, shows also that the two vectors  $V.\nu\mu$  and  $V.\nu x$  are parallel to each other, as being both perpendicular to that common plane to which  $\nu$ ,  $\mu$ , and  $x$  are parallel; hence we have the following equation between two versors of vectors, or between two vector-units,

$$UV.\nu\mu = \pm UV.\nu x; \quad . \quad . \quad . \quad . \quad . \quad (61.)$$

and therefore instead of the formula (60.) we may write

$$t\tau = \nu^{-1}UV.\nu i \pm \nu^{-1}UV.\nu x. \quad . \quad . \quad . \quad . \quad (62.)$$

In this expression for a vector touching a line of curvature, or parallel to such a tangent, the two terms connected by the sign  $\pm$  are easily seen to denote (on the principles of the present calculus) two equally long vectors, in the directions respectively of the projections of the two cyclic normals  $i$  and  $x$  on a plane perpendicular to  $\nu$ ; that is, on the tangent plane to the ellipsoid at the proposed point, or on any plane parallel thereto. If then we draw two straight lines through the point of contact, bisecting the acute and obtuse angles which will in general be formed at that point by the projections on the tangent plane of two indefinite lines drawn through the same point in the directions of the two cyclic normals, or in directions perpendicular to the two planes of circular section of the surface, *the two rectangular bisectors of angles, so obtained, will be the tangents to the two lines of curvature*: which very simple construction agrees perfectly with known geometrical results, as will be more clearly seen, when it is slightly transformed as follows.

54. If we multiply either of the two tangential vectors  $\tau$  by the normal vector  $\nu$ , the product of these two rectangular vectors will be, by one of the fundamental and *peculiar*\* princi-

\* See the author's Letter of October 17, 1843, to John T. Graves, Esq., printed in the Supplementary Number of the Philosophical Magazine for December 1844: in which Letter, the three fundamental symbols  $i$ ,  $j$ ,  $k$  were what it has been since proposed to name *direction-units*.

ples of the calculus of quaternions, a third vector rectangular to both; we shall therefore only pass by this multiplication, so far as *directions* are concerned, from one to the other of the tangents of the two lines of curvature: consequently we may omit the factor  $v^{-1}$  in the second member of (62.), at least if we change (for greater facility of comparison of the results among themselves) the ambiguous sign  $\pm$  to its opposite. We may also suppress the scalar coefficient  $t$ , if we only wish to form an expression for a line  $\tau$  which shall have the required *direction* of a tangent, without obliging the *length* of this line  $\tau$  to take any previously chosen value. The formula for the system of the two tangents to the two lines of curvature thus takes the simplified form:

$$\tau = UV.vi \mp UV.vx; \quad . \quad . \quad . \quad . \quad . \quad (63.)$$

in which the two terms connected by the sign  $\mp$  are two vector-units, in the respective directions of the traces of the two cyclic planes upon the tangent plane. The tangents to the two lines of curvature at any point of the surface of an ellipsoid (and the same result holds good also for other surfaces of the second order), are therefore parallel to the two rectangular straight lines which bisect the angles between those traces; or they are themselves the bisectors of the angles made at the point of contact by the traces of planes parallel to the two cyclic planes. The discovery of this remarkable geometrical theorem appears to be due to M. Chasles. It is only brought forward here for the sake of the *process* by which it has been above deduced (and by which the writer was in fact led to perceive the theorem before he was aware that it was already known), through an application of the method of quaternions, and as a corollary from the geometrical construction of the ellipsoid itself to which that method conducted him\*. For that new geometrical *construction* has been shown (in a recent Number of this Magazine) to admit of being easily *retranslated* into that quaternion form of the *equation*† of the ellipsoid, namely

$$T(ip + px) = x^2 - i^2, \text{ equation (9.), art. (38.),}$$

as an *interpretation* of which equation it had been assigned by the present writer; and then a *general* method for investigating by quaternions the directions of the lines of curvature on *any* curved surface whatever, conducts, as has been shown (in

\* See the Numbers of the Philosophical Magazine for June, September, and October 1847; or the Proceedings of the Royal Irish Academy for July 1846.

† Another very simple construction, derived from the same quaternion equation, and serving to generate, by a moving sphere, a system of *two* reciprocal ellipsoids, will be given in an early Number of this Magazine.

articles 46 and 47), to the equation of those lines for the ellipsoid,

$$S.v\tau\iota x = 0 \quad (49.);$$

from which, when combined with the general equation  $S.v\tau = 0$ , the formula (63.) has been deduced, and geometrically interpreted as above.

55. Another mode of investigating generally the directions of those tangential vectors  $\tau$  which satisfy the system of the two conditions in art. 51, may be derived from observing that those conditions fail to distinguish one such tangential vector from another in each of the two cases where the variable normal  $v$  coincides in direction with either of the two fixed cyclic normals,  $\iota$  and  $x$ ; that is, at the four *umbilical points* of the ellipsoid, as might have been expected from the known properties of that surface. In fact if we suppose

$$v = m\iota, \quad S.\iota\tau = 0, \quad . \quad . \quad . \quad . \quad . \quad (64.)$$

where  $m$  is a scalar coefficient, that is if we attend to either of those two opposite *umbilics* at which  $v$  has the direction of  $\iota$ , we find the value

$$v\tau\iota\tau x = m(\iota\tau)^2 x, \quad . \quad . \quad . \quad . \quad . \quad (65.)$$

which is here a vector-form, because by (64.) the product  $\iota\tau$  denotes in this case a *pure vector*, so that *its square (like that of every other vector in this theory) will be a negative scalar*, by one of the fundamental and *peculiar\** principles of the present calculus; the scalar part of the product  $v\tau\iota\tau x$  therefore vanishes, or the condition (49.) is satisfied by the suppositions (64.). Again, if we suppose

$$v = m'x, \quad . \quad . \quad . \quad . \quad . \quad . \quad (66.)$$

$m'$  being another scalar coefficient, that is if we consider either of those two other opposite umbilics at which  $v$  has the direction of  $x$ , we are conducted to this other expression,

$$v\tau\iota\tau x = m'x\tau\iota\tau x; \quad . \quad . \quad . \quad . \quad . \quad . \quad (67.)$$

which also is a vector-form, by the principles of the 20th article. In this manner we may be led to see that if in general we decompose, by orthogonal projections, each of the two cyclic normals,  $\iota$  and  $x$ , into two partial or component vectors,  $\iota'$ ,  $\iota''$ , and  $x'$ ,  $x''$ , of which  $\iota'$  and  $x'$  shall be tangential to the surface, or perpendicular to the variable normal  $v$ , but  $\iota''$  and  $x''$  parallel to that normal, in such a manner as to satisfy the two sets of equations,

$$\left. \begin{aligned} \iota &= \iota' + \iota''; & S.\iota'v &= 0; & V.\iota''v &= 0; \\ x &= x' + x''; & S.x'v &= 0; & V.x''v &= 0; \end{aligned} \right\} \quad . \quad . \quad (68.)$$

\* See the author's letter of October 17, 1843, already cited in a note to article 54.



then, on substituting these values for  $i$  and  $x$  in the condition (49.), or in the equation  $0 = S. \nu \tau i \tau x$ , the terms involving  $i''$  and  $x''$  will vanish of themselves, and the equation to be satisfied will become

$$0 = S. \nu \tau i' \tau x'; \quad . \quad . \quad . \quad . \quad . \quad (69.)$$

which is thus far a simplified form of the equation (49.), that three of the four directions to be compared (namely those of  $i'$ ,  $x'$ , and  $\tau$ ) are now parallel to one common plane, namely to the plane which touches the ellipsoid at the proposed point, and to which the fourth direction (that of  $\nu$ ) is perpendicular. Decomposing the two quaternion products,  $\tau i'$  and  $\tau x'$ , into their respective scalar and vector parts, by the general formulæ,

$$\left. \begin{aligned} \tau i' &= S. \tau i' + V. \tau i'; \\ \tau x' &= S. \tau x' + V. \tau x'; \end{aligned} \right\} \quad . \quad . \quad . \quad . \quad . \quad (70.)$$

and observing that the vectors  $V. \tau i'$  and  $V. \tau x'$  both represent lines parallel to  $\nu$ , because  $\nu$  is perpendicular to the common plane of  $\tau, i', x'$ ; so that the three following binary products,  $V. \tau i'. V. \tau x'$ ,  $\nu V. \tau i'$ ,  $\nu V. \tau x'$ , are in the present question scalars; we find that we may write

$$S. \nu \tau i' \tau x' = \nu S. \tau i'. V. \tau x' + \nu V. \tau i'. S. \tau x'. \quad . \quad . \quad (71.)$$

Hence the equation (69.) or (49.) reduces itself, after being multiplied by  $\nu^{-1}$ , to the form

$$S. \tau i'. V. \tau x' + V. \tau i'. S. \tau x' = 0; \quad . \quad . \quad . \quad . \quad (72.)$$

which gives, in general, by the rules of the present calculus,

$$\frac{V. i' \tau}{S. i' \tau} = \frac{V. \tau x'}{S. \tau x'}; \quad . \quad . \quad . \quad . \quad . \quad (73.)$$

and by another transformation,

$$\frac{V. i' \tau^{-1}}{[S. i' \tau^{-1}]} = - \frac{V. x' \tau^{-1}}{S. x' \tau^{-1}}; \quad . \quad . \quad . \quad . \quad . \quad (74.)$$

which may perhaps be not inconveniently written also thus:

$$\frac{V}{S} \cdot \frac{i'}{\tau} = - \frac{V}{S} \cdot \frac{x'}{\tau}; \quad . \quad . \quad . \quad . \quad . \quad (75.)$$

in using which abridged notation, we must be careful to remember, respecting the characteristic  $\frac{V}{S}$ , of which the effect is to form or to denote the *quotient of the vector part divided by the scalar part* of any quaternion expression to which it is prefixed, that *this new characteristic of operation is not* (like  $S$  and  $V$  themselves) *distributive relatively to the operand*. The vector denoted by the first member of (74.) or of (75.) is a line perpendicular to the plane of  $i'$  and  $\tau$ , that is to the tangent

plane of the ellipsoid; and its length is the trigonometric tangent of the angle of rotation in that plane from the direction of the line  $\tau$  to that of the line  $i'$ ; while a similar interpretation applies to the second member of either of the same two equations, the sign — in that second member signifying here that the two equally long angular motions, or rotations, from  $\tau$  to  $i'$ , and from  $\tau$  to  $x'$ , are performed in opposite directions. Thus the vector  $\tau$ , which touches a line of curvature, coincides in direction with the bisector of the angle in the tangent plane between the projections,  $i'$  and  $x'$ , of the cyclic normals thereupon; or with that other line, at right angles to this last bisector, which bisects in like manner the other and supplementary angle in the same tangent plane, between the directions of  $i'$  and  $-x'$ : since  $x'$  may be changed to  $-x'$ , without altering essentially any one of the four last equations between  $\tau, i', x'$ . Those two rectangular and known directions of the tangents to the lines of curvature at any point of an ellipsoid, which were obtained by the process of article 53, are therefore obtained also by the process of the present article; which conducts, by the help of the geometrical reasoning above indicated, to the following expression for the system of those two tangents  $\tau$ , as the symbolical solution (in the language of the present calculus) of any one of the four last equations (72.),..(75.):

$$\tau = t'(U i' \pm U x'); \quad . \quad . \quad . \quad . \quad . \quad (76.)$$

where  $t'$  is a scalar coefficient.

The agreement of this symbolical result with that marked (62.) may be made evident by observing that the equations (68.) give

$$i' = v^{-1} V.vi; \quad x' = v^{-1} V.vx; \quad . \quad . \quad . \quad . \quad . \quad (77.)$$

so that if we establish, as we may, the relation

$$t t' = (T v)^{-1}, \quad . \quad . \quad . \quad . \quad . \quad (78.)$$

between the arbitrary scalar coefficients  $t$  and  $t'$ , which enter into the formulæ (62.) and (76.), those formulæ will coincide with each other. And to show, without introducing geometrical considerations, that (for example) the form (73.) of the recent condition relatively to  $\tau$  is symbolically satisfied by the expression (76.), we may remark that this expression, when operated upon according to the *general rules* of this calculus, gives

$$\left. \begin{aligned} T x'. V. i' \tau &= \pm t' V. i' x'; & T x'. S. i' \tau &= t' (-T. i' x' \pm S. i' x'); \\ T i'. V. \tau x' &= t' V. i' x'; & T i'. S. \tau x' &= t' (S. i' x' \mp T. i' x'); \end{aligned} \right\} \quad (79.)$$

and that therefore the two members of (73.) do in fact receive,

in virtue of (76.), one common symbolical value, namely one or other of the two which are included in the ambiguous form

$$\frac{V.'x'}{S.'x' \mp T.'x'}; \dots \dots \dots (80.)$$

respecting which form it may not be useless to remark that the product of its two values is unity.

[To be continued.]

LXXVI. *Contributions to the Chemical History of Gun-Cotton and Xyloidine.* By Mr. JOHN HALL GLADSTONE, of University College, London\*.

AT the commencement of the present year, having perceived that considerable doubt rested on the ultimate composition of gun-cotton, I undertook a series of experiments with a view to ascertain it, if possible; and during my investigation my attention was drawn to various papers that appeared on the subject, where I found contradictory accounts, not only of the results of analysis, but also of the action of various reputed solvents. The experiments detailed below, although they are far from exhausting the subject, may serve to explain some of these anomalies, and to point out a few facts, which, as far as I have been able to learn, have not been hitherto noticed.

The cotton employed was that used by jewellers, well-carded, perfectly white, and free from imperfections. An analysis of the substance by combustion with oxide of copper in a stream of oxygen yielded the following results:—

Cotton employed . . . . .	3·16 grs.
Carbonic acid produced . . . .	5·14 ...
Water produced . . . . .	2·06 ...

These proportions are,—

Carbon . . . .	44·37
Hydrogen . . .	7·24
Oxygen . . . .	48·39
	<hr/> 100·00

Lignine calculated from the formula  $C_{34}H_{20}O_{20}$ :—

Carbon . . . .	44·44
Hydrogen . . .	6·17
Oxygen . . . .	49·39
	<hr/> 100·00

The excess of hydrogen doubtless arises from moisture absorbed by the oxide of copper during the unavoidable delay in mixing it with the cotton.

\* Communicated by the Chemical Society; having been read June 7, 1847.

This cotton, which may be considered as pure lignine, was steeped until thoroughly wetted in a mixture of nitric acid of spec. grav. 1.502, and nearly an equal bulk of strong sulphuric acid, then well-washed with water, and dried at a temperature not exceeding  $212^{\circ}$ . In one instance 38.38 grs. of cotton became 66.84 grs., being an increase of 28.46 grs., or 74.15 per cent. In a second experiment 59.3 grs. of cotton gave an increase of 43.7 grs., or 73.7 per cent. The gun-cotton, or pyroxyline, thus produced resembled the original cotton in physical properties very closely, and exploded at about  $370^{\circ}$ , producing no smoke and leaving no residue.

The action of various solvents and reagents upon this substance was found to be as follows:—It is absolutely insoluble in pure water, and nearly so in strong alcohol, æther, whether hydrated or anhydrous, and in a mixture of æther with  $\frac{1}{10}$ th part of alcohol; but acetic æther instantly destroys its fibre, and dissolves it in large quantity. The solution yields on spontaneous evaporation a white powder of the same weight as the original pyroxyline, but I have found it very difficult to drive off the last traces of the solvent. The action of sulphuric acid upon it differs from that exerted upon unaltered cotton; for, while the latter is instantly dissolved by the strong acid, and charred upon a slight elevation of temperature, pyroxyline dissolves with difficulty unless the acid be warmed, evolving at the same time nitric oxide and other gases, and not being charred even upon boiling. With the aid of heat it dissolves immediately in a solution of potash. By means of these three last-mentioned tests I was able to prove the absence of any unaltered cotton in the product under examination. The action of other reagents upon gun-cotton was not so decided; it was dissolved, but not without long boiling, by ammonia, the alkaline carbonates, hydrochloric acid, acetic acid, both glacial and dilute, and weak sulphuric acid. These solutions, as well as the two preceding, contained nitric acid; nothing could be precipitated from them by dilution or neutralization; and when evaporated they yielded only a dark brown amorphous matter. It is evident that none of these reagents restore the lignine in its original condition; and they do not afford any means of ascertaining whether the compound contains the elements of nitric or hyponitric acid.

As there exists a great discrepancy in the accounts given of the increase of weight in making gun-cotton, I examined whether the length of time it was immersed in the acid liquor, or the proportions of the acids employed, were the cause. The length of immersion I found to produce no alteration; but



upon employing two measures of sulphuric acid to one of nitric acid, I obtained a product resembling in all respects ordinary pyroxyline, yet 42.77 grs. gave an increase of only 24.31 grs., or 56.84 per cent. Upon a repetition of this experiment I found the increase to be 59.93 per cent., and again 70.6 per cent. Suspecting from the disparity of these results that something might be dissolved in the acid liquor, I immersed 6.7 grs. of cotton in a large quantity of the mixed acids, but it increased 4.9 grs., or 73.1 per cent. Perceiving that I had obtained an opposite effect to that anticipated, I treated 12.64 grs. of cotton with just sufficient of the mixture to wet it thoroughly: the fibre was evidently somewhat destroyed; the increase in weight was only 6.54 grs., or 51.74 per cent., and the acid liquor squeezed from the cotton, neutralized with ammonia, evaporated to dryness, and heated, gave abundant evidence of organic matter being present. Lest however it might be supposed that the whole had not been converted into pyroxyline, it was treated again with the mixed acids, but that produced an increase of only 0.12 gr. The action of various solvents confirmed its identity with ordinary pyroxyline, while its solubility in potash proved that the transformation had been very nearly complete. A repetition of the experiment gave similar results. It thus appears that the small increase in weight in the preparation of pyroxyline takes place when there is not sufficient nitric acid present to prevent the peculiar action of the sulphuric acid, namely, that of dissolving and altering it. When however the increase amounted to about 74 per cent., I was never able to detect the presence of oxalic acid or other organic matter in the acid liquor; and as no gas is evolved during the preparation of pyroxyline, it may be concluded that there is no secondary product containing carbon.

Subsequently, when Dr. Schönbein had specified *his* method of making gun-cotton, I treated 18.78 grs. of cotton with a mixture of three parts of sulphuric acid and one of nitric acid, sp. gr. 1.5, following his directions. The result was 32.92 grs. of a substance similar to that produced in former experiments, being an increase of 75.20 per cent. On another occasion 80.95 grs. of cotton gave an increase of 61.10 grs., or 75.47 per cent. The action of solvents and reagents confirmed the identity of this pyroxyline with that obtained in my previous experiments, and I was equally able to establish the absence of any secondary product containing carbon.

In determining the ultimate composition of pyroxyline several precautions were found to be necessary. In the ana-

lyses recorded below it was cut into small pieces, and, after the weight was taken, mixed carefully with oxide of copper. To prevent its caking together the admixture of a little asbestos was found useful. This was introduced into a long combustion-tube, then some fresh oxide of copper, and upon it again some fused into lumps so as to fill the whole bore for about 7 inches. Lastly, was added a mixture of copper turnings and reduced copper for about 9 inches. The combustion conducted cautiously in the usual manner gave the following results; the pyroxyline burnt in the sixth experiment having been prepared by Schönbein's method.

	I.	II.	III.	IV.	V.	VI.
Pyroxyline employed	4.09	4.61	3.57	4.85	4.55	2.905
Carb. acid produced	4.20	4.52	3.42	4.88	...	2.84
Water produced . .	1.19	1.36	...	...	1.34	0.87

Hence in 100 parts,—

	I.	II.	III.	IV.	V.	VI.
Carbon .	27.90	26.74	26.10	27.44	...	26.65
Hydrogen	3.22	3.27	...	...	3.27	3.32

In order to determine the amount of nitrogen the differential mode was adopted, as the method of MM. Will and Varrentrapp is inapplicable to substances containing this element in so highly oxidized a state. The same precautions were taken as in the estimation of carbon; and the collected gases gave the following results after due correction for barometrical pressure:—

	I.	II.	Another specimen.
Carbonic acid .	25.0	38.5	23.9
Nitrogen . . .	5.5	8.5	5.1

These proportions are,—

Nitrogen.	Carbonic acid.
1	4.55
1	4.53
1	4.68

The volumes of the gases represent respectively equivalents of carbon and nitrogen, and since no secondary product is formed in the conversion of lignine into pyroxyline, the 24 equivalents of carbon in the former must be found in the latter. This will give the following ratio in equivalents of carbon and nitrogen according to the three experiments above cited:—

	I.	II.	III.
Carbon . . .	24.0	24.0	24.0
Nitrogen . . .	5.28	5.3	5.12

or 24 : 5, which accords with the proportions assigned by M. Pelouze\*.

The formula which best agrees with these results is the following :— $C_{24} \left\{ \begin{smallmatrix} H_{15} \\ 5 NO_4 \end{smallmatrix} \right\} O_{20}$ , which reckoned to 100 parts, gives—

Carbon . . . . .	26·23
Hydrogen . . . . .	2·73
Nitrogen . . . . .	12·75
Oxygen . . . . .	58·29

In order to compare pyroxyline with xyloidine, I treated starch with fuming nitric acid until the whole was converted into a gelatinous mass. The addition of water then threw down a white powder, which was subsequently well-washed and dried. The iodine test proved the absence of all unaltered starch. The xyloidine thus obtained explodes at about 360°, leaving a carbonaceous residue. It is slightly soluble in æther, with which it is capable of forming a peculiar compound not yet investigated ; more so in alcohol, but most of all in æther mixed with a small proportion of alcohol, or in acetic æther. It is dissolved by strong sulphuric acid without the aid of heat, and by boiling solutions of potash, ammonia, hydrochloric acid and dilute sulphuric acid. These solutions contain nitric acid, and nothing is precipitated from them by dilution or neutralization. Xyloidine is also soluble in strong acetic acid, or in nitric acid, whether fuming or of sp. gr. 1·25, but is reprecipitated from either by dilution.

It was also found that nitric acid of ordinary strength (sp. gr. 1·45) answered equally well in the preparation of this substance ; but when acid of sp. gr. 1·41 was employed no such result was obtained. Starch treated with a mixture of equal measures of nitric and sulphuric acids produced a substance of greater combustibility, and more closely resembling pyroxyline, but differing from it in being soluble in glacial acetic acid, and in a mixture of æther with one-tenth part of alcohol, as also in the action that acetic æther exerts upon it. Xyloidine also when subjected to the mixed acids gave a product identical with the above, as far at least as the action of solvents can prove.

Xyloidine burnt by means of oxide of copper, with the usual precautions, gave the following results. The substance employed in the third experiment was made from arrow-root.

\* *Comptes Rendus*, Jan. 4.

	I.	II.	III.
Xyloidine employed . .	4.77	5.23	6.75
Carbonic acid produced .	5.30	5.91	7.87
Water produced . . .	1.84	1.96	2.80

Hence in 100 parts,—

	I.	II.	III.
Carbon . .	30.30	30.82	31.79
Hydrogen .	4.28	4.16	4.60

In the determination of nitrogen by the differential method the proportions of the gases obtained were,—

	I.	II.	III.
Carbonic acid .	70.7	53.4	53.8
Nitrogen . . .	10.6	6.9	8.0

These are in the proportion of—

	I.	II.	III.
Carbon . .	24.0	24.0	24.0
Nitrogen . .	3.59	3.10	3.57

These numbers suggest the simple substitution product  $C_{24}\left\{\begin{smallmatrix} H_{17} \\ 3NO_4 \end{smallmatrix}\right\}O_{20}$ , in which the per-centage of carbon would be 31.37, and of hydrogen 3.70; yet the amount of nitrogen is somewhat too great, and there is far from being sufficient evidence to prove the definiteness of the substance itself. The wide difference also in the results obtained by various chemists can scarcely be accounted for, except upon the supposition that they have operated upon very different substances.

The solubility of xyloidine in nitric acid led me to examine whether any alteration could be effected upon pyroxyline by similar means. The most dilute acid which I found to have any effect upon it in the cold was that of sp. gr. 1.414; but the alteration took place by means of this only after long standing, and but to a slight extent. Nitric acid of sp. gr. 1.45 however is capable of dissolving pyroxyline, and alters both its composition and properties, as will be presently described; whilst fuming nitric acid has not the slightest effect upon it. The new product just mentioned is acted upon somewhat differently by various solvents, according to whether it exists in a fibrous condition, or in powder as precipitated from solution; yet I have found by experiment that no alteration in weight is effected by this change of condition. When in fibre it is slightly soluble in strong alcohol, æther, a mixture of æther with one-tenth part of alcohol, and acetic æther; but when in the pulverulent state it is very soluble in these menstrua, and in glacial acetic acid. In either con-



dition it leaves a carbonaceous residue on combustion, is dissolved by nitric acid, whether of sp. gr. 1.25 or 1.5, and reprecipitated upon dilution. Strong sulphuric acid also dissolves it in the cold, and chars it at a temperature below 180°. These two last properties show that the original pyroxyline was perfectly free from admixture with this new substance.

There occurs a considerable decrease of weight through this transformation. In the first experiment 32 grs. of substance operated upon gave 25.82 grs. of the new product; in the second 43.64 grs. of the one yielded 34.68 of the other. Now assuming the increase in the preparation of pyroxyline to be 75 per cent., the weight of the new product above that of the original cotton would be, as calculated from these figures, 41.1 and 39.05 per cent.

When this new product, whether in the fibrous or the pulverulent condition, was treated with a mixture of equal parts of nitric and sulphuric acids, it increased considerably in weight, and the resulting substance had all the properties of pyroxyline as prepared in the usual manner. 11.16 grs. of the one yielded 13.56 grs. of the other; the quantity that should theoretically have been obtained, calculating it from the decrease in making the new product, is 13.84 or 14.04 grs. Again, 12.35 grs. of the substance as precipitated from solution gave 15.75 grs., the theoretical amount would have been 15.31 or 15.54 grs. This result proves the distinctness of the new product from xyloidine, a fact that could not have been ascertained from the action of the before-mentioned solvents.

Whilst engaged in obtaining these results, I also examined the action of nitric acid of various degrees of strength upon pure cotton. By treating it with nitric acid of sp. gr. 1.5 I obtained a product evidently different from gun-cotton, but as it did not appear to be homogeneous throughout, I passed on to investigate the action of a weaker acid. That of sp. gr. 1.45 gave a substance which proved to be identical with the product of the action of the same acid upon pyroxyline. Upon a repetition of the experiment 68.54 grs. increased in weight 14.61 grs., or 21.31 per cent.—a smaller increase than might have been anticipated, but which may easily be accounted for by the fact that the whole cotton had not been transformed, as was proved by a considerable portion being left undissolved by a boiling solution of potash. Nitric acid of sp. gr. 1.414 produced the same alteration, but only to a small extent, and after long standing. 23.75 grs. of cotton soaked in nitric acid of sp. gr. 1.516 became a hard mass,

and increased in weight 13·49 grs., or 56·8 per cent.; the action of various solvents upon the resulting substance indicated that it was a mixture of pyroxyline and the new product. On another occasion, when the transformation by means of nitric acid sp. gr. 1·47 proved to be complete, 29·52 grs. of cotton increased 9·51 grs., or 32·89 per cent. But in order to obtain a substance sufficiently pure for analysis 16·29 grs. of cotton were treated with enough nitric acid to dissolve the whole; the new product was precipitated by dilution, and the increase in weight was found to be 5·34 grs., or 32·78 per cent. In these instances there occurred a secondary product containing carbon not precipitable by water.

When this was subjected to combustion with oxide of copper, the following results were obtained:—

	I.	II.	Another specimen.
Substance employed .	3·15	2·985	3·165
Carbonic acid produced	3·58	3·39	3·55
Water produced . . .	1·00	1·01	1·14

Hence in 100 parts,—

Carbon .	30·99	30·97	30·59
Hydrogen	3·52	3·75	4·00

I was unable to obtain any very accurate estimation of nitrogen by the differential method: the results most to be depended upon were—

Carbonic acid . . .	120·7	76·7
Nitrogen . . . . .	13·6	8·3

In the proportion of

Carbon . . . . .	24·0	24·0
Nitrogen . . . . .	2·7	2·6

These numbers lead me to think that there are 3 equivalents of nitrogen in the compound, especially as I observed during the combustion that the substance became charred even 1 or 2 inches beyond the glowing charcoal, which will account for the deficiency of nitrogen when compared with the carbonic acid. Hence the composition of the new product coincides very nearly with that calculated from the

formula  $C_{24} \left\{ \begin{matrix} H_{17} \\ 3NO_4 \end{matrix} \right\} O_{20}$ , namely,

Carbon . . . . .	31·37
Hydrogen . . . . .	3·70
Nitrogen . . . . .	9·15
Oxygen . . . . .	55·78
	<hr/> 100·00

Under this supposition the increase in weight in the preparation would be 41·66 per cent.; very similar to that calculated from the results obtained by the action of nitric acid, sp. gr. 1·45, on pyroxyline, namely, 39·05 and 41·1 per cent.

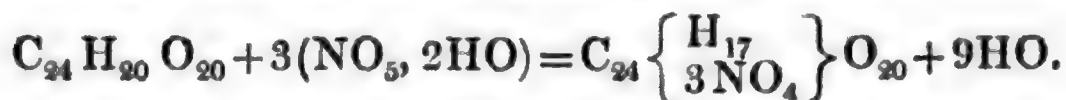
In order to add an additional proof of the identity of the two substances obtained by the action of nitric acid of sp. gr. 1·45 on cotton and on pyroxyline, and also of the fact that pyroxyline is reproduced by the action of mixed sulphuric and nitric acids upon the new product, the experiment was repeated with a portion of the substance made from pure cotton: the result was pyroxyline. In the transformation 26·56 grs. became 38·04: now these 26·56 grs. were produced from 21·81 grs. of the original cotton; hence the increase upon the cotton itself would be 16·23 grs., or 74·4 per cent., coinciding with the amount usually obtained in the preparation of pyroxyline.

I. From these results it appears that in the treatment of woody fibre by nitric acid raised to its highest degree of strength by the addition of sulphuric acid, 5 equivalents of the acid combine with 1 of lignine to produce pyroxyline, displacing 5 equivalents of the elements of water, as indicated by the formula  $C_{24} \left\{ \begin{smallmatrix} H_{15} \\ 5NO_4 \end{smallmatrix} \right\} O_{20}$ . The amount per cent. of carbon and hydrogen hence deduced closely agrees also with that assigned by Mr. Ransome\* and M. Pettenkofer†.

	Calculated.	Ransome.	Pettenkofer.
Carbon . .	26·23	26·28	26·26
Hydrogen .	2·73	3·16	2·75

In this case the synthetical experiment would give an increase of 69·44 per cent.—nearly the amount obtained in the best experiments. My own analyses however have yielded a somewhat larger amount of carbon.

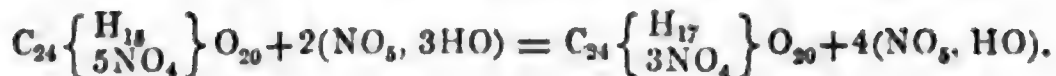
II. If lignine be treated with nitric acid combined with more than 1 equivalent of water, another compound is produced, containing a smaller proportion of the elements of nitric acid, most probably  $C_{24} \left\{ \begin{smallmatrix} H_{17} \\ 3NO_4 \end{smallmatrix} \right\} O_{20}$ , and very closely resembling, but not identical with, pyroxyline.



Also if pyroxyline itself be treated with nitric acid containing 3 equivalents of water, the same compound results:

\* Phil. Mag., January 1847.

† Pharmaceutisches Central Blatt, Dec. 30th, 1846.



And this transformation may be reversed.

Whilst completing my examination of this substance, my attention was drawn to the communication of M. Payen in the *Comptes Rendus* of Jan. 25th, where some properties of "coton hypoazotique" are described. It is possibly the same; yet, in order to express its distinctness from pyroxyline, I would propose as the appellation of my substance cotton-xyloidine.

Before concluding I would acknowledge my obligations to several chemists whose published investigations on the same subject have suggested many of my experiments, and more particularly to Professor Fownes for the valuable advice with which from time to time he has favoured me.

## LXXVII. *Proceedings of Learned Societies.*

### ROYAL ASTRONOMICAL SOCIETY.

[Continued from p. 389.]

June 11, **O**N the Opinion of Copernicus with respect to the 1847. Light of the Planets. By Professor De Morgan.

The common story is, that Copernicus, on being opposed by the argument that Mercury and Venus did not show phases, answered that the phases would be discovered some day. The first place in which I find this story is in Keill's Lectures. It is also given by Dr. Smith, in his well-known Treatise on Optics, by Bailli, and by others. But I cannot find it mentioned either by Melchior Adam or Gassendil, in their biographies of Copernicus; nor by Rheticus, in his celebrated *Narratio*, descriptive of the system of Copernicus; nor by Kepler, nor by Riccioli, in their collections of arguments for and against the heliocentric theory; nor by Galileo, when announcing and commenting on the discovery of the phases; and, what is most to the purpose, Müller, in his excellent edition of the great work of Copernicus, when referring to the discovery of the phases of Venus, as made since, and unknown to, Copernicus, does not say a word on any prediction or opinion of the latter.

This story may then be rejected, as the gossip of a time posterior to Copernicus. If we try to examine what the opinion of Copernicus on this matter really was, a point of some little curiosity arises. It depends on one word, whether he did or did not assert his belief in one or other of these two opinions,—that the planets shine by their own light, or that they are saturated by the solar light, which, as it were, soaks through them. I support the affirmative: that is to say, I hold it sufficiently certain that Copernicus did express himself to the effect that one or the other of these suppositions was the truth.

If we take the first edition of the work *De Revolutionibus*, which



was printed from the manuscript furnished by Copernicus himself, there is little doubt about the matter. There are but two passages which bear or can bear upon the question. The first is in the *ad lectorem*, in which the writer (Osiander, though even Delambre make him Copernicus) asks whether any one acquainted with geometry or optics can receive the Ptolemaic epicycle then used to explain the motion in longitude of Venus? But the meaning of the allusion to optics is explained in the next sentence, by a reference (and by no means a fortunate one) to the changes of apparent diameter of Venus derived from that epicycle; changes which, as they made the perigeon diameter more than four times as great as the apogean, were assured to be falsified by common experience. The second passage is the one on which this discussion must turn. In book i. chap. x., after noting that some had theretofore believed Mercury and Venus to come between the earth and sun, he mentions the difficulty arising from the absence of the remarkable phase, which we now call the transit over the sun's disc. He describes the opinion just mentioned favourably, referring, not to his own view, but to that of those others who had held it. This is not an uncommon idiom: persons advocating an unpopular opinion are very apt to describe the maintainers of it in the third person, though themselves be of the number. But when he comes to describe what he takes to be the necessary consequence of the opinion, he lapses into the first person as follows:—"Non ergo fatemur in stellis opacitatem esse aliquam lunari similem, sed vel proprio lumine, vel solari totis imbutas corporibus fulgere, et idcirco solem non impediri . . . ."

These are the words of the first edition (Nuremberg, 1543). That Copernicus could have answered any objection, either by word or writing, is impossible, since he drew his last breath within a few hours of the time when, not able to open it from weakness, he saw the first printed copy. The second edition (Basle, 1566) is usually said to have been edited by Rheticus. The reason of this is that the name of Rheticus appears in the title-page. But this appearance only arises from the *Narratio*, &c. of Rheticus being added to the edition; and it is only the description of this edition which brings Rheticus into the title-page. There is no mark whatever of his having been the editor; and as the work was printed at Basle, where I cannot find that Rheticus ever sojourned, and as the latter was deeply engaged at the time in his enormous trigonometrical calculation, some proof of his editorship must be given before it is admitted. As the point is of importance, I will notice, that unless Rheticus had made some stay at Basle, it is very unlikely he should have edited a work printed there. He did not edit the first edition, only because it was found convenient to print it at Nuremberg instead of at Wittemberg; and it was accordingly entrusted to Osiander. Now, if ever there were a connexion between two men, and between one of them and the book of the other, which made it desirable and even necessary that the first should edit the second, it was the case of Rheticus and the first edition of the *De Revolutionibus*, &c.; and yet no arrangement could be made by which the sheets printed at

Nuremberg could be revised at Wittemberg. It is very unlikely, then, that Rheticus should have edited the second edition, when, as far as we know, a similar impediment existed.

The third edition, by Müller (Amsterdam, 1617), has no authority as to the text above that of the second.

Now both the second and third editions change the word *fatemur* into *fatentur*, thus causing Copernicus to throw the opinion in question upon his predecessors, instead of directly making it his own. Not that it would be conclusive, even if the emendation were adopted: for, as I have said, Copernicus is evidently speaking with approbation of the opinions which he describes; and it would be difficult to say why *comperiunt* or *putant* in one sentence should imply approbation, and *fatentur*, in the next, should be at least disavowal, if not disapprobation. If Rheticus, who knew the mind of Copernicus better than any one, had been the editor, I can conceive that stress ought to be laid upon the change of the first into the third person as an emendation; that is, I should be somewhat staggered by Rheticus having thought it necessary to make such an alteration.

But, Rheticus not being in the question, as I think, for the reasons given above, the next best authority on an opinion of Copernicus is Galileo. Now the latter, in speaking of the phases of Venus, expressly attributes to Copernicus the maintenance of one of the two alternatives,—that the planet is either self-luminous or perforated by the solar rays. Of these alternatives, he says, in his letter to Velsar (Works, vol. ii. pp. 88, 89), “Al Copernico medesimo convien amettere come possibile, anzi pur come necessaria una delle dette posizioni.” And that such was the opinion of Copernicus is also assumed by the writer of the note on the *Sydericus Nuncius* in the volume just mentioned, and by others, even down to our own time; as by Mr. Drinkwater Bethune, in his life of Galileo. In fact, with the exception of the unsupported story mentioned at the beginning of this paper, there is nowhere, that I can find, anything against my conclusion. And it is to be remembered, that Copernicus nowhere shows any of that acumen in matters of physics, apart from mathematics, which has often enabled the cultivators of the former to make steps more than proportionate to their knowledge of the latter. Ptolemy, the great promoter of the old theory, and Copernicus, its destroyer, were both mathematicians in a peculiar sense; Ptolemy being far the more sagacious in questions of pure experiment. Their grounds of confidence are mathematical; and Copernicus, in particular, dares to face his own physics (for there is no reason to suppose he was beyond his age in mechanical philosophy) with reasons drawn entirely from probabilities afforded by mathematics.

There is much reason to regret the practice of associating with the names of those who have led the way in great discovery the glory which is due to their followers. The disadvantage is twofold. In the first place, it introduces into the history of science an index error of from one to two centuries; secondly, those who come to in-

quire are disappointed to find that they must lower their opinion of great men, and are perhaps led to do it to a greater extent than justice requires. Our usual popular treatises speak of Copernicus as if, besides himself, he had in him no inconsiderable fraction of Kepler, Galileo, Newton and Halley. What is a person to think who comes from those histories to actual investigation, when he finds in Copernicus himself the immovable *centrum mundi* (only reading sun for earth) of the Ptolemaists, their epicycles, and a suspicion, at least, of the solid orbs?

On the Formation and Application of Fine Metallic Wires to Optical Instruments. By Mr. Ulrich.

Dr. Wollaston, in the Philosophical Transactions for 1813, proposed a method of forming wires of gold or platinum of any degree of tenuity. The discovery does not appear to have been much used, owing, as Mr. Ulrich supposes, to the difficulty of application.

Mr. Ulrich forms the fine wire by inserting a gold or platinum wire in the centre of a silver cylinder of much larger dimensions, which is afterwards drawn out by the usual process. When the silver wire has been sufficiently extended, Mr. Ulrich cuts it into short lengths and attaches platina rings to each end. The rings are hooked upon a hooked fork, and the whole is plunged into heated nitric acid, when the silver coating is dissolved.

The artist may now wire his cell according to his fancy. Mr. Ulrich's plan seems to be, to hold one end by an overplate; then to allow the wire to be stretched by its platina ring, and to fix the other overplate. He recommends using a cell of the same material as the wire, as, otherwise, a difference of expansion might break or slacken the wires.

On the properties of Rock as a foundation of the Piers of Meridian Instruments, with an Account of the Detection of a hitherto unsuspected Cause of Error in the Edinburgh Transit. By Professor C. P. Smyth.

Some years ago doubts were expressed of the fitness of a rock foundation for an observatory. It does not appear that any experiments were made, or that any reason was adduced beyond this, that as tremor was unfavourable to the performance of large telescopes, and as rock was more capable of transmitting tremors than less compact material, therefore rock was to be avoided when choosing a site for an observatory. The author or authors of this opinion were probably but ill-acquainted with the mode of working an observatory, or the requisites for obtaining accuracy in meridian observations; yet it is certain that an undue importance was attached in some cases to these very idle surmises. At the present time it is not likely that any intelligent person would be misled by such authorities, and it is therefore unnecessary to mention here the mischief they have caused\*. It is to be wished that the founders of future observatories,

\* The effect of tremor on a telescope is probably familiar to every reader of this notice. It causes a sort of burr round the object, and destroys the sharpness of outline and definition. This is probably more injurious in reflecting than in refracting telescopes; but we may fairly doubt whether it



who can command a rock foundation, should make use of their good fortune; and that those who cannot, would look carefully to the possible effects of moisture, which are probably more extensive, and vary more rapidly, than those of temperature.

The observatory of Edinburgh is placed on the Calton Hill. This is chiefly of a porphyritic formation. The apex was blasted away to obtain a level area, on which the observatory was erected. The site of each pier was cut away until a sound part of the rock was arrived at (it was not necessary to go deeper for this purpose than six or nine inches), when the exact size of the foundation was at once marked out and the space carefully levelled. The foundation stone was also carefully smoothed, and then laid in its place with milk of lime. As the foundation and stone were both rather hollow, except for three inches at the outer edge, which was polished, the fitting was very perfect. There are no vertical joints, and each stone was laid in the same manner as the foundation stone. As one of the principal thoroughfares of Edinburgh runs about 100 feet below, and only 300 feet distant from, the observatory, tremors were confidently predicted by the alarmists. Professor Henderson, however, found none, nor any interruption to his observations in mercury. Professor Smyth adds that he finds no annoyance from the railroad about 300 feet below, and at a horizontal distance of 500 feet.

So far the observatory founded on a rock came out victoriously from its ordeal, but Professor Henderson, in the course of his work, found a well-marked annual variation of the *level* of the transit, which he attributed to the expansion of the rock. This variation seemed so intimately connected with temperature that he latterly took his factor for level correction from the thermometer, having found a constant agreement between this and the indications of the spirit level. The maximum of this change amounted to between 0<sup>·</sup>2 and 0<sup>·</sup>3 in the value of the level factor, and the variations were tolerably regular.

On computing the azimuthal factors for 1841, Professor Smyth

is more felt on solid than on loose foundations. In a *standard* observatory, where observations are made principally in the meridian, tremor scarcely affects the *accuracy* of observation at all, unless it is so excessive as to change the position of the microscopes, piers, &c. Now this is obviously the least likely to happen when the foundation is on rock; the tremors are propagated through the substance, without in any respect altering its form. Sudden and discontinuous changes, which obey no law, are those only which are to be feared in a well-directed observatory. Tremor is chiefly objectionable as disturbing the mercurial horizon, which, however, is now mostly used as a verification, not as the ordinary mode of observing; and when this inconvenience only occurs occasionally, it can generally be avoided or palliated by a little contrivance or foresight. Unless the *adjustments* are kept in a fluctuating and uncertain state by occasional small oscillations (and we believe no careful experiments have been directed to this point), they are minor evils. The experience of the Oxford and of the Edinburgh Observatory is, so far as it goes, conclusive against any danger from moderate exposure to tremors in a well-founded and well-managed observatory.—S.



was very much disturbed on finding variations, which sometimes altered the factor as much as  $0^{\circ}3$  in a day, and more than  $1^{\circ}0$  in the course of the year. These changes in azimuth had been remarked by Professor Henderson, and were attributed to the irregular action of the counterpoises, which were consequently removed. On a comparison of these errors with the indications of thermometers plunged in the rock there were apparent marks of correspondence.

There are several thermometers inserted at different depths in the rock near the observatory, which had been carefully observed in the year 1841\*. The indications of these thermometers were projected on paper, and the curves thus formed compared with a curve traced according to the course of the azimuthal deviation. It was thus made evident, that the curve of azimuthal deviation, though having, like the other curves, an annual maximum, did not otherwise resemble the curves belonging to the deep-seated thermometers at all; and, in fact, it came nearest the curve traced out by the thermometer attached to the barometer and by the free thermometer exposed to the outer air. Hence the cause of the deviation was not to be looked for in the effect of temperature on the foundations or on the massive transit piers, but on smaller parts more readily affected, such as the metallic mounting. These were accordingly examined. In the azimuthal Y, the construction was found to be much as usual, but the artist has adopted an adjustment for the vertical Y, which seems liable to suspicion. There are two vertical screws applied from below; one, pushing, on the north side of the middle, and the other, pulling, at the south side. The Y is prevented from turning in a vertical plane by jamming horizontal screws, which press a plate against the north face of the Y so as to bring the whole tightly against a stopping-piece, which blocks the south face. Professor Smyth's present opinion is, that the effect of expansion on the two screws, which are in contrary states of constraint, is to alter the adjustment; certainly the arrangement looks unmechanical. In the ordinary mode of construction, in this country at least, the elevating Y is either raised by one central screw, or by two screws, one on each side of the centre; in which case a drawing-screw may be placed at the centre. There is thus no tendency to twist, and the side-plates which confine the Y laterally have to exert little restraining force. Professor Smyth has communicated with MM. Repsold, the makers of this magnificent instrument, and is awaiting their reply before adopting any remedy†.

\* Some years ago, Professor J. D. Forbes had four thermometers sunk in the rock with their bulbs at the depths of 24, 12, 6, 3 French feet and a fifth on the surface merely covered with sand.

† Sudden and lawless changes in azimuth forbid independent determinations of the azimuthal deviation (which are also the best), viz. from the consecutive semidiurnal transits of circumpolar stars. The possessor of an imperfectly mounted instrument must content himself with assuming the fundamental places of his close circumpolar stars, and determine his azimuthal error from each of them. This will, with proper caution, be found quite sufficient for objects not too near the pole, especially when the clock-error stars are pretty numerous, and situated above and below the object to be determined.

LXXVIII. *Intelligence and Miscellaneous Articles.*

ON OSMIAMIC ACID. BY MM. J. FRITZSCHE AND H. STRUVE.

**W**HEN caustic ammonia is added to a solution of osmic acid in excess of potash, the deep orange colour of the liquid becomes rapidly a bright yellow, and a new salt is produced and separated, either immediately or by evaporating the liquid at a gentle heat, which is a yellow crystalline powder.

The formation of this new compound does not necessarily depend on the presence of potash or any other oxide, but uniformly upon that of ammonia; the ammoniacal salt is, however, subject to alteration, and decomposes during evaporation. It is better therefore to cause a basic oxide to intervene.

M. Gerhardt remarks that the formula of the osmiamates which the authors have given requires correction; they agree, he states, with the formula  $OS, O^3 N (M)$ .

The properties of the osmiamates are as follows: they decompose by heat with explosion; and several of them undergo the same decomposition when struck. Among the products of this decomposition are metallic osmium, an osmiate, or a less oxygenated osmic compound. Protosmiamate of mercury volatilizes without explosion, when heated quickly; and it diffuses a strong smell of osmic acid.

Osmiamic acid can be obtained only in solution in water. In order to prepare it, osmiamate of barytes is to be cautiously decomposed by sulphuric acid, or recently prepared and moist osmiamate of silver is to be decomposed by dilute hydrochloric acid. After filtration a bright yellow-coloured solution is obtained, which may be preserved for several days, if it be sufficiently dilute; on the other hand, if too concentrated, it becomes brownish and decomposes with the disengagement of gas, osmic acid is set free, and a black non-explosive substance is deposited which contains osmium.

The same metamorphosis occurs when the weak acid is evaporated over sulphuric acid.

Osmiamic acid not only expels carbonic acid from carbonates, but also decomposes chloride of potassium. In fact crystals of osmiamate of potash are obtained, if a crystal of chloride of potassium with a drop of solution of osmiamic acid be exposed to evaporation on a strip of glass.

Zinc dissolves in solution of osmiamic acid, with the evolution of a little gas; part of the acid is decomposed, and the zinc is covered with a very adherent black deposit, and flocculi appear in the liquid which possess the odour of osmic acid. When all the undecomposed acid is saturated with zinc, the metamorphosis ceases.

In the cold, acids do not decompose osmic acid or the osmiamates: sulphuric, nitric or hydrochloric acid may be added to their solutions without inconvenience; but decomposition readily occurs when heat is applied, and it is rendered apparent by the disengagement of osmic acid and by the brown colour of the liquor; the products vary according to the nature of the acid employed.

Osmiamates are obtained, either directly by the action of osmic acid on a solution of bases in ammonia, as the salts of potash, zinc, and silver, or by precipitating the potash salt by metallic salts, or by decomposing the silver salt by chlorides.

MM. Fritzsche and Struve have stated that these salts yield no hydrogen by analysis. In two experiments the potash salt gave by combustion with oxide of copper only 0.072 and 0.033 of hydrogen; whereas 0.34 are required for one equivalent of hydrogen.

The osmiamates undergo an interesting decomposition by the action of hydrochloric acid. The products vary according to the concentration of the acid. If the potash salt be sprinkled with concentrated acid, energetic action immediately ensues, accompanied with the disengagement of chlorine and probably of its oxide; the hydrochloric acid assumes a fine purple tint, and the crystals of osmiamate of potash are covered with a crust of small red crystals of two different kinds; if the salt employed be powdered, and the action of the hydrochloric acid be long enough continued, all the osmiamate undergoes this change; the nature of which the authors have not hitherto succeeded in explaining.

If dilute hydrochloric acid be added to a solution of osmiamate of potash saturated cold, no decomposition occurs at common temperatures, the metamorphosis taking place only at a higher temperature. It is then more complicated, the liquor temporarily assumes a red and brown tint, and soon emits a smell of osmic acid, which is abundantly disengaged as soon as the liquor is heated to ebullition. If the solution be evaporated to the crystallizing point, as soon as it ceases to emit osmic acid, a mixture of salts is obtained, among which, as shown by the microscope, are hexagonal green tables, green needles, and another red salt, &c. These salts appear to be decomposed by water, for they were not obtainable by solution and recrystallization.—*Journ. de Ph. et de Ch.*, Octobre 1847.

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ON THE PREPARATION AND PROPERTIES OF SOME OSMIAMATES. BY MM. FRITZSCHE AND STRUVE.

*Osmiamate of Potash.*—This salt is best prepared by dissolving solid osmic acid in a concentrated solution of caustic potash, with the addition of ammonia during the agitation of the mixture. The solution becomes of a bright yellow tint, and the osmiamate of potash is deposited in the state of a yellow granular powder. The product of the distillation of osmic liquors may also be directly passed into a solution of potash, containing ammonia and properly cooled; the simultaneous distillation of nitrous vapours must be carefully avoided, as they would decompose the osmiamate of potash.

In both cases, the mother-water which has deposited osmiamate of potash is to be evaporated with a gentle heat; carbonate of potash may be used instead of caustic, but not so advantageously; the osmiamate of potash is to be dissolved in a very small quantity of boiling water; on cooling the solution yields small crystals of the salt of a lemon-yellow colour; these crystals are of considerable size

when prepared from a cold saturated solution by spontaneous evaporation, their form being an acute octahedron with a square base.

Osmiamate of potash is much less soluble in alcohol than in water; it dissolves without alteration, and decomposes but very little when the solution is evaporated; it contains no water of crystallization; it may be heated to  $356^{\circ}$  F. without decomposing, but it becomes brownish and is rapidly decomposed at a higher temperature, with violent projections.

This salt yielded by analysis—

Osmium	.....	67.900
Nitrogen	..... 4.126	4.820
Potash	.....	16.126

M. Gerhardt gives as an amended formula  $\text{OSO}^3\text{N}(\text{K})$ .

*Osmiamate of Soda* is best obtained from the silver salt and chloride of sodium; the crystals are prismatic, contain water of crystallization, and are very soluble in water.

*Osmiamate of Ammonia* is prepared in the same manner. It forms large anhydrous crystals, which appear to be isomorphous with the salt of potash; at  $258^{\circ}$  F. it decomposes with explosion. This salt is very soluble in water and in alcohol.

*Osmiamate of Barytes* readily crystallizes in yellow brilliant needles of several lines in length. This salt is readily soluble in water, and explodes at about  $300^{\circ}$  F. It yielded by analysis—

Barytes	.....	23.88
Osmium	.....	61.07
Nitrogen	.....	4.269

the formula according to M. Gerhardt being  $\text{OSO}^3\text{N}(\text{Ba})$ .

*Osmiamate of Ammonia and Zinc* is obtained either by dissolving osmic acid in a solution of a salt of zinc in caustic ammonia, or by mixing a solution of osmic acid in ammonia with the solution of a salt of zinc. A yellow, bright crystalline powder is soon deposited, which is deprived of the mother-water by washings with ammonia.

This compound is very permanent; it may be dried in the air, and remains without losing ammonia. It is nearly insoluble in ammonia, water decomposes it even when cold; when boiled in water it is completely decomposed with the deposition of oxide of zinc, the disengagement of half of its ammonia, and yielding osmiamate of ammonia. Formula according to M. Gerhardt  $\text{OSO}^3\text{N}(\text{Zn}), 2\text{NH}^3$ .

*Osmiamate of Lead*.—A solution of nitrate of lead is not precipitated by a concentrated solution of osmiamate of potash; after some time some crystals are however formed, which are not sufficiently stable for examination. A solution of acetate of lead gives with the solution of the osmiamates a non-crystalline precipitate, which is at first of a dirty yellow colour, but it soon becomes of a purple tint with the extrication of osmic acid.

If a solution of chloride of lead, or a solution of nitrate of lead with the addition of hydrochloric acid, be added to a solution of osmiamate of potash, a yellow crystalline precipitate is soon obtained, which the authors consider to be a compound of equal equivalents of chloride and osmiamate of lead.



*Protosmiamate of Mercury* forms a bright yellow precipitate ; it is not crystalline, and is insoluble in water ; the perosmiamate of mercury forms prismatic crystals.

*Osmiamate of Silver* is obtained directly by dissolving osmic acid in an ammoniacal solution of a salt of silver, and afterwards supersaturating with nitric acid. It may also be obtained by adding to a solution of osmic acid in ammonia nitric acid in excess at first, and then a salt of silver. It may also be prepared by double decomposition with the salts of silver and soluble osmiamates.

Osmiamate of silver is a crystalline powder of a lemon-yellow colour ; it is very slightly soluble in water and in cold nitric acid, more soluble in ammonia, and may be combined with it. It may be dried in the dark without blackening, *in vacuo*, over sulphuric acid ; eventually, however, it suffers decomposition, and then gives out osmic acid ; at 176° F. it decomposes suddenly and with violent detonation ; it is also decomposed by percussion, and likewise when sulphuretted hydrogen is passed over the dried salt ; nitric acid decomposes it readily when heated ; the liquor first acquires a brown tint, and gradually becomes colourless with the disengagement of osmic acid.

This salt yields by analysis—

Oxide of silver ..	32.08	32.060	32.13
Osmium .....	55.011		

M. Gerhardt gives as its formula  $\text{OSO}^3\text{N}(\text{Ag})$ .—*Journ. de Ph. et de Ch.*, Octobre 1847.

#### ON SULPHATO-CHLORIDE OF COPPER,—A NEW MINERAL.

BY ARTHUR CONNELL, ESQ.

This mineral occurs in small but very beautiful fibrous crystals, of a fine blue colour, which is pale when the fibres are delicate, but much deeper when they become somewhat thicker. Their form, according to Mr. Brooke, is a hexagonal prism with the edges replaced, thus belonging to the rhombohedral system. They possess considerable translucency, and have a vitreous lustre. On account of the small quantity which he possessed, Mr. Connell was unable to state the specific gravity, hardness, or fracture. Their locality is Cornwall. Mr. Brooke is aware of the existence of only a very few specimens of the mineral : one is in the British Museum.

Like atacamite, this mineral colours the blowpipe flame as well as the simple flame of a candle, of a fine greenish-blue, indicating the presence of chloride of copper. Reduced to powder, and mixed in sufficient quantity with charcoal powder, and then heated in a close tube, it gives decided, although not strongly marked, indications of the presence of sulphuric [sulphurous?] acid by the smell, and partial bleaching of Brazil wood paper, the remainder of the paper being reddened, doubtless by muriatic acid vapours. Alone, in the close tube, it yields a little water, and other appearances resembling those afforded by atacamite. Heated alone on charcoal before the blowpipe, it decrepitates strongly ; but when previously deprived

of the greater part of its water by gentle heat, and then powdered and moistened, and heated on charcoal, it gives no traces of arsenic, although arseniate of copper is associated with it in the specimens. The residue is a dark reddish slag or globule.

The crystals are not soluble in boiling water, but dissolve entirely and pretty readily in nitric or muriatic acid, especially by the aid of a gentle heat. The solutions have the colour belonging to copper solutions; and in the act of dissolving a very few bubbles of gas may be observed to arise, indicating probably the presence of a minute quantity of carbonate. The solutions yield, with barytic salts, a white precipitate insoluble in acids; and the nitric solution gives, with nitrate of silver, a white and curdy precipitate insoluble in acids or water, but soluble in ammonia. Ammonia in excess, added to the original solution, gives the fine deep blue of copper.

These appearances, in conjunction with the blowpipe reactions, are sufficient to show that the constituents of the mineral are sulphuric acid, chlorine, copper, and a little water; but Mr. Connell had not sufficient of the mineral to determine the proportions of its constituents. The chloride is apparently more abundant than the sulphate.—Jameson's *Journal*, October 1847.

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#### ON THE FORMATION OF VALERIANIC ACID. BY M. THERAULT.

The author remarks that it has been long known that the oil of potatoes yields valerianic acid under the influence of the caustic alkalis; and it has also been stated that the oil of valerian gave analogous results. M. Therault thought it would be an interesting subject of inquiry to determine in what manner this transformation occurs, and whether it is complete or only partial; and in the latter case to examine into the nature of the non-acidifiable product; whether the alkalies directly produced a true chemical reaction on the elements of the oil; and lastly, whether the intervention of other agents is not requisite to effect the transformation.

In order to resolve these questions, the following experiments were performed, care being taken to operate with oil perfectly deprived of any trace of acid.

1. A portion of oil was mixed with distilled water, and divided into two parts, one of which was exposed to the contact of the air, and the other put into a bottle to prevent its action.

2. Another portion of the oil was mixed with caustic potash, perfectly dry and reduced to powder, and divided as in the preceding experiment.

3. A mixture was prepared of six parts of oil and three parts of potash, previously dissolved in one part of water, and the mixture was divided as in the foregoing experiments.

The following observations were made on these mixtures. After the contact of a month, that portion of the mixture of the first experiment which had been submitted to the action of the air, had become sensibly acid; in the second portion no change had occurred. In the second experiment no sensible trace of valerianic acid was

produced ; the potash and the oil had nearly retained their original properties.

Circumstances were quite different in the third experiment. The mixture had hardly been made when it became of the consistence of honey, and of a red colour of considerable intensity : perfect saponification might be suspected. M. Bonastre had previously remarked this action of the caustic alkalies on some essential oils, and had proposed it as a means of distinguishing mixtures of them ; and he noticed the partial combination of the oil of valerian with soda. This fact might induce the belief that this oil was a substance of a complex nature ; M. Therault is, however, of opinion that this is not the case, but that the observation of M. Bonastre was derived from the circumstance of the oil which he employed containing valerianic acid, which would explain in this case the partial combination with soda. The author attentively examined the nature of this mixture : it was perfectly homogeneous, and comparable to crotonic soap. Treated with water and suffered to remain undisturbed, the oil soon collected on the surface ; it was separated, and the filtered liquor was saturated with acetic acid. No sensible trace of oil was reproduced, nor was the formation of valerianic acid detected ; it was therefore certain that no chemical action had occurred ; and the name of combination given to this mixture appears to the author to be improper under these circumstances.

M. Therault relies upon this last fact as corroborating the result of the third experiment. The portion of the mixture kept from the contact of the air, underwent no change of properties after one month ; no combination had occurred between the oil and the potash ; no valerianic acid was formed, or at any rate no appreciable quantity.

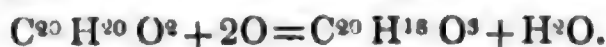
On the contrary, that portion which had been exposed to the action of the air contained valerianic acid, in minute quantity certainly, but it was very appreciable : the matter was slightly decolorized. The action of the air was allowed to continue, and after six months' exposure nearly the whole of the oil had disappeared, and the mixture was almost entirely decolorized, a slight amber tint only remaining.

The decoloration occurred nearly in direct proportion to the quantity of acid formed : the action of the air was however continued, to ascertain whether the whole of the oil could be acidified ; the operation required nearly six months, but it was then complete ; the mixture had assumed the aspect of white, slightly grumous honey ; well-defined crystals of valerianate of potash had formed, and were dispersed throughout the mass. It was covered with a solution of this salt and of potash, without any trace of oil.

It may be concluded from the experiments detailed, that valerianic acid does not pre-exist in valerian root ; that it is entirely the product of the oxidizement of its oil ; that this oxidizement is due to the oxygen of the air ; and that water and the caustic alkalies greatly facilitate this oxidizement. The author also concludes that the caustic alkalies exert no direct action on the elements of the oil ; that they act only by the property which they possess of forming an

intimate mixture with the oil, and exposing it in a state of perfect division to the oxygen of the air. Lastly, M. Therault is of opinion that oil of valerian is not a substance of a complex nature, and that it may be entirely converted into valerianic acid.

This conversion is readily explained. According to Ettling, the formula of valerianic acid is  $C^{20}H^{18}O^3 + H^2O$ ; if that of oil of valerian be  $C^{20}H^{20}O^2$ , and if two atoms of oxygen be made to intervene, one of which combines with two atoms of hydrogen to form water, and the other be added, we shall have the following equation:



M. Therault finishes his memoir with observing, that the process for extracting valerianic acid, proposed by Messrs. Smith of Edinburgh in the *Journal de Pharmacie* for January last, appears to be a good one; excepting that he would propose to use a caustic instead of a carbonated alkali, and after having boiled the mixture, to expose it for a month to the air, stirring it several times a day; not to subject the residue to pressure, and to distil with the roots, which greatly facilitate the operation. When afterwards the distilled liquor has been saturated by means of an alkali, and the valerianate of potash or soda has been concentrated, it is essential not to employ an excess of sulphuric acid to separate the valerianic acid; it would be better to leave a small portion of the valerianate undecomposed, for towards the end of the distillation, the organic matter mixed with the salt is carbonized and sulphurous acid is formed, which appears to react upon the valerianic acid.—*Journ. de Pharm. et de Ch.*, Septembre 1847.

NOTE ON THE MEASUREMENT OF THE DOUBLE SULPHATES OF ZINC AND SODA, AND OF MAGNESIA AND SODA. BY PROF. W. H. MILLER.

The crystals were not good enough for me to obtain a very satisfactory result from a few observations; they are however sufficiently good to show that they are isomorphous.

The crystals belong to the oblique prismatic system.

The angles between normals to the faces are—for the oxide of zinc salt,

$$n n' \quad 113^\circ 4'$$

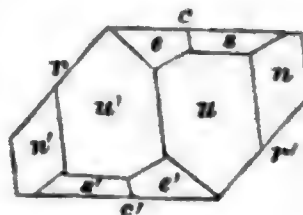
$$u u' \quad 74^\circ 12'$$

the angle between a normal to  $c$  and the intersection of

$$u, u' = 10^\circ 22'$$

$$u c \quad 83^\circ 46'$$

$$r c \quad 49^\circ 54'$$



The symbols of the simple forms, in the notation which I use, are—

$$c \ 001, \ r \ \bar{1}01, \ n \ 110,$$

$$u \ 120, \ e \ 011, \ s \ 121.$$

The faces of the magnesian salt are more irregular than those of the former, so that I cannot pretend to determine the difference between the angles of these crystals.



The angles given above must be considered as rough approximations only. In a little time perhaps I may be able to obtain more accurate values of them.—*From the Proceedings of the Chemical Society*, vol. iii. p. 391.

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#### NATIVE CARBONATE OF NICKEL.

This new mineral was exhibited last year at the Philosophical Society's exhibition in Glasgow, and was examined at the time, at the request of Dr. R. D. Thomson, by his pupil Mr. John Brown, in the College Laboratory. It occurs in the form of thin green crystalline layers, on the surface of chrome iron ore from America. It dissolves with effervescence in dilute hydrochloric acid. The solution is precipitated black by sulphohydret of ammonia; is precipitated and dissolved in excess by caustic ammonia, yielding a characteristic coloured solution. Caustic soda precipitates the solution green, without resolution. It is accompanied, apparently in union, by carbonates of lime and magnesia—isomorphous bodies. The fact of its occurring on the surface of chrome iron, and having been mistaken for sesquioxide of chrome, renders it probable that oxide of nickel may exist in that mineral occasionally.—R. D. T.

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#### AN EXAMINATION AND ANALYSIS OF THE "NADELERZ," OR NEEDLE ORE OF BISMUTH. BY E. J. CHAPMAN, ESQ.

THE "needle ore" occurs in thin prismatic crystals, generally forming more or less radiated groups imbedded in quartz, at Ekatherinenburg in Siberia, the only known locality in which it has been hitherto found. The crystals are too imperfect to admit of measurement; but they appear to belong to the Trimetric or Prismatic system, and to have for the primary form a right rectangular prism, or perhaps more correctly a right rhombic one, in which the angle MM closely approaches a right angle.

The colour of this mineral is dark steel-gray on the fractured surface, but externally the true colour is usually masked by a yellow tarnish. The powder or "streak" is black; the degree of hardness 2.0 to 2.5, or between that of rock-salt and calc-spar; and the specific gravity about 6.1.

Before the blowpipe it fuses instantly and may be almost entirely volatilized, forming a yellow incrustation of the mingled oxides of lead and bismuth on the support. The presence of bismuth and copper may be ascertained by fusion with "microcosmic salt" and a little tin on charcoal in the reducing flame, when the lead, which is clear whilst hot, becomes on cooling of a grayish-black colour with red patches. With carbonate of soda on charcoal in the same flame, it forms an alkaline sulphuret. The lead is best detected by boiling a fragment in nitric acid, filtering, dissolving the residue (sulphate of lead) in caustic potash, diluting the solution, and reprecipitating by sulphuric acid.

This ore was first described by Karsten and analysed by John; and although a considerable period has elapsed since the date of this analysis, yet, probably from the rarity of the mineral, its composition

has been examined by but one other chemist, Frick, in Poggendorff's 'Annalen,' xxxi. p. 529.

These analyses have given—

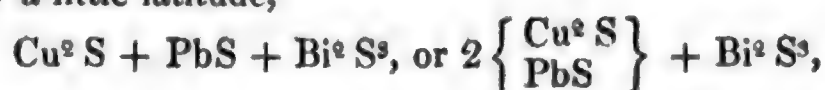
1. By John:—

	Per cent.	Atomic relations.	
Sulphur.....	11.58	0.057	3
Bismuth .....	43.20	0.032	2
Lead.....	24.32	0.018	1
Copper.....	12.10	0.031	2
Nickel .....	1.58		
Tellurium.....	1.32		
Gold.....	0.79		
	94.89		

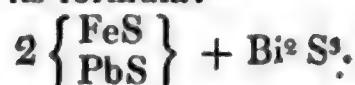
2. By Frick:—

Sulphur .....	16.61	0.0826	6
Bismuth .....	36.45	0.0410	3
Lead .....	36.05	0.0270	2
Copper .....	10.59	0.0267	2
	99.70		

The first analysis does not admit of any rational formula; but if we consider the loss, more than 5 per cent., to be sulphur, we may obtain by a little latitude,



a formula analogous to that of the kobellite from Ilvena in Sweden, analysed and named by Setterberg, and in which the electro-negative atoms in the base are to the electro-negative atoms in the acid as 2 to 3. The following is its formula:—



The second analysis yields also but an inexpressive and unsatisfactory result. The formula, if such it can be termed, obtained from it is—



I have now to enter into the details of a third analysis, executed lately by myself, on a specimen kindly given to me by Colonel Jackson, F.R.S., who brought it with him from Russia. The ore was accompanied in the quartz by minute tufts of malachite, which, together with the matrix, were carefully removed, by the aid of a microscope, from the substance analysed.

8.38 grs. of the mineral in powder were boiled in strong nitric acid.

A residue of 4.92 grs. of sulphate of lead remained, and 0.26 grs. of sulphur. The 4.92 grs. of sulphate of lead (obtained, it should be stated, after solution of the residue in potash and subsequent re-conversion) = 3.36 grs. of lead and 0.52 of sulphur.

Carbonate of ammonia in excess was then added to the clear solution; and after remaining for three hours at a gentle heat, it was filtered from the precipitate, which (after being well-washed with

the same reagent, and the "washings" added to the original solution) was dissolved in acetic acid; and a slip of pure lead being immersed in the solution, the whole was covered up immediately and suffered to stand for four hours. The slip of lead weighed 22·63 grs.

The four hours having elapsed, the lead was taken from the solution, and, after separation of the precipitated bismuth, dried and weighed. It weighed 19·21 grs.; loss, 3·42 grs. On the addition of sulphuric acid, 5·03 grs. of sulphate of lead were obtained, which are equal to 3·435 of lead; and this amount corresponding so nearly with the loss in the metallic precipitant, the whole of the lead present in the mineral may be considered to have been converted into  $\text{PbO}$ ,  $\text{SO}^3$  by the first operation.

The bismuth precipitate was washed with cold distilled water (which had been boiled), dissolved in nitric acid, and again thrown down by carbonate of ammonia. The oxide of bismuth weighed 2·60 grs., equivalent to 2·33 grs. of bismuth.

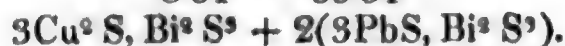
To the original solution (containing carbonate of ammonia) a few drops of ammonia were added, and it was then gently evaporated until the ammoniacal odour was entirely destroyed. Solution of potash was then added, and the whole boiled. The black oxide of copper, well-washed with hot water, ignited, and weighed in a covered crucible, came to 1·31 grs., an amount equal to 1·05 of copper.

Finally, chloride of barium was added to the potash solution, which produced a precipitate of sulphate of baryta weighing 5·72 grs., an amount corresponding to 0·79 of sulphur. The whole of the sulphur present in the mineral was therefore

$$1\cdot57 \text{ grs. } (0\cdot52 + 0\cdot26 + 0\cdot79).$$

The following table exhibits the above analysis and its atomic deductions:—

		Per cent.	Atomic relations.	
Sulphur .....	1·57	18·78	0·0935	3 or 18
Bismuth .....	2·33	27·93	0·0315	1 or 6
Lead.....	3·36	40·10	0·0309	1 or 6
Copper.....	1·05	12·53	0·0317	1 or 6
	8·31	99·34		



This formula is identical with that of the bournonite (from the analyses of H. Rose, Smithson, &c.), substituting only  $\text{Bi}^3\text{S}^3$  for  $\text{Sb}^3\text{S}^3$ , as below:—



In each ore, the electro-negative atoms in the basic compounds are, to the electro-negative atoms in the acid compounds, as 1 to 1, as expressed in the accompanying general formula:—



As the bournonite crystallizes also in the same system as the needle ore, and indeed affects probably the same primary form within close measurements, the isomorphous relationship of these minerals is sufficiently apparent.

Most English mineralogists give a right rectangular prism for the primary form of the bournonite; but Dufrénoy, in his recent Treatise, vol. iii. p. 18, after an examination of numerous crystals, considers a right rhombic prism in which the angle  $MM = 93^{\circ} 40'$  to be the correct primary form. The modified rectangular prism in which the bournonite usually occurs is in this light a secondary form, derived from the primary by the replacement of its lateral edges by the planes  $g' h'$  in the notation of Haüy.

The specimen of the needle ore which furnished the above analysis, exhibited here and there in the quartz transverse rhombic sections, in which an accustomed eye might easily perceive that the obtuse angle was included between  $90^{\circ}$  and  $100^{\circ}$ .

I could not detect in this specimen the presence of tellurium, found by John in the needle ore; it is however perfectly conceivable that, under certain circumstances, a portion of the PbS may be replaced by PbTe.—*From the Chemical Gazette for September 1, 1847.*

#### ACTION OF ANHYDROUS PHOSPHORIC ACID ON AMMONIACAL SALTS. BY M. DUMAS.

The author finds that when anhydrous phosphoric acid is made to react upon crystallized acetate of ammonia, there distils a liquid the fixed boiling-point of which is  $170^{\circ}$  F., and which is miscible with water in all proportions. When purified by digestion with a saturated solution of chloride of calcium, and then distilled from solid chloride of calcium and from magnesia, it still possesses the boiling-point above mentioned.

Analysis gave the following numbers:—

	Experiment.	Calculation.
Carbon . . . . .	57.4	58.5
Hydrogen . . . . .	7.4	7.3
Nitrogen . . . . .	34.4	34.2
	<u>99.2</u>	<u>100.0</u>

The density of the vapour gave the number 1.45. The above results lead to the very simple formula  $C^4 H^3 N$ , which differs from acetate of ammonia by four equivalents less of water. Its composition is similar to that of nitroguret of acetylene.

But a point of view, which the reactions will warrant, would give to this substance the following rational formula,  $C^2 NH$ ,  $C^2 H^2$ , which would make hydrocyanate of methylene of it, or an isomeric of it.

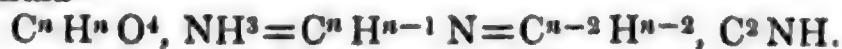
The reactions which have been examined gave rise to some curious phenomena. Thus solution of potash at a boiling heat disengages ammonia and regenerates acetic acid; chromic acid has no action; nitric acid is not decomposed by this liquid even when heated to ebullition. Potassium acts vividly in the cold, and with the disengagement of heat; cyanide of potassium is formed, and an inflammable mixture of free and carburetted hydrogen gases is evolved.

It is well known that M. Fehling obtained a substance of analogous composition to that now described by distilling benzoate of ammonia with a naked fire; he did not however attach to the discovery the views which have been now developed, nor did he study



its reactions. M. Dumas proposes to examine, under the new point of view described, the action of anhydrous phosphoric acid on the ammoniacal salts formed by the volatile organic acids.

M. Dumas remarks that if the product which he has obtained should constitute a compound identical with hydrocyanate of methylene, all these ammoniacal salts, treated in the same manner, should yield æthers corresponding to certain alcohols, according to the general formula—



In decomposing the latter by potash, there might be produced alcohol  $C^{n-2} H^{n-2}$ ,  $2HO$ , and prepared by this method, all the alcohols from the fatty acids.—*Comptes Rendus*, Septembre 13, 1847.

#### METEOROLOGICAL OBSERVATIONS FOR OCT. 1847.

*Chiswick*.—October 1. Hazy: cloudy. 2. Cloudy. 3. Light clouds and fine: overcast. 4. Foggy: fine. 5. Fine: light clouds: clear at night. 6. Dense fog: very fine: lightning and rain at night. 7. Fine: rain: lightning at night: clear. 8. Very fine. 9, 10. Rain. 11. Rain in forenoon: clear at night. 12. Slight fog: very fine. 13. Foggy: hazy: cloudy at night. 14. Hazy and drizzly: cloudy. 15. Hazy and cold: slight rain. 16. Foggy: very fine. 17. Foggy, with slight drizzle: very fine. 18. Slight fog: rain. 19. Exceedingly fine: rain. 20. Very fine: rain at night. 21. Rain: clear at night. 22. Fine. 23. Densely clouded and boisterous: rain. 24. Slight showers. 25. Very clear: fine: clear and frosty. 26. Frosty: uniformly overcast. 27. Fine: rain. 28. Hazy and mild. 29. Exceedingly fine. 30. Overcast and mild. 31. Cloudy and mild.

Mean temperature of the month .....  $52^{\circ} \cdot 14$   
 Mean temperature of Oct. 1846 .....  $50 \cdot 37$   
 Mean temperature of Oct. for the last twenty years .....  $50 \cdot 42$   
 Average amount of rain in Oct. .... 2'60 inches.

*Boston*.—Oct. 1—5. Cloudy. 6. Rain. 7. Fine: rain p.m. 8. Fine. 9. Fog: eclipse of the sun invisible until three-quarters over: fog. 10. Rain: rain a.m. 11—13. Fine. 14, 15. Cloudy. 16. Fine. 17, 18. Fog. 19, 20. Fine. 21. Cloudy: rain a.m. 22. Fine. 23. Cloudy: rain p.m. 24—26. Fine. 27. Rain: rain a.m. and p.m. 28. Fog. 29. Rain: rain a.m. 30. Fine: rain a.m. 31. Cloudy.

*Sandwich Manse, Orkney*.—Oct. 1. Clear: cloudy. 2. Cloudy: clear. 3. Cloudy. 4. Cloudy: drops. 5. Bright: showers. 6. Showers. 7. Drizzle. 8. Drizzle: clear: aurora. 9. Clear: cloudy. 10. Cloudy: drizzle. 11. Clear: fog. 12. Fog. 13. Cloudy: clear: aurora. 14. Cloudy: clear. 15, 16. Clear: cloudy. 17. Showers: drizzle. 18. Rain. 19. Damp: rain. 20, 21. Showers: clear. 22. Showers: rain. 23. Showers: sleet-showers. 24. Sleet-showers. 25. Clear. 26. Drops: showers. 27. Bright: drops. 28. Cloudy. 29. Cloudy: shower: lightning. 30. Showers: rain. 31. Bright: cloudy.

*Applegarth Manse, Dumfries-shire*.—Oct. 1, 2. Chill and droughty. 3, 4. Dull, but fair. 5. Fair a.m.: showery p.m. 6. Heavy rain a.m. 7. Heavy rain a.m.: flood. 8. Frequent showers. 9. Fine a.m.: rain p.m. 10. Heavy rain. 11. Fair: rain in the night preceding. 12. Fair and fine. 13. Fair, but raw and cloudy. 14, 15. Fair, though chilly. 16. Very fine clear day. 17. Dull and cloudy. 18. Dull and cloudy: rain p.m. 19. Heavy rain. 20, 21. Occasional showers. 22. Rain a.m.: very heavy p.m. 23. Rain early a.m.: fine day. 24. Heavy showers. 25. Fair: fine: clear. 26. Rain nearly all day. 27. Heavy rain and flood. 28. Fog: cleared p.m. 29. Fair and fine. 30. Fair a.m.: heavy rain p.m. 31. Rain early a.m.: cleared.

Mean temperature of the month .....  $49^{\circ} \cdot 5$   
 Mean temperature of Oct. 1846 .....  $49 \cdot 5$   
 Mean temperature of Oct. for twenty-five years .....  $49 \cdot 6$   
 Average rain in Oct. for twenty years ..... 3'56 inches.  
 Rain in Oct. 1847 ..... 5'09 "

*Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at Chiswick, near London; by Mr. Veall, at Boston; by the Rev. W. Dunbar, at Applegarth Manse, Dumfries-shire; and by the Rev. C. Clouston, at Sandwick Manse, Orkney.*

Days of Month.	Barometer.						Thermometer.						Wind.				Rain.					
	Chiswick.			Dumfries-shire.			Orkney, Sandwick.			Chiswick.			Dumfries-shire.			Orkney, Sandwick.			Boston.		Chiswick.	
	Max.		Min.	9 a.m.		9 p.m.	9 a.m.		8 p.m.	Max.		Min.	8 a.m.		Min.	Max.		Min.	Dumfries-shire.		Orkney, Sandwick.	
	1847.	Oct.																				
1.	30.050	29.995	29.64	30.15	30.15	30.41	29.64	30.43	30.15	65	52	56	59½	42	48½	48½	50	48½	calm	ne.	.....	
2.	30.110	30.039	29.64	30.16	30.13	30.34	29.64	30.35	30.13	60	42	53.5	55	48	48	45½	50	45½	n.	ene.	.....	
3.	30.164	30.124	29.72	30.16	30.12	30.30	29.72	30.34	30.12	59	48	52.5	52½	47½	47½	46	49	46	n.	ene.	.....	
4.	30.039	29.886	29.64	30.03	29.90	30.07	29.64	30.17	30.07	62	39	51	53	46	49	44	49	44	ne.	ne.	.....	
5.	29.750	29.647	29.35	29.76	29.63	29.89	29.35	29.98	29.89	60	33	49	50½	53½	45	47	45	47	n.	ne.	.....	
6.	29.637	29.548	29.30	29.60	29.40	29.93	29.30	29.96	29.93	63	52	50	53	43½	49½	46	49½	46	e.	e.	.....	
7.	29.645	29.460	29.11	29.27	29.40	29.62	29.11	29.64	29.62	64	37	57	53½	48	48	51	48	51	sw.	sw.	.....	
8.	29.885	29.763	29.30	29.40	29.65	29.79	29.30	29.66	29.79	63	53	50	52½	45½	50½	46½	50½	46½	sw.	sw.	.....	
9.	29.897	29.880	29.46	29.75	29.63	29.82	29.46	29.76	29.82	62	52	49	56	38	50½	50½	50½	50½	sw.	sw.	.....	
10.	29.851	29.804	29.36	29.53	29.60	29.68	29.36	29.83	29.68	65	48	57	59½	51½	53	52½	53	52½	sw.	sw.	.....	
11.	29.846	29.839	29.43	29.68	29.73	29.88	29.43	29.83	29.88	66	46	57	61	54½	54½	51½	54½	51½	sw.	sw.	.....	
12.	29.886	29.844	29.44	29.77	29.86	29.95	29.44	29.95	30.08	72	52	55	67	52½	54½	49½	49½	49½	ene.	ene.	.....	
13.	29.919	29.793	29.50	29.95	29.97	30.13	29.50	30.16	30.13	64	49	55	54½	49½	49½	46	46	46	ne.	ne.	.....	
14.	29.864	29.739	29.50	29.88	29.80	29.79	29.50	30.01	29.79	54	48	51	52½	45	46	47½	47½	49½	e.	e.	.....	
15.	29.865	29.695	29.38	29.83	29.90	30.04	29.38	30.00	30.04	56	40	52	55	43½	46	47½	50	47½	e.	calm.	.....	
16.	29.960	29.944	29.56	29.95	29.88	30.03	29.56	30.03	29.83	62	52	51	56	46	46	47½	50	47½	ne.	ne.	.....	
17.	29.986	29.843	29.54	29.80	29.68	29.49	29.54	29.60	29.49	61	46	50	55	37½	52	52	52	52	e.	sw.	.....	
18.	29.718	29.598	29.30	29.48	29.33	29.23	29.30	29.30	29.23	62	49	49	55	52½	53½	53½	53½	53½	calm	sw.	.....	
19.	29.476	29.382	29.05	29.17	29.18	29.11	29.05	29.11	29.11	67	42	55	56	53	53	45	45	45	sw.	sw.	.....	
20.	29.856	29.708	29.25	29.47	29.45	29.48	29.25	29.38	29.48	57	39	46	54	42½	45	45	45	45	sw.	sw.	.....	
21.	30.036	29.748	29.30	29.52	29.78	29.68	29.30	29.52	29.68	58	32	50	51	42½	45	49	49	49	sw.	sw.	.....	
22.	30.155	29.932	29.60	29.67	29.37	29.46	29.60	29.46	29.46	59	44	46	54	41½	49	51	49	51	sw.	sw.	.....	
23.	29.699	29.452	29.15	29.34	29.34	29.18	29.15	29.14	29.18	60	38	57	53½	47	45	44	45	44	sw.	w.	.....	
24.	29.728	29.700	29.24	29.38	29.48	29.24	29.24	29.16	29.24	53	35	45	50½	38	45	44	45	44	sw.	sw.	.....	
25.	30.283	29.939	29.50	29.77	30.06	29.73	29.50	29.73	30.10	53	26	44	52	37	45	41	45	41	w.	sw.	.....	
26.	30.366	30.343	29.93	30.10	30.00	29.82	29.93	30.04	29.82	56	34	40	53	36	46	50	46	50	sw.	sw.	.....	
27.	30.341	30.331	29.88	29.90	30.11	29.99	29.88	29.73	29.99	57	47	49	55½	51½	52½	50	52½	50	calm	calm	.....	
28.	30.315	30.273	29.85	30.19	30.10	29.98	29.85	30.10	29.98	57	43	51	53	41	50	51	50	51	w.	w.	.....	
29.	30.207	30.167	29.73	29.97	30.04	29.84	29.73	29.89	29.84	62	38	53.5	55	40½	49½	46	49½	46	calm	calm	.....	
30.	30.282	30.106	29.77	30.04	29.70	29.78	29.77	29.84	29.78	58	48	46	53	42	47½	45	47½	45	sw.	sw.	.....	
31.	30.239	30.085	29.57	29.88	29.99	30.08	29.57	30.03	30.08	61	51	56	54½	48	45	44	45	44	sw.	sw.	.....	
Mean.	29.960	29.857	29.44	29.760	29.753	29.812	29.44	29.812	29.797	60.58	43.71	51.0	54.7	45.0	48.91	47.45	48.91	47.45	1.76	2.52	5.09	
Max.	30.239	30.085	29.57	29.88	29.99	30.08	29.57	30.03	30.08	61	51	56	54½	48	45	44	45	44	.....	.....	.....	
Min.	29.637	29.452	29.11	29.27	29.40	29.62	29.11	29.64	29.62	64	37	57	53½	48	48	51	48	51	.....	.....	.....	
Oct.	1847.	Oct.																	.....	.....	.....	



## INDEX TO VOL. XXXI.

- ACETAL**, on the preparation and composition of, [77](#).
- Acids**:—hippuric, [127](#); prussic, [146](#); arsenious, [151](#); cinnamic, [153](#); nitrocinnamic, [154](#); margaric, [167](#); oxalic, [233](#); metacetic, [266](#); valerianic, [348](#), [538](#); pectic and metapectic, [390](#); nitric, [454](#); cuminic, [459](#); carminic, [478](#); nitrococcusic, [486](#); chloric, [510](#); osmic, [534](#).
- Adams (Mr.)** on an important error in Bouvard's tables of Saturn, [143](#); on the elements of Neptune, [380](#).
- Adie (R.)** on some experiments with galvanic couples immersed in pure and in oxygenated water, [350](#).
- Airy (Mr.)** on inequalities in the motion of the moon, [384](#).
- Albumen**, action of induced electric currents on, [249](#).
- Alcohol**, action of chlorine on, [77](#).
- Aldebaran**, projection of, on the moon, [233](#).
- Algebraic equation of the fifth degree**, on the, [341](#).
- Algebraical surfaces**, on asymptotic straight lines, planes, cones and cylinders to, [425](#).
- Alizarine**, [46](#).
- Anderson (Dr. T.)** on certain products of decomposition of the fixed oils in contact with sulphur, [161](#).
- Annular eclipse of Oct. 9, 1847**, observation on the, [228](#).
- Antimony**, on the salts of, [230](#).
- Apparatus, chemical**, on some improved forms of, [156](#), [393](#).
- Arsenates**, on a new test for, [258](#).
- Arsenious acid**, on two varieties of, [151](#).
- Astringent substances**, on the means of testing the comparative value of, [150](#).
- Astronomy**, on a new notation for expressing various conditions and equations in, [134](#).
- Atmosphere**, on the polarization of the, [444](#).
- Aurora borealis of Oct. 24, 1847**, observations on the, [369](#).
- Balsam of Tolu**, on some products derived from the, [153](#).
- Bancalari (M.)** on the magnetism of flame, [421](#).
- Barreswil (M.)** on the dehydration of monohydrated sulphuric acid, [314](#).
- Bath**, analysis of the water of the thermal spring of, [56](#).
- Bile of the sheep**, on the composition of the, [366](#).
- Binney (E. W.)** on fossil calamites found standing in an erect position in the carboniferous strata near Wigan, Lancashire, [259](#).
- Birt (W. R.)** on a new kite-apparatus for meteorological observations or other purposes, [121](#).
- Bismuth**, analysis of the needle ore of, [541](#).
- Blowpipe**, improvements in the construction of the hydro-oxygen, [356](#).
- Books**, new, notices respecting, [67](#), [219](#).
- Braconnot (M.)** on the urine of the calf and the sheep, [49](#).
- Brewster (Sir D.)** on the modification of the doubly refracting and physical structure of topaz, by elastic forces emanating from minute cavities, [101](#); on the polarization of the atmosphere, [444](#); on the existence of crystals in the cavities of minerals, [497](#).
- Bronwin (Rev. B.)** on the inverse calculus of definite integrals, [12](#); on the algebraic equation of the fifth degree, [341](#).
- Brown (J.)** on the molybdate of lead, [253](#).
- Buchner (M.)** on the presence of arsenic, copper and tin, in the mineral waters of Bavaria, [392](#).
- Bussy (M.)** on two varieties of arsenious acid, [151](#).
- Caffeine and its compounds**, on the composition of, [115](#).
- Calamites**, fossil, observations on, [259](#).
- Callan (Rev. N. J.)** on a new voltaic battery, and on a cheap substitute for the nitric acid of Grove's platina battery, [81](#).
- Cambridge Philosophical Society**, proceedings of the, [130](#), [301](#), [376](#).
- Carminic acid**, researches on, [478](#).
- Catalysis**, observations on, [96](#), [192](#).
- Chapman (E. J.)** on the constitution of the needle ore of bismuth, [541](#).
- Chloric acid and the chlorates**, observations on, [510](#).
- Clays**, on the composition of, employed in pottery, [435](#).
- Coathupe (M.)** on the preparation of gun-cotton, [152](#).
- Cochineal**, researches on, [471](#), [481](#).
- Colouring matters**, on the action of a



- mixture of red prussiate of potash and caustic alkali upon, [126](#).
- Comet of 1264 and 1556, on the expected reappearance of the, [50](#).
- Commutators, remarks on, [241](#).
- Connell (A.) on the precipitate produced in spring and river waters by acetate of lead, [122](#); on the sulphato-chloride of copper, [537](#).
- Continuity, on the principle of, [137](#).
- Copernicus, on the opinion of, with respect to the light of the planets, [528](#).
- Copper, on the sulphato-chloride of, [536](#).
- Cotton, detection of, in linen, [157](#).
- Couper (R. A.) on the chemical composition of the substances employed in pottery, [435](#).
- Creatine, observations on, [236](#).
- Cuminate of ammonia, on the products of the decomposition of, [459](#).
- Daubeny (Prof.) on active and extinct volcanoes, [399](#).
- De la Rive (A.) on the voltaic arc, [321](#).
- De la Rue (W.) on a modification of the apparatus of Varrentrapp and Will for the estimation of nitrogen, [156](#); on cochineal (*Coccus cacti*), [471](#).
- De Morgan (Prof.) on the structure of the syllogism, and on the application of the theory of probabilities to questions of argument and authority, [130](#); on the opinion of Copernicus with respect to the light of the planets, [528](#).
- Domeyko (M.) on vanadate of lead and copper, [319](#).
- Doveri (M.) on some properties of silica, [315](#).
- Drach (S. M.) on eliminating the signs in star-reductions, [251](#).
- Dumas (M.) on the action of anhydrous phosphoric acid on ammoniacal salts, [544](#).
- Durocher (M.) on the extraction of silver, [317](#).
- Earth, on the amount of radiation of heat from the surface of the, at night, [69](#); on the determination of the mean density of the, [73](#).
- Ebelmen (M.) on the artificial production of minerals, and especially of precious stones, [311](#); analysis of kupfernicksel, [314](#); analysis of gray copper from Algeria, [313](#).
- Elastic medium, on the symbolical equation of vibratory motion of an, [376](#).
- Electric telegraph, on the determination of differences of longitude by means of the, [338](#).
- Electro-magnetic influence on flame and gases, [401](#), [421](#).
- Equations, on the solution of linear differential, [372](#); monogenous, observations on, [467](#).
- Euler's theorem, notice in reference to the extension of, [123](#).
- Faraday (Prof.) on the diamagnetic conditions of flame and gases, [401](#).
- Field (F.) on the products of the decomposition of cuminate of ammonia by heat, [459](#).
- Figuier (M.) on the preparation and composition of lignine, [397](#).
- Flame, on the diamagnetic conditions of, [401](#), [421](#).
- Flax, on the chemical composition of the ashes of, [36](#), [105](#).
- Fluid motion, on some cases of, [136](#).
- Fluxions, on the invention of, [35](#).
- Forster's (T. J. M.) memoir on meteors of various sorts, notice of, [219](#).
- Frankland (E.) on the chemical constitution of metacetic acid, and some other bodies related to it, [266](#).
- Fremy (M.) on the gelatinous substances of vegetables, [389](#).
- Fritzsche (J.) on the preparation and properties of osmiamic acid and some osmiamates, [534](#).
- Galloway (R.) on the water of the thermal spring of Bath (King's bath), [56](#).
- Galloway (T.) on the proper motion of the solar system, [74](#).
- Galvanic couples, account of some experiments with, [350](#).
- Gases, on the re-absorption of mixed, in a voltameter, [72](#); on the diamagnetic conditions of, [401](#), [421](#).
- Geometry, on a new notation for expressing various conditions and equations in, [134](#); contributions towards a system of symbolical, [139](#).
- Gladstone (J. H.) on the chemical history of gun-cotton and xyloidine, [519](#).
- Glaisher (J.) on the amount of the radiation of heat, at night, from the earth, and from various bodies placed on or near the surface of the earth, [69](#); on the Aurora Borealis, as it was seen on Sunday evening, Oct. [24](#), 1847, [369](#).
- Gregory (Dr. W.) on the preparation of hippuric acid, [127](#).
- Griffith (Dr. J. W.) on the composition of the bile of the sheep, [366](#).
- Grove (W. R.) on certain phenomena of voltaic ignition and the decomposition of water into its constituent gases by heat, [20](#), [91](#), [96](#); correlation of physical forces, noticed, [67](#).
- Gruner (M.) on bisilicate of iron or ferruginous pyroxene, [78](#).
- Gun-cotton, history of the discovery of,



- 7; on the preparation and composition of, [152](#), [519](#).
- Hall (Dr. M.) on the effects of certain physical and chemical agents on the nervous system, [72](#).
- Hamilton (Sir W. R.) on quaternions; or on a new system of imaginaries in algebra, [214](#), [278](#), [511](#).
- Hansen (M.) on inequalities in the motion of the moon, [382](#).
- Hare (Dr. R.) on the fusion of iridium and rhodium, [147](#); on certain improvements in the construction and supply of the hydro-oxygen blowpipe, [356](#).
- Hargreave (C. J.) on the solution of linear differential equations, [372](#).
- Hearn (G. W.) on the cause of the discrepancies observed by Mr. Baily with the Cavendish apparatus for determining the mean density of the earth, [73](#).
- Heat, on the amount of radiation of, from the earth's surface, [69](#); on the mechanical equivalent of, [173](#).
- Hebe, notice respecting the planet, [158](#).
- Heintz (M.) on creatine, [236](#).
- Higginbottom (J.) on the number of species and the mode of development of the British Triton, [74](#).
- Hind (J. R.) on the expected reappearance of the celebrated comet of 1264 and 1556, [50](#); observations of Hind's second comet in full sunshine, [145](#); on the planet Hebe, [158](#); on the new planet Iris, [237](#).
- Hippuric acid, on the preparation of, [127](#).
- How (H.) on the analysis of the ashes of the orange-tree, [271](#).
- Hutchinson (J.) on the function of the intercostal muscles, and on the respiratory movements, with some remarks on muscular power, in man, [222](#).
- Induction, memoir on, [241](#).
- Ink, invisible, on a new, [176](#).
- Integrals, on the inverse calculus of definite, [12](#).
- Iridium, on the fusion of, [147](#), [365](#).
- Iris, notice respecting the new planet, [237](#).
- Jacobi (Prof. M. H.) on the reabsorption of the mixed gases in the voltameter, [72](#).
- Jones (C. H.) on the structure and development of the liver, [224](#).
- Joule (J. P.) on the theoretical velocity of sound, [114](#); on the mechanical equivalent of heat, as determined by the heat evolved by the friction of fluids, [173](#).
- Kane (Sir R.) on the composition and characters of certain soils and waters belonging to the flax districts of Belgium, [36](#), [105](#).
- Kindt (G. C.) on the detection of cotton in linen, [157](#).
- König (P.), inventor of the printing-machine, [297](#).
- Kolbe (Dr. H.) on the chemical constitution of metacetic acid, and some other bodies related to it, [266](#); on the decomposition of valerianic acid, by means of the voltaic current [348](#).
- Kopp (M. E.) on balsam of Tolu, and some products derived from it, [153](#); on the action of hydrochloric acid in the formation of oxalic acid, [233](#).
- Ledoyen's disinfecting fluid, remarks on, [233](#).
- Lefroy (Capt. J. H.) on a great magnetic disturbance on the 24th of September 1847, [346](#).
- Liebig (Prof.) on a new test for prussic acid, and on a simple method of preparing the sulphocyanide of ammonium, [146](#).
- Lignine, on the preparation and composition of, [397](#).
- Linen, on the detection of cotton in, [157](#).
- Liver, on the structure and development of the, [224](#).
- Longitude, on the determination of differences of, by the electric telegraph, [338](#).
- Loomis (Prof.) on the determination of differences of longitude by means of the electric telegraph, [338](#).
- Lubbock (Sir J.) on the perturbations of planets moving in eccentric and inclined orbits, [1](#), [86](#); on the heat of vapours, [90](#); on the development of the disturbing function R, [144](#).
- Madder, on the colouring matters of, [46](#).
- Magnetic declination at St. Helena, on the diurnal variation of the, [70](#).
- Magnetic disturbance, on a great, [346](#).
- Magnetism, influence of, on the voltaic arc, [328](#).
- Malaguti (M.) on the extraction of silver, [317](#).
- Mannite, action of nitric acid on, [316](#).
- Margaric acid, observations on, [167](#).
- Mechanics, on a new notation for expressing various conditions and equations in, [134](#); contributions towards a system of symbolical, [139](#).
- Mercer (J.) on the action of a mixture of red prussiate of potash and caustic alkali upon colouring matters, [126](#).
- Merck (G.) on the water of the thermal spring of Bath, [56](#).
- Meridian instruments, on the properties of rock as a foundation of the piers of, [531](#).
- Metacetic acid, on the constitution of, [266](#).
- Metapectic acid, [389](#).



- Meteor of September [25](#), 1846, notice respecting the, [368](#).
- Meteors, observations on, [219](#).
- Meteorological observations, [79](#), [159](#), [239](#), [319](#), [399](#), [545](#); on a new kite-apparatus for, [191](#).
- Meteorology, suggestions for promoting the science of, [238](#).
- Methylene, on the hydrocyanate of, [544](#).
- Miller (Prof. W. [H.](#)) on the measurement of the double sulphates of zinc and soda, and of magnesia and soda, [540](#).
- Mineral waters, analyses of, [56](#), [124](#); on the presence of arsenic, copper and tin in some, [392](#).
- Minerals:—ferruginous pyroxene, [78](#); molybdate of lead, [253](#); gray copper from Algeria, [313](#); kupfernickel, [314](#); vanadate of lead and copper, [319](#); sulphatochloride of copper, [537](#); native carbonate of nickel, [541](#); needle ore of bismuth, *ib.*
- Minerals, on the artificial production of, [311](#); on the existence of crystals in the cavities of, [497](#).
- Molybdate of lead, analysis of, [253](#).
- Moon, on inequalities in the motion of the, [382](#).
- Muscles, on the function of the intercostal, [222](#).
- Neptune, on the elements of, [380](#).
- Nervous system, on the effects of certain physical and chemical reagents on the, [72](#).
- Nicholson (E. C.) on the composition of caffeine and some of its compounds, [115](#).
- Nickel, on the native carbonate of, [541](#).
- Nitric acid, theoretical views on the nature of, [7](#); on the hydrates of, [454](#).
- Nitrococcusic acid, on the preparation and composition of, [486](#).
- Nitrogen, on some modifications of the apparatus for determining, [156](#), [393](#).
- Numbers, on certain properties of prime, [70](#); account of a discovery in the theory of, [189](#); on an equation in, [293](#); on the partitions of, [301](#).
- O'Brien (Rev. M.) on a new notation for expressing various conditions and equations in geometry, mechanics and astronomy, [134](#); on a system of symbolical geometry and mechanics, [139](#); on the symbolical equation of vibratory motion of an elastic medium, whether crystallized or uncrystallized, [376](#).
- Odmyle, on the sulphuret of, [170](#).
- Oils, fixed, on certain products of the decomposition of, in contact with sulphur, [161](#).
- Optical instruments, on the formation and application of fine metallic wires to, [531](#).
- Orange-tree, analysis of the ashes of the, [271](#).
- Osmiamic acid and osmiamates, on the preparation and properties of, [534](#).
- Oxalic acid, formation of, [233](#).
- Ozone, on a new test for, [176](#).
- Papyrine, on the preparation and composition of, [398](#).
- Pectic acid, [389](#).
- Pelilot (M. E.) on the preparation and composition of the salts of antimony, [230](#).
- Phosphoric acid, anhydrous, action of, on ammoniacal salts, [544](#).
- Pierre (M. [I.](#)) on chlorosulphuret of silicon, [78](#); on the equivalent of titanium, [155](#); on the solubility of chloride of silver in hydrochloric acid, [398](#).
- Planets, on the perturbations of, [1](#), [86](#); on the opinion of Copernicus with respect to the light of the, [528](#).
- Platinum, on the fusion of large masses of, [356](#).
- Playfair (L.) on transformations produced by catalytic bodies, [193](#).
- Pollock (Sir F.) on certain properties of prime numbers, [70](#).
- Pottery, on the chemical composition of the substances employed in, [435](#).
- Poumarede (M.) on the preparation and properties of lignine, [397](#).
- Printing-machine, invention and first introduction of, by Koenig, [297](#).
- Prussic acid, on a new test for, [146](#).
- Pyroxene, ferruginous, analysis of, [78](#).
- Pyroxyline, contributions to the chemical history of, [7](#), [152](#), [519](#).
- Quaternions, on, [214](#), [278](#), [511](#).
- Reviews:—Grove's Correlation of Physical Forces, [67](#); Forster on Meteors, [219](#).
- Rhodium, on the fusion of, [147](#), [365](#).
- Richardson (T.) on the ashes of rough brown sugar and molasses, [336](#).
- Ronalds (Mr.) on a new kite-apparatus for meteorological observations or other purposes, [191](#).
- Roth (M.) on the preparation of the protoxide of tin, [392](#).
- Rowney (T. H.) on the ashes of the orange-tree, [271](#).
- Royal Society, proceedings of the, [69](#), [222](#), [372](#).
- Royal Astronomical Society, proceedings of the, [143](#), [380](#), [528](#).
- Sabine (Lieut.-Col. E.) on the diurnal variation of the magnetic declination of St. Helena, [70](#).
- Salt, culinary, on the solubility of, in alcohol, [393](#).
- Salts, ammoniacal, action of anhydrous phosphoric acid on, [544](#).

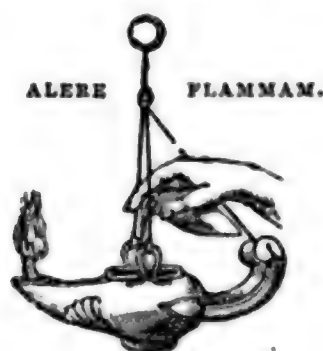


- Saturn, on an important error in Bouvard's tables of, [143](#).
- Schœnbein (Prof.) on the discovery of gun-cotton, [7](#); on a new test for ozone, [176](#).
- Schunck (Dr.) on the colouring matters of madder, [46](#).
- Silica, observations on, [315](#).
- Silicium, on the chlorosulphuret of, [78](#).
- Silver, on the extraction of, [317](#); solubility of the chloride of, in muriatic acid, [398](#).
- Slatter (Rev. J.) on the meteor of September [25](#), 1846, [368](#).
- Smith (Mr. A.) on the hydrates of nitric acid, [454](#).
- Smyth (Prof. C. P.) on the properties of rock as a foundation of the piers of meridian instruments, and on the detection of a cause of error in the Edinburgh transit, [531](#).
- Sobrero (M.) on nitric mannite, [316](#).
- Soils and waters of the flax districts of Belgium, on the composition and characters of, [36](#), [105](#).
- Solar system, on the proper motion of the, [74](#).
- Sound, on the theoretical velocity of, [114](#).
- Spinelle, on the artificial production of, [312](#).
- Star-reductions, on eliminating the signs in, [251](#).
- Stas (M.) on the action of chlorine on alcohol—formation of acetal, [77](#).
- Stokes (G. G.) on some cases of fluid motion, [136](#); on the theory of oscillatory waves, [138](#).
- Storms, observations relating to the laws of, [338](#).
- Struve (H.) on the preparation and properties of osmiamic acid and some osmiamates, [534](#).
- Sugar, analyses of the ashes of rough brown, [336](#).
- Sulphocyanide of ammonium, simple method of preparing, [146](#).
- Sulphuric acid, on the dehydration of, [314](#).
- Sulphates, on the measurement of some double, [540](#).
- Syllogism, on the structure of the, [130](#).
- Sylvester (J. J.) on a discovery in the theory of numbers relative to the equation  $Ax^3 + By^3 + Cz^3 = Dxyz$ , [189](#), [293](#); on the general solution (in certain cases) of the equation  $x^3 + y^3 + Az^3 = Mxyz$ , &c., [467](#).
- Taylor (R.) on the invention and first introduction of Mr. Kœnig's printing-machine, [297](#).
- Taylor (T.) on some improved forms of chemical apparatus, [393](#).
- Therault (M.) on the formation of valeric acid, [538](#).
- Thompson (L.) on chloric acid and the chlorates, [510](#).
- Thomson (Dr. R. D.) on a test for arseniates, [258](#).
- Tin, on the preparation of the protoxide of, [392](#).
- Titanium, on the equivalent of, [155](#).
- Tolene, composition of, [153](#).
- Topaz, on the modification of the doubly refracting and physical structure of, [101](#); on the crystals in the cavities of the, [504](#).
- Triton, on the number of British species and mode of development of, [74](#).
- Tyrosine, on the properties and composition of, [496](#).
- Ulrich, Mr., on the formation and application of fine metallic wires to optical instruments, [531](#).
- Urine of the calf and the sheep, comparative analysis of the, [49](#).
- Valerianic acid, on the decomposition of, by the voltaic current, [348](#); on the formation of, [538](#).
- Vapours, on the heat of, [90](#).
- Veall (S.) on a means for promoting the science of meteorology, [238](#).
- Vegetables, on the gelatinous substances of, [389](#).
- Voltaic arc, researches on the, [321](#).
- battery, description of a new, [81](#).
- current, on the decomposition of valerianic acid by the, [348](#).
- ignition, on certain phænomena of, [20](#), [91](#).
- Voltameter, on the reabsorption of mixed gases in a, [72](#).
- Wagner (M.) on the solubility of common salt in alcohol, [393](#).
- Walter (Mr. John), false statements in the Times newspaper and Mechanics' Magazine concerning him as regards Kœnig's printing-machine, [297](#).
- Warburton (H.) on the partitions of numbers, on combinations, and on permutations, [301](#).
- Warington (R.) on the means of testing the comparative value of astringent substances for the purposes of tanning, [150](#).
- Wartmann (Prof. E.) on induction, [241](#).
- Water, decomposition of, by heat, [20](#), [91](#); on the decomposition of, by platinum, [177](#).
- Waves, oscillatory, on the theory of, [138](#).
- Weddle (T.) on asymptotic straight lines,

- planes, cones and cylinders to algebraical surfaces, [425](#).
- Weld (C. R.) on the invention of fluxions, [35](#).
- Wilson (Dr. G.) on the decomposition of water by platinum and the black oxide of iron at a white heat, [177](#).
- Young (Prof. J. R.) on the extension of Euler's theorem, [123](#); on the principle of continuity in reference to certain results of analysis, [137](#).
- Xyloidine, contributions to the chemical history of, [519](#).
- Zantedeschi (Prof.) on the motions presented by flame when under the electro-magnetic influence, [421](#).

END OF THE THIRTY-FIRST VOLUME.

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